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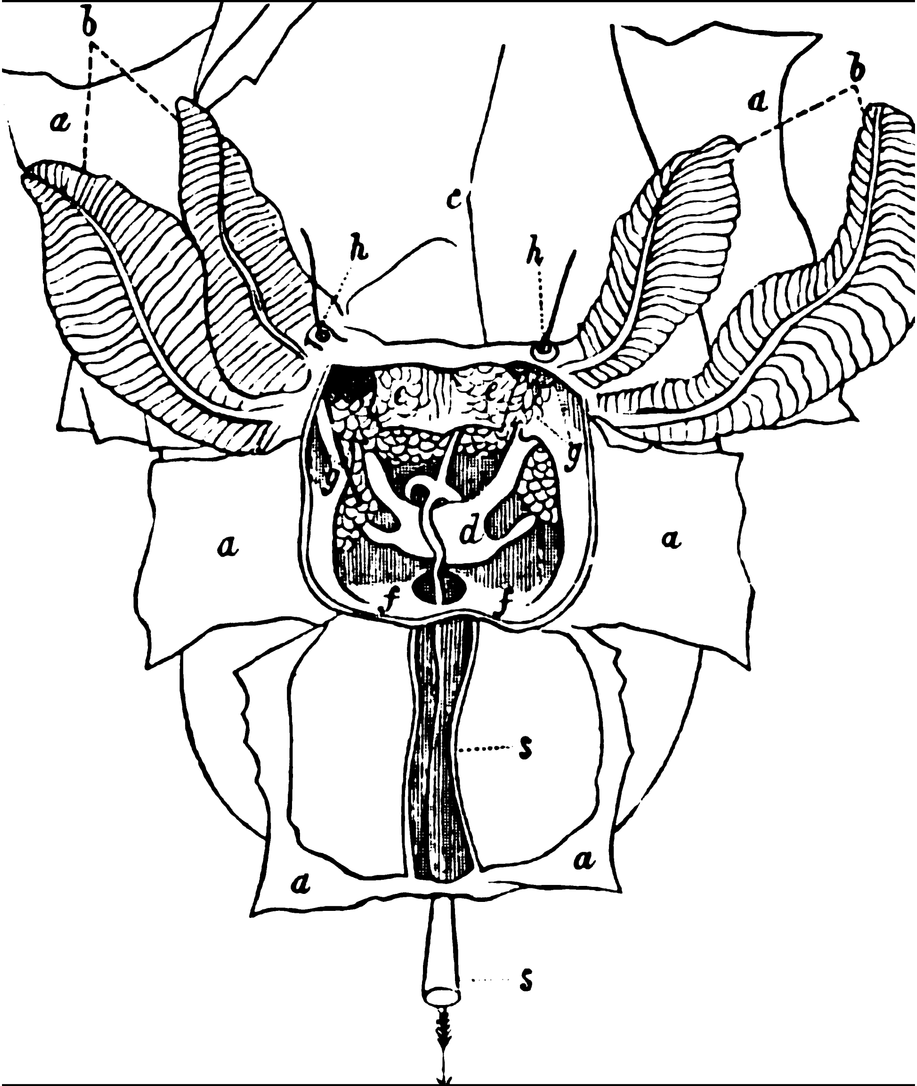

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Monit. Polit.* lib. i. cap. 1.

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III. and IV. Illustrative of Dr. Schleiden's Memoir on the Development of the Organization in Phænogamous Plants, and of Mr. F. Watkins's Paper on Electro-magnetic Motive Machines.

ERRATA.

Page 12, equations (7) *for* n in both places *read* $n_{,,}$.

Page 13, line 21, *for* $(1 - \sin(n_{,}t - kx))$ *read* $(1 - \sin^2(n_{,}t - kx)$.

Page 46, line 5, *for* latter *read* former.

— — — 23 *for* $\frac{n}{a_{,}^2}$ *read* $\frac{n^2}{a_{,}^2}$.

Page 28, 29, *for* Walgon *read* Watson.

We have been kindly furnished by Dr. Schleiden with the following errata which occur in the translation of his paper.

Page 175, line 6, *for* their points, *read* the extreme point of the axis

Ibid. in the note, line 6, the *plumula* in this family becomes *enveloped* by means of an elevation of the cotyledon *perfectly closed*

Page 177, line 4, *after* disproportionately large,—what in the expanded flower has been called *paleæ*.

Ibid. line 3 note, the hyphen to be inserted after 3,—two 3-leaved.

Page 182, line 18, *instead of* again deviate, &c. *read* require an explanation of structure different from that which is commonly given, must be included, &c.

Page 183, line 2, *instead of* these, *read* this.

Page 186, 9 lines from bottom, *for* almost entirely compressed, *read* entirely supplanted

Ibid. 7 lines from bottom, *after* thin *insert* inner.

Page 409 line 39 *for* C *read* $\overset{,}{\mathrm{C}}$

40 *for* A *read* $\overset{,}{\mathrm{A}}$

Page 410 line 5 *for* $\underline{\dot{\mathrm{N}}\mathrm{a},}$ Na Cl, $\overset{,}{\mathrm{N}}\underline{\mathrm{a\,N}}$ *read* $\dot{\mathrm{N}}\mathrm{a},\ \underline{\mathrm{N}}\mathrm{a}\ \underline{\mathrm{Cl}},\ \underline{\overset{,}{\mathrm{N}}\mathrm{a}}$

Page 463 line 10 *for* sodium *read* iodine.

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Fig. I.

Fossil

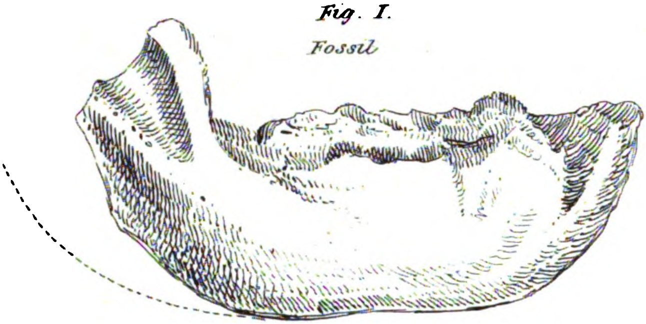
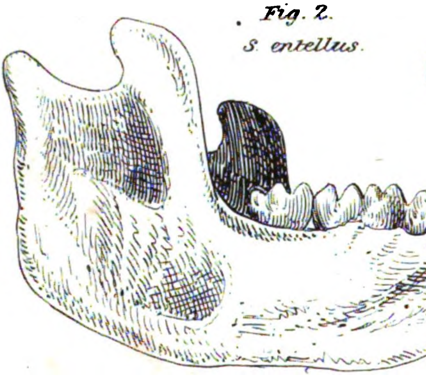


Fig. 2.

S. entellus.



Fossil. Fig. 4.

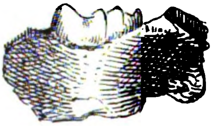
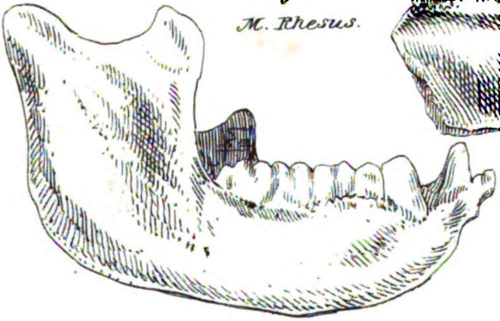


Fig. 3.

M. Rhesus.



Fossil. Fig. 5.

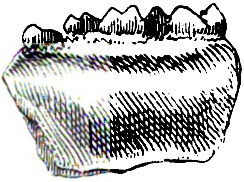


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Fig. 1.

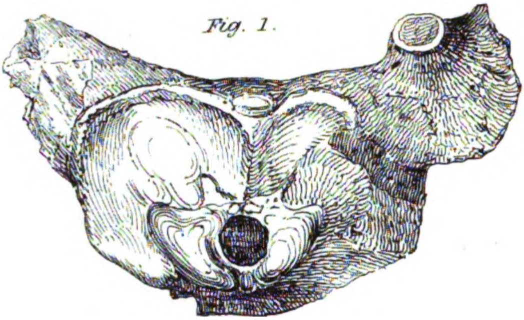

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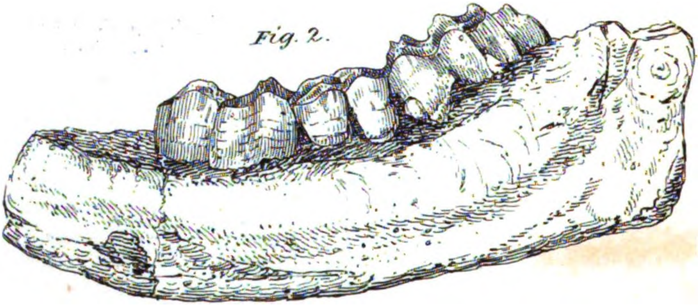
Fig. 2.



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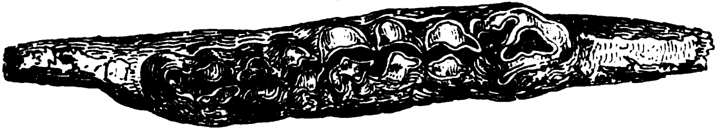

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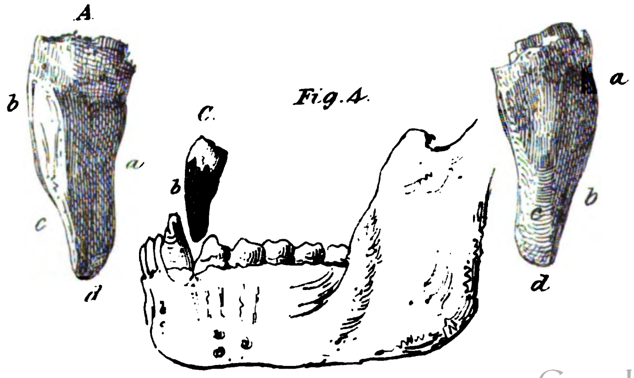
Fig. 4.



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THE
LONDON AND EDINBURGH
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[THIRD SERIES.]

JANUARY 1838.

I. *Researches on the Maximum Density of Liquids.* *By* C. Despretz.*

THIS work, says the author, will be divided into two parts. The first memoir will contain a series of experiments on the determination of the temperature of the maximum density of pure water, and on the dilatation of that fluid from this maximum to its boiling point, and on the other hand to − 13° Cent. The second will consist of the results of the experiments on the maximum density of sea-water, and on the general course of the phænomenon in aqueous, saline, acid, alkaline, and alcoholic solutions of different degrees of concentration.

Extract from the First Memoir.

From 1832 and 1833, I have been occupied in the investigation of these subjects, and have laid some of the results before the Academy. Since that time I have repeated and modified the experiments, so that I believe I have now arrived at results more certain and general. I proved that all saline solutions have, like pure water, a maximum density. I at first only wished to know whether sea-water did or did not possess a maximum. It is known that Marcet of Geneva and Ermann of Berlin, the only two physicists who have of later date devoted themselves to experimental researches on this subject, did not find a maximum; we shall see the reason of this in the second memoir. The solution of this problem offered much interest to physicists on account of the phænomena of the temperature of the polar and equinoxial seas. I was led to make experiments on pure water, a question which is also very important from its connection with the determination of the Gramme, because I noticed that those who had occupied themselves with this intricate subject had every one of them left it in considerable uncertainty. Hällström, to whom we are indebted for the latest work on this subject, found by each process a different number, he therefore assigns as limits 4°·85 and 3°·4. The number upon which he fixed after a detailed discussion of the known results is 4°·1 ± 0°·3*. We see the uncertainty in which this memoir leaves us. I shall not speak of Rudberg, who gave in to the Academy of Stockholm the same number as that which I made known to the Academy at Paris rather more than a year before.

---

* Translated by Mr. W. Francis, from an Extract by MM. Gay-Lussac, Arago, and Becquerel, given in the *Comptes Rendus de l'Academie des Sciences*, 1837; vol. iv. pp. 124, 435.

Four methods have hitherto been employed in this kind of research; apparently the most simple consists in weighing a body in water of different temperatures. The necessity of agitating the fluid in order to distribute the heat uniformly renders this method of difficult execution, because this agitation necessarily sets the scales in motion. Lefebvre-Gineau, Hällström, and others have employed this.

In the second the same vessel is weighed full of water at temperatures near to the maximum. Blagden and Gilpin made use of this method. I also have tried it. I have even subjected it to numerous trials; but it is not sensitive enough. This objection may also be applied to the first method.

We should naturally think that refraction would furnish a very delicate mode of determining this point, but we know from the experiments of Arago, that water, while it is dilated by cold, reflects light more and more powerfully; this fact, which is not less singular than that of the maximum itself, excludes the use of refraction from our researches. We might also determine the temperature of the maximum by the help of the relation discovered by Savart between the temperature and diameter of sheets of water (*nappes*), but this process would require great practical dexterity in experiments on the efflux of liquids.

The process which appeared to me to be the most suitable, was to compare the progress of a water thermometer with a mercurial thermometer. For this purpose I constructed six water and four mercury thermometers. All these instruments were divided into parts of equal volume. In order to get rid of the error which arises from the conical form of the tubes,

---

* M. Despretz is evidently unacquainted with the last memoir which appeared in Poggendorff's *Annalen*, vol. xxxiv. p. 220.—(W. F.)

I disposed them so that the variation in the size of the diameter took the one direction and the other alternately. In the first experiments the instruments were placed in the midst of a liquid which was gradually made cold; and after it had passed the apparent maximum, the apparatus was left to the calorific influence of the surrounding bodies; it then became warm, and arrived at the point of departure. By performing the experiment so that the heating kept equal pace with the refrigeration, the error occasioned by the want of coincidence between the water thermometer and the mercury thermometer was avoided; the first being always behind the second. I also lessened very much this cause of error by taking the mean of the results obtained; nevertheless I preferred observing in the statical condition.

After various essays, which need not be enumerated, I adopted the following apparatus.

It consists of a cylindrical copper vessel, similar to a large eprouvette In this vessel two water thermometers and three mercury thermometers are suspended, the two first alternating with the latter; all the reservoirs are at the same height; the vessel is corked so as to hinder the access of external air. It is then placed in a large earthen vessel filled with a mixture at various temperatures, from + 16° Cent. to the freezing of the water, which takes place at times at −5°, at times at −10°, sometimes at −15°, and even at −20° Cent. We should remember that Gay-Lussac had previously observed water to remain in a liquid state at −12° Cent.

Each experiment lasted from eight to ten hours, during which from eight to ten numbers were taken.

The curve of the apparent dilatation is drawn, and then a tangent parallel to the line of the dilatation of the glass is drawn to it; for the maximum is that point where the absolute dilatation of the water is zero, *i. e.* where the apparent dilatation observed is equal to the effect produced by the contraction of the glass. The maximum might also have been fixed by the method of calculation which Biot has followed in his *Traité de Physique*, but we preferred the graphic method, which perhaps indicates better the course of the results.

The determination of the absolute maximum requires the knowledge of the dilatation of the glass. As the composition of this substance varies more or less, we had to find the dilatation of those tubes of which our thermometers were formed; I found it to be:

Between 28° and 100° Centig. = 0·0000258
—— 0 28 —— = 0·0000255
—— 0 100 —— = 0·0000257,

which latter number only differs by unity in the third figure from that obtained by Dulong and Petit. The dilatation of glass increases therefore from 0° to 100°. But the increase assigned by Hällström is evidently too great, not only since this increase differs widely from the one we have found, but because it is in opposition to the general course of dilatation established by the two physicists above-mentioned.

The process just described has also the advantage of being the only one applicable to water at low temperatures, and to solutions whose maximum is below their freezing point in a state of agitation, which is the case in all solutions somewhat concentrated. The maximum may also be determined by a process independent of the dilatation of the glass. This process is founded on the fact that in a liquid mass the strata of which are at unequal temperatures, those molecules having the temperature of the maximum sink while the others rise. I have considerably modified this process, previously employed by Hope, Tralles, Rumford and Hällström*. The latter thought, on account of the discordance of his results, that this method ought to be rejected. In fact it is, as it was employed by them, more adapted to prove the existence of a *maximum*, than to determine its temperature with accuracy. The following is a short description of the process employed by me:

We took a porcelain vessel capable of holding above thirteen pints;—a larger vessel would have required too much time, for in five hours this did not become more than a few degrees colder in the air;—the temperature of the fluid was several degrees below zero. A smaller vessel, an eprouvette for instance, would have become cold too rapidly, and the consequence would be that the thermometers would indicate a temperature too much at variance with the real temperature of the liquid.

Four thermometers, the stems of which were passed through the side of the vessel, were placed horizontally in a vertical plane, two on the one side and two on the other. The distance between the first thermometer and the bottom of the vessel amounted to 54 millimetres, this being also the distance between each two thermometers. The entire height of the vessel was 270 millimetres and the diameter 160. The thermometers alternated, so that No. 1 and No. 3 came out on one side, No. 2 and No. 4 on the opposite one. The middle of the reservoir of each thermometer was in the axis of the vessel.

The vessel was suspended by three cords of equal length, with its axis in a vertical position. As soon as it was filled with water at a lower or higher temperature than the surrounding air, according to the experiment, it was closed with a porcelain cover; after waiting a few moments the temperature of the thermometer was noted down from minute to minute. A curve of the temperatures was then drawn, the time being taken as abscissæ, the temperature as ordinates.

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* Poggendorff's *Annalen*, vol. ix. p. 530.—(W. F.)

It is known that, under its maximum, the water below is warmer than that above, and *vice versâ* when above the maximum. We might thence have concluded that curves of the temperatures would bisect one another at one point, which would be the temperature of the maximum; this however was not the case. Near the 4th degree the curves cut one another at many points. I obtained the maximum in the following manner: I took,

1. The mean of all the temperatures where the curves suddenly change their direction.
2. The mean of the temperatures corresponding to the points of intersection.
3. The mean of the points where the curve drawn with the mean temperatures intersected the other four points.
4. The mean of these three results.

We see that the method of Tralles, thus modified, ought to conduct to a result more certain than those hitherto obtained.

The mean of the two heating experiments is 4°·058. But the thermometers being graduated in a vertical position, and observed here in a horizontal one, a small correction is necessary on account of the pressure of the mercury; as was also a second, proceeding from the action of the air on the stem of the thermometers: these two corrections, the influence of which had been estimated by previous experiments, reduce this mean to 3°·969.

The two cooling experiments gave 3°·995 for the corrected mean. The common mean is 3°·982. The difference, 0°·026, is in the direction in which it ought to be; for in a state of motion, *i. e.* whilst being warmed or cooled, the temperature of a liquid is not indicated exactly by a thermometer. If the fluid is cooling the thermometer indicates too much, if becoming warm too little. The retardation in question will be greater in proportion to the rapidity of motion of the heat. Moreover, the less the partial results from which the true result is drawn differ among each other, the greater will be the probability of the exactitude of a series of experiments. This condition then seems to be fulfilled by our experiments, since the difference between the greatest and smallest result amounts only to 0°·026.

Hällström indeed obtained by the process of Tralles a smaller number in heating than in cooling, but as the difference amounted to 1°·3, he thought this method offered little exactness.

If instead of taking the mean of the higher temperatures at 4°, and that of the lower temperatures, we had taken the mean of all the temperatures relative to one curve, we should have obtained 3°·988 instead of 3°·982: difference 0°·006.

The results obtained with the water thermometers, that is to say, by the first method, were as follows:

Seven experiments with one tube			3°·99 C.
Seven	——	another	4· 02
Two	——	a third	4· 01
Two	——	a fourth	3· 96.

The mean from these eighteen experiments is 4° C., which agrees within two hundredths with the result of the former process.

Before and after each experiment the zero of the thermometer was examined. This verification is absolutely necessary, because the zeros of thermometers, even of those which have been constructed for a long time, differ when these instruments are kept for some time at a low or high temperature. We shall have occasion to return to this important fact on another occasion.

So many contradictory results have been obtained on the subject of the maximum of density of pure water, that it is quite unnecessary to mention here in what these experiments may be regarded as more nearly approaching to the truth. They occupied me for a year. I constructed, I graduated all the instruments myself. The weighings were performed with the greatest care. Fearful of partial errors, all the results were represented by drawings on a very large scale. I lay before the Academy a few only of the numbers and curves. Although we cannot answer for the hundredth part of a degree, considering the extreme mobility of glass instruments, we yet remark that the difference of the single results with 4°, a difference which in general has amounted to some hundredths, never surpassed 0°·1, and that the two processes, which have not the least relation with one another, have furnished sensibly the same result. Nevertheless, on account of the importance of the subject, I shall have the honour in a short time of laying before the Academy some experiments made by a process not yet described, and employed at very low temperatures.

This memoir closes with a table of the dilatation of water from degree to degree, from the maximum to the boiling point, and from the maximum to −13° Cent. The dilatation is a little stronger below than above the maximum.

This dilatation amounts to $\frac{43}{1000}$ from 4° to 100°.

Various points of the scale have been verified by fixed temperatures, as that of æther, alcohol, &c. The curve of dilatation is almost a parabola for a very considerable space on the scale.

Extract from the Second Memoir.

The question respecting the maximum of density of saline solutions, immediately connected with the researches relative to the temperature of the sea at various depths, has been for a long time agitated among natural philosophers, who, however, are far from agreeing with each other on this subject; thus, as Ermann remarks, while Rumford, Marcet, and Berzelius think that salt water has no maximum, Gay-Lussac, Scoresby, and Sabine, guided by analogy, profess quite a contrary opinion*.

The importance of the question had induced several physicists to attempt its solution by direct experiments, the only way of bringing out the truth in such cases.

Marcet (in 1819) read before the Royal Society of London a memoir, in which he related some experiments by means of which he had proved that sea-water contracts down to the freezing point. He only says that the fluid *appeared* to expand below 5°·6 Cent.

Ermann, the son of the learned Secretary to the Academy of Berlin, undertook in 1817, at the suggestion of Humboldt, some observations for the same purpose. Four different methods proved to this able physicist that there did not exist a maximum for sea-water between 8° and −3°. Science was already in possession of a memoir by Blagden, in which that learned Englishman contended that the maximum sunk like the freezing point, remaining at a distance equal to that of pure water. It is impossible to account for the manner in which Blagden was led to this conclusion, which is in opposition to all experiments made on this subject, none of which gave to sea-water a maximum above the freezing point.

Of the four methods described in my paper on the maximum of pure water, says M. Despretz, one only is applicable to aqueous solutions. It is that in which the course of a water thermometer is compared with that of a mercury thermometer. In the experiments with saline solutions, as in those with pure water, four thermometers filled with saline solutions and four with mercury were immersed in a large vessel, the temperature of which was gradually lowered to six or seven points, which I sought to render fixed. In order to avoid the influence of the warming or cooling of the vessel, thermometers containing mercury and saline solutions were taken alternately. A curve was traced with the apparent contractions and expansions, to which was drawn a tangent parallel to the line of expansion of the glass. The tangential point gave the temperature of the maximum, *i. e.* the point where the expansion is equal to the contraction of the glass, which is evidently the point where the absolute dilatation of this solution is zero. This is the transition of the contraction into the expansion by cold.

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* Captain Sabine's remarks on this subject will be found in Phil. Mag. First Series, vol. lxiii. p. 70.—Edit.

M. Despretz did not find a single aqueous solution which did not show a maximum either above or below the freezing point. The solutions which contain 1 to 3 centimetres of foreign matter are in the first predicament; those containing more, in the latter.

Every one can demonstrate the existence of a maximum for any aqueous solution whatever; for this purpose it is only necessary to construct a thermometer with the solution, and to lower the temperature rather slowly: the liquid is seen to contract down to a certain point, and then by a continued refrigeration regularly to expand.

These experiments being very long and laborious, after having proved the existence of a maximum for any aqueous solution, the author contented himself with extending these researches to eleven different substances: sea water, chloride of sodium, chloride of calcium, carbonate of potash, carbonate of soda, sulphate of potash, sulphate of soda, sulphate of copper, and alcohol.

With the exception of sea-water, every substance was dissolved in pure water in seven different proportions. These ten substances therefore give seventy solutions. The nature of the substances was varied, in order to follow the general course of the phænomenon. Among them were deliquescents, efflorescents, bodies which are not affected by the air; some very soluble, others of little solubility.

We will begin with mentioning the results which relate to sea-water. I first operated, says M. Despretz, with an artificially formed sea-water according to Marcet's analysis; but M. Arago, to whom I mentioned my first experiments, had the kindness to offer me some sea-water collected by M. Freycinet in the Southern Ocean. This water weighed at 20° 1°·0273. The mean from twelve experiments gave —2°·55 for the temperature of the freezing point in a state of agitation; at the instant of freezing the thermometer returned to —1°·84 of density. This fluid has its maximum density at —3°·67. This is the mean deduced from five experiments with three different tubes. One tube gave twice −3°·69; a second −3°·60 and −3°·59; a third −3°·77. We now see the reason why Marcet and Ermann did not discover any maximum density in sea-water, because they searched for it above the freezing point, while it is situated at more than one degree below.

The solution of the question relative to sea-water sufficed for the purposes of physical geography; but the history of corpuscular properties required a more general solution. It was necessary to extend these experiments to a certain number of aqueous solutions in order to discover the course which the maximum takes as the addition of foreign matter lowers it.

For this purpose I dissolved several quantities of foreign matter in the proportions 1, 2, 4, 6, 12, and 24. Each of the substances was employed in a pure state, which it is now very easy to ensure. The chloride of sodium, the chloride of calcium, the carbonate of potash and that of soda were melted. The carbonate of potash was obtained by calcining pure and crystallized bicarbonate. The sulphate of copper was employed crystallized. Water not being an essential part of this salt, it was subtracted; while the pure hydrate of potash, concentrated sulphuric acid and absolute alcohol, (water being in certain respects essential to their composition, since heat alone does not expel it), were considered as anhydrous bodies. We will mention some of the results obtained:

Sea Salt.

0·000123	of salt...	freezing point[*]	−1°·21,	Max.	+1°·19	Cent.
0·0246	—	—	−2·24	—	−1·69	—
0·0371	—	—	−2·77	—	−4·75	—
0·0741	—	—	−5·10	—	−16·00	—

Chloride of Calcium.

0·0371	of salt...	freezing point	−3°·92,	Max.	−2°·43	Cent.
0·0741	—	—	−5°·28	—	10·43	—

This sinking of the maximum, says the author, cannot be the consequence of a partial freezing of the liquid mass, since the curve, representing the expansions above and below the maximum, is quite regular, as the drawings which I now lay before the Academy will show; the freezing of the smallest part would determine, in the curve, points which by geometricians are denominated *singular*. Besides, this partial freezing could scarcely take place without causing the freezing of almost the entire mass. The coincidence also which exists between the experiments performed on the same solution, but with different tubes, excludes all idea of freezing. Thus, for the solution of sea salt at 0°·0371, one tube gave −4°·80, −4°·73, −4°·76, the mean of which is −4°·76. A second tube gave −4°·73, −4°·72, −4°·77, the mean of which is −4°·74; which differs from the first only by two hundredths.

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[*] The temperatures are those marked by the thermometer at the moment when the liquid is on the point of freezing. The temperatures indicating the actual freezing, i. e. which are for the solutions what zero is for pure water, are not so low.

We conceive that there does not always exist the same agreement in the partial experiments; many however exhibit but a small difference.

In comparing the various experiments, we see that it is neither the more soluble salts, nor the salts which most retard the freezing point, that lower the maximum most; for instance, the chloride of calcium lowers the maximum much less than sea salt; the sulphate of potash less than the sulphate of soda. This result is obtained whatever may be the degree of concentration of the solutions compared.

The two following results, says Despretz, appear to me to be proved:

1. Sea water, and all aqueous solutions, acid, alcoholic, saline and alkaline, have a maximum of density.

2. This maximum sinks much quicker than the freezing point, the variation of which, as well as that of the density, is nearly proportional to the quantity of matter added to the water.

The point of the maximum remains at first above that of the freezing point; it then reaches it, and finally sinks below it. Even with seven hundredths of salt, acid, or alkali, the maximum may be at 12 degrees below the freezing point, so that it is impossible to discover it except by exposing the fluid solution in narrow tubes to temperatures far below that point.

II. *Continuation of Researches in the Undulatory Theory of Light;—On the Cause of Elliptical Polarization.* By JOHN TOVEY, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN continuation of my Researches in the Undulatory Theory of Light, I submit to you the following investigation, showing mechanically the cause of elliptical polarization. In integrating the equations of this theory it has hitherto been assumed that the molecules of the undulating medium are, in their state of equilibrium, so arranged in pairs as to make the sums composed of odd powers of differences vanish. When this is *not* the case the phænomenon of elliptical polarization is the result.

We have found (see L. and E. Phil. Mag., vol. ix. p. 421.) that when the displacements ξ are neglected,

$$\left.\begin{aligned} \frac{d^2\eta}{dt^2} &= m\,\Sigma\left\{\phi(r)+\psi(r)(\Delta y\,\Delta\eta+\Delta z\,\Delta\zeta)\,\Delta y\right\}, \\ \frac{d^2\zeta}{dt^2} &= m\,\Sigma\left\{\phi(r)+\psi(r)(\Delta y\,\Delta\eta+\Delta z\,\Delta\zeta)\,\Delta z\right\}. \end{aligned}\right\} \quad (1.)$$

Assume

$$\left.\begin{aligned} \eta &= a\sin(nt-kx), \\ \zeta &= \rho\,a\sin(nt-kx-b); \end{aligned}\right\} \quad (2.)$$

then

$$\begin{aligned} \Delta\eta &= a\sin(nt-kx-k\,\Delta x)-a\sin(nt-kx) \\ &= a(\cos k\,\Delta x-1)\sin(nt-kx)-a\sin k\,\Delta x\cos(nt-kx): \end{aligned}$$

also,

$$\begin{aligned} \Delta\eta &= a\sin(nt-kx-b-k\,\Delta x+b)-a\sin(nt-kx-b+b) \\ &= a\,\{\cos(k\,\Delta x-b)-\cos b\}\sin(nt-kx-b) \\ &\quad -a\,\{\sin(k\,\Delta x-b)+\sin b\}\cos(nt-kx-b). \end{aligned}$$

In like manner we find,

$$\begin{aligned} \Delta\zeta &= \rho\,a\sin(nt-kx-b-k\,\Delta x)-\rho\,a\sin(nt-kx-b) \\ &= \rho\,a(\cos k\,\Delta x-1)\sin(nt-kx-b)-\rho\,a\sin k\,\Delta x\cos \\ &\qquad (nt-kx-b): \end{aligned}$$

also,

$$\begin{aligned} \Delta\zeta &= \rho\,a\,\{\cos(k\,\Delta x+b)-\cos b\}\sin(nt-kx) \\ &\quad -\rho\,a\,\{\sin(k\,\Delta x+b)-\sin b\}\cos(nt-kx). \end{aligned}$$

Now put

$$\left.\begin{aligned} m\,\Sigma.\{\phi(r)+\psi(r)\,\Delta y^2\}(\cos k\,\Delta x-1) &= s, \\ m\,\Sigma.\{\phi(r)+\psi(r)\,\Delta z^2\}(\cos k\,\Delta x-1) &= s', \\ m\,\Sigma.\{\phi(r)+\psi(r)\,\Delta y^2\}\sin k\,\Delta x &= s_{,}, \\ m\,\Sigma.\{\phi(r)+\psi(r)\,\Delta z^2\}\sin k\,\Delta x &= s'_{,}, \\ m\,\Sigma.\psi(r)\,\Delta y\,\Delta z\,\{\cos(k\,\Delta x-b)-\cos b\} &= s_2, \\ m\,\Sigma.\psi(r)\,\Delta y\,\Delta z\,\{\cos(k\,\Delta x+b)-\cos b\} &= s'_2, \\ m\,\Sigma.\psi(r)\,\Delta y\,\Delta z\,\{\sin(k\,\Delta x-b)+\sin b\} &= s_3, \\ m\,\Sigma.\psi(r)\,\Delta y\,\Delta z\,\{\sin(k\,\Delta x+b)-\sin b\} &= s'_3; \end{aligned}\right\} \quad (3.)$$

and substitute, in the equations (1.), the values of η, ζ, $\Delta\eta$, $\Delta\zeta$;

taking, for the first equation, the values of $\Delta\eta$ and $\Delta\zeta$ which involve the arc $(nt-kx)$, and for the second equation, the values involving $(nt-kx-b)$. We shall then have

$$(n^2+s+\rho s'_2)\sin(nt-kx)-(s_{\prime}+\rho s'_3)\cos(nt-kx)=0,$$
$$(\rho(n^2+s')+s_2)\sin(nt-kx-b)-(\rho s'_{\prime}+s_3)\cos(nt-kx-b)=0;$$

and since these equations are true for all values of t and x, they resolve themselves into

$$\left.\begin{aligned} n^2+s+\rho s'_2&=0,\\ \rho(n^2+s')+s_2&=0,\\ s_{\prime}+\rho s'_3&=0,\\ \rho s'_{\prime}+s_3&=0.\end{aligned}\right\}\qquad(4.)$$

These equations may be satisfied by means of the four arbitrary quantities n, ρ, k, b; the last two being contained in them implicitly.

From the first and second of these equations we derive

$$\rho=-\frac{n^2+s}{s'_2}=-\frac{s_2}{n^2+s'},$$

$$n^2=-\frac{s+s'}{2}\pm\sqrt{\left(\frac{(s-s')^2}{4}+s_2s'_2\right)}.$$

Let $$\sqrt{\left(\frac{(s-s')^2}{4}+s_2s'_2\right)}=\frac{s-s'}{2}+\epsilon;$$

then the two values of n^2 will be

$$n_{\prime}^2=-s-\epsilon,\qquad n_{\prime\prime}^2=-s'+\epsilon;\qquad(5.)$$

and those of ρ

$$\rho_{\prime}=\frac{\epsilon}{s'_2},\qquad \rho_{\prime\prime}=-\frac{s_2}{\epsilon}.\qquad(6.)$$

These values being substituted in the assumed expressions for η and ζ, we have

$$\left.\begin{aligned}\eta&=a_{\prime}\sin(n_{\prime}t-kx)+a_{\prime\prime}\sin(nt-kx),\\ \zeta&=\rho_{\prime}a_{\prime}\sin(n_{\prime}t-kx-b)+\rho_{\prime\prime}a_{\prime\prime}(nt-kx-b).\end{aligned}\right\}\qquad(7.)$$

It has been seen (at p. 501 of your 8th vol.) that any function of x and t may be expressed by a series of which each term has the form $(p\sin kx+q\cos kx)$; where p and q are functions of t, and k a constant quantity. For the same reason any function of t may be expressed by a series, each term having the form $A\sin nt+B\cos nt$, where t is the only variable. Therefore, for $(p\sin kx+q\cos kx)$ we may write $(A\sin nt+B\cos nt)\sin kx+(A\sin nt+B\cos nt)\cos kx$, which, by the rules of trigonometry, may be reduced to the form $$a\sin(nt-kx-b)+a'\sin(nt+kx-b');$$

and this, when the waves move only in the direction of x positive, becomes $a \sin (n t - k x - b)$.

Now the equations (1.) being of the first degree, may be satisfied not only by the second members of the equations (7.), but by the sum of any number of functions of the same form. It follows, therefore, from the observations just made, and from the circumstance that the origin of x is arbitrary, that the complete integrals of the equations (1.) may be expressed by

$$\begin{aligned} \eta &= \Sigma \, \{a_{,} \sin (n_{,} t - k x) + a_{,,} \sin (n_{,,} t - k x)\}, \\ \zeta &= \Sigma \, \{\rho_{,} a_{,} \sin (n_{,} t - k x - b) + \rho_{,,} a_{,,} \sin (n_{,,} t - k x - b)\}: \end{aligned} \quad (8.)$$

provided that the waves travel in the direction of x positive, and that the displacements η and ζ are functions of x and t.

Suppose the arbitrary coefficients in the equations (8.) to be all zero except $a_{,}$, then

$$\begin{aligned} \eta &= a_{,} \sin (n_{,} t - k x), \\ \zeta &= \rho_{,} a_{,} \sin (n_{,} t - k x - b) \quad (9.) \\ &= \rho_{,} a_{,} \{\sin (n_{,} t - k x) \cos b - \sin b \cos (n_{,} t - k x)\}; \end{aligned}$$

therefore

$$\begin{aligned} (\zeta - \rho_{,} a_{,} \sin (n_{,} t - k x) \cos b)^2 &= \rho_{,}^2 a_{,}^2 \sin^2 b \cos^2 (n_{,} t - k x) \\ &= \varrho_{,}^2 a_{,}^2 \sin^2 b \, (1 - \sin (n_{,} t - k x)): \end{aligned}$$

hence we have

$$(\zeta - \rho_{,} \cos b \, . \, \eta)^2 - \rho_{,}^2 a_{,}^2 \sin^2 b \, . \left(1 - \frac{\eta}{a_{,}^2}\right) = 0,$$

an equation to an ellipse of which η and ζ are the coordinates. Consequently, when the system is in a state of motion which can be expressed by the equations (9.), every molecule describes an ellipse round its place of rest; and the equations (8.) show that the general motion of the system is equivalent to a number of coexisting motions of the same kind.

In a future paper I purpose to apply these formulæ to the case of elliptical polarization produced by quartz crystal.

I am, Gentlemen, yours, &c.

Littlemoor, Clitheroe, Nov. 24, 1837. JOHN TOVEY.

P.S. To justify our neglect of the displacements ξ, it may be well here to observe, in addition to what has been remarked (vol. ix. p. 421.) that when x is taken, as we have supposed, perpendicular to the wave-surface, neither the displacements η, ζ, nor their differences $\Delta \eta$, $\Delta \zeta$, cause any change of density in the medium; but the differences $\Delta \xi$ imply a change of density. If then we suppose the force by which the æther resists compression to be so great that in the motions producing light it may be regarded as incompressible, the differences $\Delta\xi$ vanish, and the last two of our general equations (3.) of vol. viii., p. 9, become the same as the equations (1.) of this paper.

The reason assigned by Mr. Kelland (vol. ix. p. 341.) why the displacements ξ are insensible, or, in other words, why there is no vibration in the direction of transmission, cannot, I conceive, be the true one; because it implies a state of unstable equilibrium. (See Pratt's Mechanical Philosophy, art. 508.) I believe the ingenious Fresnel considered the true reason to be the one which we have just supposed.

III. *Observations upon the Œconomy of several Species of* Hymenoptera *found in a Garden at Clapton. By* A. KENNEDY, *Esq.* *

HAVING had leisure this spring and summer to devote more time than usual to my favourite pursuit, entomology, I have paid considerable attention to the œconomy of various insects inhabiting a summer-house in my father's grounds, made of the stumps of trees, and having a thatched roof, which has afforded me much amusement from the number of different species of *Hymenoptera* which nidificate either in the posts or thatch.

The following is a list of the insects I have observed, and I shall make a few remarks upon the œconomy of each species, some of the facts relating to which I believe have not before been recorded.

Fam. DIPLOLEPIDÆ.

1. Cratomus megacephalus, *F.*

Fam. SAPYGIDÆ.

2. Sapyga 4-guttata, *F.*

Fam. CRABRONIDÆ.

3. Trypoxylon figulus, *Lat.*
4. ——— clavicerum, *St. F.*
5. Crabro spinipectus, *Shuck.*?
6. Stigmus troglodytes, *Van L.*
7. Diodontus insignis, *Van L.*
8. ——— gracilis, *Curt.*
9. ——— corniger, *Shuck.*
10. Pemphredon lugubris, *F.*
11. Pemphredon morio, *Van L.*
12. ——— unicolor, *Lat.*
13. Psen atratum, *Pz.*

Fam. VESPIDÆ.

14. Odynerus quadratus, *Don.*
15. ——— bidens, *L.*

Fam. ANDRENIDÆ.

16. Hylæus signatus, *Pz.*

Fam. APIDÆ.

17. Chelostoma florisomnis, *L.*
18. Osmia bicornis, *L.*
19. ———spinulosa, *K.*
20. Heriades campanularum, *K.*

1. *Cratomus megacephalus,* Fab.

I have observed four or five specimens of this insect settling upon the posts of the summer-house, but I have not ascertained any thing of its œconomy.

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* Communicated by the Author.

2. *Sapyga 4-guttata**, Fab.

The œconomy of this insect, I believe, is not known. I have taken two or three specimens flying about the posts. The male is much rarer than the female.

3. *Trypoxylon figulus*†, Lat.

This insect entombs spiders for the supply of its young ones. I have often watched it carrying the spiders into holes in the posts, and also into straws in the thatch. On splitting open one of the latter I found a number of cells filled with spiders and separated from one another by partitions of clay. Between each cell, there was a space left of about a quarter of an inch, so that there were two partitions between each cell, and between the last cell and the outside. There was one egg in each cell attached to the abdomen of a spider near the bottom of the cell. The *Trypoxylon* sometimes buries very large spiders compared with its own size, so that it can hardly jam them into its hole.

I was one day much amused with a male, who when the female was absent often came, entered, and remained at the entrance with his antennæ just projecting as if he was keeping watch to keep out parasitical insects; and once when I placed my hand over the hole so as to prevent the female entering, after repeated attempts she flew away, and returned with the male as if to ask his advice respecting the obstruction to her nest.

The number of spiders in the cells of course differs according to their size, there sometimes being only two if very large, and sometimes as many as twelve or more if small. The *Trypoxylon* does not appear to be partial to any particular species. The female makes a buzzing noise when she is constructing the clay partitions. I believe the œconomy of this insect has never been distinctly ascertained before.

4. *Trypoxylon clavicerum*‡, St. F.

The habits of this insect are similar to those of *T. figulus*, only burying very small spiders, and not leaving any space between the cells. I believe that Mr. Shuckard and another gentleman are the only persons who have taken it besides myself. Its œconomy has not been noticed before.

5. *Crabro spinipectus*, Shuck.?

The male of this insect is common about the posts of the summer-house, but I have not been able to discover the female.

* Curt. Brit. Ent., p. 532.

† *Ibid.* p. 652.

‡ *Ibid.*

6. *Stigmus troglodytes*, Van L.

I have taken four or five females of this insect and one male. On the 22nd of July I saw a female enter a straw with its prey in its mouth, and on splitting the straw open I found a great many minute insects, which appeared to be the larvæ of a *Thrips*. I should think there must have been at least fifty in one cell. There were two cells, separated from one another by partitions which appeared to be made of the scrapings of the inside of the straw cemented together. I also noticed a female with its prey enter a hole in one of the posts. I do not think the œconomy of this insect has ever been noticed before.

7. *Diodontus insignis*, Van L.

The males of *D. insignis* were common about the summer-house from the beginning of July to the end of the same month, but I have not been able to find one female.

8. *Diodontus gracilis* *, Curt.

The female supplies its young with aphides, which I have noticed it take from the leaves of the ivy. It makes its cells in the straws of the thatch, and separates them by partitions made apparently of the same materials as those of *D. corniger*. I have not taken any males.

9. *Diodontus corniger*, Shuck.

The male of this insect I first took on the 3rd of July, and the female on the 8th. The females were tolerably common towards the end of the month, but I have only taken six or seven males. The female provides aphides for the food of its young, and it appears to take them from the holes of other insects. I have often watched it entering holes in the posts and returning with aphides to its own hole. It carried them one by one in its mouth; and what was very curious, in going from its own to the other hole it ran straight along the post, but in returning with an aphis, although the holes were not half a foot apart, it flew off some distance before it conveyed its prey home. No other insect appeared to inhabit the holes from which it took the aphides. The partitions between each cell are made of a sticky transparent substance laid over with small fibres of wood. I have watched the female closing the orifice with the same material. After she had completely closed it with the propolis she went into another hole, and returned with small fibres of wood, which she plastered over, and this when dry became hard and strong. The habits of this insect have not been noticed by any one else, and I believe Mr. Shuckard is the only person who has taken it before myself.

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* Curt. Brit. Ent., pl. 496.

10. *Pemphredon lugubris*, Fab.

I have watched this insect burrow into the wood and throw out the saw-dust. It appears to prefer decayed wood for making its cells, in which it deposits aphides.

11. *Pemphredon Morio*, Van L.

I am not quite certain whether I took this insect at the summer-house or not, but I think I did.

12. *Pemphredon unicolor*[*], Lat.

The œconomy of *P. unicolor* is, I believe, well known. I have taken it carrying an aphis, but have not examined its cells.

13. *Psen atratum*[†], Pz.

This insect has been exceedingly numerous this year, using the straws in the thatch to deposit its prey in, in some of which I have counted as many as a hundred aphide. The partitions appear to be made of the scrapings of the inside of the straw cemented together. The egg is white and semitransparent, and is attached to the abdomen of an aphis near the bottom of the cell. The males first appeared the beginning of July, flying about the thatch and the neighbouring shrubs in thousands. They disappeared about the end of the month. The females did not become numerous until the 10th.

14. *Odynerus quadratus*[‡], Don.

This insect entombs small green caterpillars having sixteen feet; six pectoral, eight abdominal, and two anal. On cutting open a post where I saw the female enter with its prey, I found a tunnel of about four inches in length running parallel with the sides of the post, and divided into three or four cells by partitions of clay. In each of these cells were about ten caterpillars closely packed, and a long white egg attached to the side of the cell near the bottom.

I first noticed this insect the beginning of June, and it was abundant during the whole of that month.

15. *Odynerus bidens*, Linn.

I observed a female of *Odynerus bidens* burrowing into a post the beginning of July, and a day or two afterwards I captured her while conveying her prey, which appeared to be the larva of a *Chrysomela*. On opening the post the end of July I found a tunnel two inches in depth, divided into three cells by partitions of clay. In the first cell the *Odynerus* was in the pupa state, and in the two lower ones in that of larva. Each of the cells contained the remains of larvæ, and in one of them was a small dipterous insect quite perfect.

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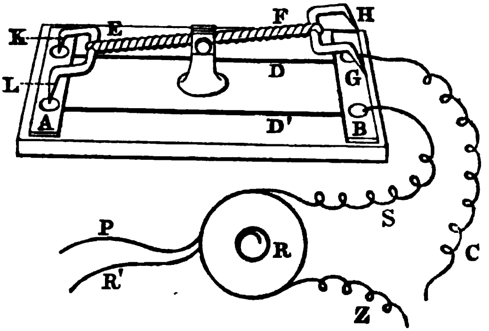
[*] Curt. Brit. Ent., pl. 632. [†] *Ibid.*, fol. 25. [‡] *Ibid.*, fol. 137.



piece of hard wood, A and B, each having two holes excavated in it for the purpose of holding mercury: each of these holes communicates by means of thick copper wires, D D', with that opposed to it in the other piece of wood at the opposite end of the board. Midway between these receptacles for mercury is a wooden support, so contrived that a piece of soft iron wire $\frac{1}{8}$ inch in diameter and 5 inches in length may oscillate between the cheeks cut in its upper part, with as little friction as possible, the iron wire being supported by milled-headed screws. Around this iron wire E F, are wound two helices of thin insulated copper wire, in the same direction, (from right to left,) in such a manner that the two ends of one helix may terminate in the copper points G H, and the ends of the other helix in the points K L. Two small horse-shoe magnets (not shown in the figure) are then fixed on proper supports, so that they may each be placed near an end of the iron bar E F in a vertical plane just posterior to it, so that on depressing the end F of the bar it may be opposite one pole (say the south) of one magnet, and consequently the end E will be opposite the other pole of the second magnet. On elevating the end F, the contrary will of course take place, and for this purpose it is hardly necessary to say that the similar poles of the magnets should be in the same direction.

From this description it is evident, that on connecting the cups of mercury in A or B with the two plates of a single voltaic battery, the bar E F will become a temporary magnet if the ends of either helix are allowed to dip in the mercury; and if connection with the battery is properly made, the ends or poles of the temporary magnet will be repelled by the poles of the permanent magnet to which they are opposed; the bar will consequently move, and so cause the immersion of the ends of the second helix in the other cups of mercury, repulsion will again occur, and so on: about 300 oscillations of the iron bar can be thus obtained in a minute. On connecting the ends of the thick helix of the coil before described by the battery of a single pair by means of this apparatus, a series of induced currents may be obtained from the extremities of the longer helix capable not only of communicating a series of intense shocks, but of exerting powerful electrolytic action. This connection is best made in the manner shown in the accompanying figure, in which R represents a section of the reel, S one end of the short helix connected with a cup of mercury in the piece of wood B, Z the other end of the short helix connected with one plate of the battery, whilst the wire C connects the other cup of mercury in B with the other plate of the voltaic couple. It is scarcely necessary to state that the intensity of the induced current may be increased by inserting in the hollow axis of the reel a bar of very soft iron, or what will be still better, a bundle of soft iron wire, which becoming magnetic, will considerably increase the dynamic power of the coil. In this case, indeed, the sparks produced when the ends of the helices round the iron bar E F leave the mercury are very brilliant, accompanied by a loud snapping noise and a vivid combustion of the mercury, clouds of the oxide of that metal being copiously evolved.

If the ends P R of the long and thin coil are furnished with platinum points and immersed in water acidulated with sulphuric acid, rapid electrolytic action ensues, torrents of minute bubbles of oxygen and hydrogen being evolved. If instead of water the points are pressed upon paper moistened with iodide of potassium, electrolytic action ensues*, iodine and oxide of potassium being separated. Solutions of sulphate of potass and soda, chloride of potassium, sodium, antimony and copper are also rapidly decomposed. In these experiments it will be found that the great majority of the electropositive elements (for example) appear constantly at one termination of the coil, *cæteris paribus*, but not all, for it must not be forgotten that on making as well as breaking contact with the electromotor an induced current takes place in the long coil, although of far weaker intensity than the latter, to which it is opposed in direction, and consequently in electrolytic effects.

If the ends P R of the long coil are furnished with copper cylinders for handles and then grasped even with the *un-*

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* If whilst the oscillating bar of the contact-breaker is vibrating rapidly, we fix a piece of well-burnt charcoal on one of the terminations of the fine coil, and draw the other termination lightly over it, a rapid succession of minute but brilliant sparks are obtained. These sparks depend entirely upon the induced current, as the fine coil has no connection with the electromotor. For the exhibition of this, as well as of the electric light of an energetic arrangement, I find pencils of that kind of artificial graphite found lining the interior of the iron cylinders used for the distillation of coal in our gas manufactories very far superior to box-wood, or indeed any other form of charcoal.

moistened hands, the intensity of the rapid succession of shocks will be found absolutely intolerable, even when the battery used consists of but two plates presenting each 6 or 8 square inches of surface.

This magnetic contact-breaker will, I flatter myself, be found eventually of service to the chemist for electrolytic purposes; whilst as affording a ready mode of applying voltaic electricity for medical purposes, I think it will be considered of considerable service as dispensing with manual labour, and affording currents of far greater intensity than can be obtained from several dozen, or even a far greater number of pairs of plates excited by strong acids, a process equally inconvenient and expensive.

In conclusion, I ought to observe that the application of a permanent magnet to effect the rupture of contact without manual labour is by no means original with me, although in justice to myself I must state, that when I first contrived the above-described instrument I was not aware of a similar principle having been adopted for this purpose. In the last number of Prof. Silliman's Journal is a paper by Dr. Page describing several pieces of apparatus to be used with Dr. Henry's gigantic coils; one of these contrivances, ill-described however, consists of a bar of iron covered with a helix oscillating *between* the poles of a *single* permanent magnet, constituting, from my own experience, a very ineffective arrangement. To Dr. Page, however, must in justice be accorded the originality of the application of permanent magnets for the purpose of breaking contact.

I remain, my dear Sir, yours truly,

22, Wilmington Square, Nov. 2, 1837. GOLDING BIRD.

To Richard Phillips, Esq., F.R.S. L. & E., &c.

N.B. The contact-breaker described in this letter was constructed for me by Mr. Neeves, of Great St. Andrew's Street, Holborn.

V. *On a singular Development of Polarizing Structure in the Crystalline Lens after Death; and on the Cause, the Prevention, and the Cure of Cataract.* By Sir DAVID BREWSTER, K.G.H., V.P.R.S. Ed.*

IN examining the changes which are produced by age in the polarizing structure of the crystalline lenses of animals, I was induced to compare these changes with those which I conceived might take place, after death, when the lens was allowed to indurate in the air, or was preserved in a fluid medium. After many fruitless experiments I found that distilled water was the only fluid which did not affect the transparency of the capsule, and my observations were therefore made with lenses immersed in that fluid. The general polarizing structure of the crystalline in the *sheep*, *horse*, and *cow*, consists of *three* rings, each composed of *four* sectors of polarized light, the two innermost rings being *positive* like *zircon*, and the outermost *negative*, like *calcareous* spar. In other cases, especially when the lenses were taken from older animals, *four* rings were seen, the innermost of which was positive as before, and the rest *negative* and *positive* in succession.

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* From the Report of the Sixth Meeting of the British Association: Transactions of the Sections, pp. 16, 111. See Lond. and Edinb. Phil. Mag., vol. viii. pp. 193, 416.

I now placed a lens which gave three rings, in a glass trough containing distilled water, and I observed the changes which it experienced from day to day. These changes were such as I had not anticipated; but though I have observed and delineated them under various modifications, I shall confine myself at present to the statement of the general result. There is a *black* ring between the two positive structures or luminous rings. After some hours' immersion in distilled water, this black ring becomes *brownish*, and on the second day after the death of the animal, a *faint blue* ring of the first order makes its appearance in the middle of it, and its double refraction, as exhibited by its polarized tint, increases from day to day, till the tint reaches the *white* of the first order. Simultaneously with this change of colour, the breadth of this new ring gradually increases, encroaching slightly upon the inner positive ring, but considerably upon the *second* positive ring; so that the black or neutral ring which separates the two positive structures, and in the middle of which a new luminous ring is created, divides itself into two black neutral rings, the *one* advancing *outwards*, and *diminishing* the breadth as well as the intensity of the second series of positive sectors, and the other advancing inwards, and diminishing the breadth and intensity of the inner or central sectors. While these changes are going on, the *outer* luminous or *negative* ring advances inwards, encroaching also on the *second* positive ring.

Upon examining the character of the new luminous ring, the development of which has produced all these changes, I found it to be *negative*, so that at a certain stage of these variations we have a *positive* and a *negative* doubly refracting structure succeeding each other alternately, from the centre to the circumference of the lens, such as I have often observed in lenses taken from animals of greater age, and examined immediately after death.

After this stage of perfect development, when there is a marked symmetry both in the relative size and polarizing intensities of the four series of sectors, the lens begins to break up. The new *negative* ring encroaches so much on the two *positive* ones, which it separates, that the outer one is sometimes completely extinguished, while the breadth and tint of the inner sectors are greatly diminished, so that the highest double refraction exists in the newly developed ring. In a day or two this ring also experiences a great change of distinctness and intensity, and the lens commonly bursts on the fifth or sixth day, sometimes in the direction of the septa or lines where its fibres have their origin and termination, and sometimes in other directions.

In order to give a general idea of the cause of these singular changes, I may state that the capsule which incloses the lens is a highly elastic membrane—that it absorbs distilled water abundantly—and that, in consequence of this property, the lens gradually increases in bulk, and becomes more globular, till the capsule bursts with the expansive force of the overgrown lens. That the reaction of the elastic capsule contributes to modify the polarizing structure of the interior mass, cannot admit of a doubt, as it is easy to prove that that structure is altered by mechanical pressure; but I cannot conceive how such a reaction could create a new negative structure between two positive ones, and produce the other phænomena which I have described. I have been led therefore to the opinion, that there is in the crystalline lens the germ of the perfect structure, or rather the capability of its being developed by the absorption of the aqueous humour; that this perfect structure is not produced till the animal frame is completely formed; and that when it begins to decay the lens changes its density and its focal length, and sometimes degenerates into that state which is characterized by hard and soft cataract.

The results, of which I have now given an exceedingly brief notice, appear to me to afford a satisfactory explanation of those changes in the lens which terminate in cataract, a disease which seems to be more prevalent than in former times. Accidental circumstances have led me to study the progress of this disease in one peculiar case, in which it was arrested and cured; and I am sanguine in the hope that a rational method of preventing, and even of stopping the progress of this alarming disease, before the laminæ of the lens have been greatly separated or decomposed, may be deduced from the preceding observations.

As the experiments, however, and views upon which this expectation is founded, are more of a physiological than of a physical nature, I am desirous of submitting an account of them to the Medical Section, that they may undergo that strict examination which they could receive only from the experience and science of that distinguished body.

On the Cause, the Prevention, and the Cure of Cataract.

Having submitted to the Physical Section an account of a singular change of structure produced by the action of distilled water upon the crystalline lens after death, Sir D. Brewster was desirous of communicating to the Medical Section some views which this, and previous observations, have led him to entertain respecting the cause and the prevention and cure of cataract.

"The change of structure to which I have referred consists in the development of a negative polarizing band or ring between the two positive rings nearest the centre of the lens; the gradual encroachment of this new structure upon the original polarizing structure of the lens; and the final bursting of the lens after it had swelled to almost a globular form by the absorption of distilled water.

"As the crystalline lens floats in its capsule there can be no doubt that it is nourished by the absorption of the water and albumen of the aqueous humour, and that its healthy condition must depend on the relative proportion of these ingredients. When the water is in excess the lens will grow soft, and may even burst by its over absorption; and when the supply of water is too scanty, the lens will, as it were, dry and indurate, the fibres and laminæ formerly in optical contact will separate, and the light being reflected at their surfaces, the lens will necessarily exhibit that white opacity which constitutes the common cataract.

"This defect in the healthy secretion of the aqueous humour, as well as the disposition of the lens to soften or to indurate by the excess or defect of water, may occur at any period of life, and may arise from the general state of health of the patient; but it is most likely to occur between the ages of 40 and 60, when the lens is known to experience that change in its condition which requires the use of spectacles. At this period the eye requires to be carefully watched, and to be used with great caution; and if any symptoms appear of a separation of the fibres or laminæ, those means should be adopted which, by improving the general health, are most likely to restore the aqueous humour to its usual state. Nothing is more easy than to determine at any time the sound state of the crystalline lens; and by the examination of a small luminous image placed at a distance, and the interposition of minute apertures and minute opake bodies of a spherical form, it is easy to ascertain the exact point of the crystalline where the fibres and laminæ have begun to separate, and to observe from day to day whether the disease is gaining ground or disappearing.

" In so far as I know, cataract in its early stages, when it may be stopped or cured, has never been studied by medical men; and even when it is discovered, and exhibits itself in white opacity, the oculist does not attempt to reunite the separating fibres, but waits with patience till the lens is ready to be couched or extracted.

" Considering cataract, therefore, as a disease which arises from the unhealthy secretion of the aqueous humour, I have no hesitation in saying that it may be resisted in its early stages, and in proof of this I may adduce the case of my own eye, in which the disease had made considerable progress. One evening I happened to fix my eye on a very bright light, and was surprised to see round the flame a series of brightly coloured prismatic images, arranged symmetrically and in reference to the septa to which the fibres of the lens are related. This phænomenon alarmed me greatly, as I had observed the very same images in looking through the lenses of animals partially indurated, and in which the fibres had begun to separate. These images became more distinct from day to day, and lines of white light of an irregular triangular form afterwards made their appearance. By stopping out the bad parts of the lens by interposing a small opake body sufficient to prevent the light from falling upon it, the vision became perfect, and by placing an aperture of the same size in the same position, so as to make the light fall only on the diseased part of the lens, the vision entirely failed.

" Being now quite aware of the nature and locality of the disease though no opacity had taken place so as to appear externally, I paid the greatest attention to diet and regimen, and abstained from reading at night, and all exposure of the eyes to fatigue or strong lights. These precautions did not at first produce any decided change in the optical appearances occasioned by the disease; but in about eight months from its commencement I saw the coloured images and the luminous streaks disappear in a moment, indicating in the most unequivocal manner that the vacant space between the fibres or laminæ had been filled up with a fluid substance transmitted through the capsule from the aqueous humour. These changes took place at that period of life when the eye undergoes that change of condition which requires the use of glasses, and

I have no doubt that the incipient separation of the laminæ would have terminated in confirmed cataract had it not been observed in time, and its progress arrested by the means already mentioned. Since that time the eye, though exposed to the hardest work, has preserved its strength, and is now as serviceable as it had ever been.

"If the cataract had made greater progress, and resisted the simple treatment which was employed, I should not have hesitated to puncture the cornea, in the expectation of changing the condition of the aqueous humour by its evacuation, or even of injecting distilled water or an albuminous solution into the aqueous cavity."

VI. *On Tritiodide of Mercury.* *By Mr.* Robert Hunt[*].

I AM not aware that any one has observed more than two combinations of iodine and mercury, the yellow iodide and the scarlet biniodide; therefore a short account of a third may not be uninteresting.

If with a saturated solution of the iodide of potassium we unite as much iodine as it will dissolve, and then add a sufficient quantity of the bichloride of mercury to separate the iodine of the salt, instead of the scarlet biniode, a purple-brown powder will be precipitated, which will be found to be one proportional of mercury combined with three proportionals of iodine, or

$$\left.\begin{array}{ll}\text{Iodine} \ldots\ldots\ldots\ldots\ldots & 72{\cdot}1\\ \text{Mercury} \ldots\ldots\ldots\ldots & 27{\cdot}9\end{array}\right\}\text{ in 100 parts.}$$

$$\begin{array}{lr}\text{Three equivalents of iodine being} \ldots\ldots & 126 \times 3 = 378\\ \text{One equivalent of mercury} \ldots\ldots\ldots\ldots\ldots & 202\\ \hline & 580\end{array}$$

is its equivalent number.

The tritiodide of mercury is soon resolved by exposure to the air into the biniodide, as it is also by alcohol, which separates one proportional of iodine. Heat likewise drives off a portion of the iodine, and the binary compound results. But if it is exposed in a strong glass tube filled with carbonic acid or the vapour of æther, and hermetically sealed, to the heat of a spirit flame, it sublimes in deep amber-coloured acicular crystals, which are tolerably permanent in the air.

It is soluble in hot chloride of sodium, from which on cooling black fibrous crystals form, which I suspect to be a compound of chloriodic acid and soda; but I have not yet had an opportunity of properly examining this compound.

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[*] Communicated by the Author.

I find the addition of a little hydrochloric acid to the solution of the bichloride of mercury renders the precipitated tritiodide of mercury more permanent in the air.

ROBERT HUNT.

VII. *A simple Mode of exhibiting the Colours of thin Plates.* *By Mr.* JAMES WALGON.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

ALTHOUGH Newton's rings have often been described, I believe they have been but rarely seen. The reason is, because few persons have lenses, clamps, and screws suitable for Newton's experiment. In your Journal for the month of March in the present year, (L. & E. Phil. Mag., vol. x. p. 183,) there is an article by the late Dr. Ritchie, entitled, " A simple mode of exhibiting Newton's rings." Two circular pieces of thin plate glass, separated at the circumference by a single gold-leaf, are to be used instead of lenses; but in order to bring the glass plates to touch in the centre, we must have " a rectangular frame of iron or brass, and a screw." I am afraid, therefore, that Newton's rings, as an experiment for illustrating the colours of thin plates, are not likely to be often visible, except in diagrams.

I shall now proceed to describe a very simple mode of exhibiting the colours of thin plates by means of an experiment which requires no expensive apparatus.

We have all seen the brilliant colours which are reflected from the narrow cracks in mica, and there are little concentric coloured rings in this mineral, which may be found by using a magnifier. It occurred to me lately that possibly these splendid colours might be made to appear in the mineral whenever required. To effect this I obtained a thin plate or film of air, by introducing a lancet into the edge of a *clear* plate of mica, carefully separating the laminæ to the extent of about one inch square. Then holding the plate of mica with both hands, and pressing the middle finger of the right hand just under the spot where the film of air was made, I was much gratified by the appearance of several beautiful curved lines or bands of different colours, which followed the direction in which I moved my finger. These curved lines or bands in their forms very much resemble those of a fortification agate. The perfect rings are small, but they expand or contract according to the degree of pressure. The colours also change with the pressure of the finger, and when the finger is altogether withdrawn, these beautiful tints all vanish in a moment. We have then in this easy experiment a palpable proof that it is the distance between the surfaces only which determines the colours of thin plates.

The plate of mica which we employ should be about the size of a page in an octavo book*, and about the thickness of a card. This size is convenient to hold with both hands, and the circumference of such a plate of mica will admit of several distinct plates of air. Films should be made between different laminæ of the mineral, by which means we shall possess films of air, covered with plates of mica of different degrees of thickness.

The colours may be seen very well with the naked eye, but they appear more beautiful when examined with a magnifier. The plate of mica should be held near a window, so as to reflect the light.

Looking over your Number for October, p. 375, I saw Dr. Reade's paper "on a permanent soap-bubble." I soon made this beautiful experiment, and beheld the splendid colours as described.

In Dr. Reade's permanent soap-bubble we have a liquid film, and in my experiment a film of air.

I am, Gentlemen, yours, &c.,

London, Nov. 1, 1837. JAMES WALGON.

VIII. *Jervine, a new Vegetable Base.* *By* EDWARD SIMON *of Berlin*†.

I HAVE been so fortunate as to discover in the roots of the *Veratrum album* (Radices Hellebori albi), along with veratria, a new vegetable base possessing some very remarkable properties.

The alkaline extract from the root is boiled several times with water, which has been acidulated with muriatic acid, and the clarified acid liquid precipitated by a solution of pure carbonate of soda. It is necessary that the carbonate of soda be free from any mixture of sulphate of soda. The precipitate is dissolved in alcohol: the solution is then decolorated by means of carbon, and is separated almost, but not completely, from the alcohol by distillation. The residuum consolidates on cooling into a crystalline mass. This is then subjected to pressure, by which it is freed from the greatest part of the uncrystallizable veratria. If the pressed cake be once more moistened with alcohol and pressed, we obtain the new base tolerably pure.

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* Such plates of mica may be had of Knights, Foster Lane.

† From Poggendorff's *Annalen*, vol. xlix. p. 569.

The expressed liquid contains both bases, namely, the new one and veratria. In order to separate the one from the other, the liquid was evaporated to dryness, and the residuum boiled with diluted sulphuric acid. The new base forms with sulphuric acid a salt of very difficult solution, which on cooling is precipitated, while the sulphate of veratria remains dissolved. The treatment with sulphuric acid is again repeated with the residuum. This combination of the new base with sulphuric acid, so difficult of solution, is decomposed by boiling it with a solution of the carbonate of soda.

The most suitable name for the new base would perhaps be veratria, if we might give to what has hitherto been called veratria the name of sabadilline, from its occurring in the seeds of sabadilla, which contain none of the new base. But the name veratria is so generally received for the base from sabadilla seed, that it would be wrong to change it. We could not well term the new base helleborine, since by this means the confusion which already prevails between hellebore and *Veratrum* might be further increased, and it is also possible that a peculiar base may be discovered in some species of hellebore. I have given to the new substance the name of *Jervine*, because Caspar Bauhin, in his *Pinax Theatri Botanici*, p. 186, states, that the Spaniards call the poison from the *Helleborus albus de Balastera*, or *de Jerva*.

Jervine has some very peculiar properties. The most remarkable is, that it forms combinations with sulphuric acid, nitric acid, and muriatic acid, which are not very easily dissolved in water. Of these the combination with sulphuric acid is the most difficult of solution. By an excess of acids these salts do not become much more soluble. If, however, the combination with sulphuric acid be boiled with much water, it is dissolved; on cooling it again separates itself. The acetic and phosphoric acids form with this base combinations easily soluble in water. The base is precipitated from these solutions by the addition of the three above-mentioned mineral acids. By alcohol, the salts of the base which are difficult of solution, are rendered soluble; the solubility, however, in alcohol is not so great as in the salts of the other organic bases. It has previously been mentioned, that the combinations of the base which are hard to be dissolved, for instance those with sulphuric acid, are decomposed by boiling them with alkaline carbonates.

[M. Simon is evidently unacquainted with the *sabadilline* described by M. Couerbe, in *Ann. de Chim. et de Phys.*, vol. lii., p. 376; a substance closely allied to that which he has noticed, if not identical with it.—Edit.]

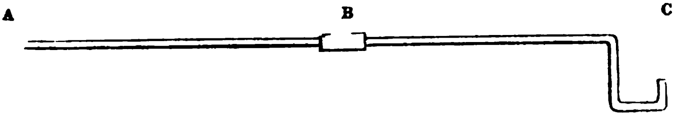
IX. *On a Method of Analysing Organic Compounds.* By ROBERT RIGG, *Esq., M.R.I.*[*]

To Richard Phillips, Esq., F.R.S.

DEAR SIR,

HAVING been so frequently solicited by those who have seen me analyse organic compounds to make an early and more public communication than I have yet done[†] of the method which I adopt, I beg the favour of your inserting in the Philosophical Magazine the following brief account of my very simple apparatus, premising that if at any period I should publish my researches altogether, I shall then go into detail upon this department of chemical manipulation.

The analytical apparatus consists of two small glass tubes connected by a caoutchouc collar, as shown below.



A. A tube, in which is placed the organic compound to be analysed, and which for the analysis of one grain is from seven to ten inches in length, and from three to four tenths of a cubic inch in content.

B. A caoutchouc collar, about an inch in length, in which is put a little dry amianthus or cotton wool.

C. A bent thermometer tube for conveying the gaseous products to the receivers standing over mercury.

The compound to be analysed, a portion of it having been burnt in a platinum spoon with a view to determine the quantity of *residual matter*, is mixed in the usual way with black oxide of copper[‡], varying in quantity from thirty to fifty grains for each cubic inch of carbonic acid gas that will be formed, and varying also with the quantity of water that will be formed. This mixture is put into the *clean* and *dry* tube, and upon it an inch or more of the same kind of oxide: the tube is then filled up with from fifteen to twenty-five grains of dry *amianthus*, and which, during the process of decomposition, condenses the vapour of water, and dries the gaseous products. The part of the tube which includes the amianthus is then heated, so as to drive off all moisture and decomposable matter that may be combined with it, and allowed to cool, when it is weighed, and attached to the bent tube by the collar, as shown in the diagram. The bent tube is placed in the mercurial trough, and the analysing tube rests on the frame or cradle, made of two pieces of strong wire bent at both ends at right angles, and connected together in an oblique direction by slender wires, as represented in the subjoined diagram.

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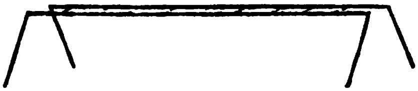
[*] See p. 422 of our last volume.—EDIT.

[†] A diagram and description of the apparatus was laid before the Royal Society about two years ago; and the apparatus itself, together with a tube for measuring minute quantities of nitrogen, before the Chemical Section of the British Association in September last.

[‡] The black oxide, which I prepare by burning copper turnings, and *not from nitrate of copper*, is exposed to a white heat for an hour at least, and well stirred. I afterwards spread it on a plate, where it lies from ten to twelve hours; put it into a bottle, shake it well, and then accurately determine the quantity of air and moisture it has condensed; these remain stationary, when it is kept in stoppered bottles, during the use of two or three pounds of oxide so prepared. [See Dr. Prout's observations on this subject in Phil. Mag. and Annals, vol. iii. p. 35. EDIT.]



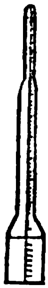
A spirit lamp upon the principle invented by Mr. Cooper, and which can at pleasure be made to give off a flame from one to six, or from one to ten inches in length, and about six inches in height, is what I use. A flame, about an inch in length, is first applied to that part of the tube where no organic compound lies; so soon as this part of the oxide is brought to a red heat, the flame is gradually but very slowly increased in length, until all that part of the tube, where the compound and black oxide are, is at a white heat. During this period the tube is turned round in the flame, the caoutchouc collar admitting of this being done at pleasure. At no time is the decomposition of the substance under analysis quick, but, on the contrary, very slow. The ignited part of the tube being kept at a high temperature, we insure perfect combustion, and prevent the formation of carbonic oxide, which is in all probability the source of much error in quick processes.

During the latter part of the process, and when it is certain that all the atmospheric air has been expelled, a portion of the gaseous products is collected in a separate small tube graduated to hundredths of a cubic inch. When no more gas passes over, the flame is extinguished, and the contents of the tube are shifted by raising and lowering it with a view to that end, and without removing the bent tube from the mercurial trough. The whole being arranged again as at the commencement, that part of the tube which contained the compound under analysis is submitted to a higher temperature, if possible, than before.

The analysing tube is now detached from the other, allowed to cool, and weighed, and the weight lost is found to be that of the gaseous products which have passed from it, all the water having been absorbed by the *amianthus*.

The bent tube is at this time filled with the gaseous products of the analysis, and when the analysing tube is cooled, about $\frac{2}{10}$ of its interstices are also filled with the same, and this I take into account in calculating the products.

On heating the analysing tube again, driving off all the moisture, allowing it to cool, and then weighing it, I have the total loss in weight to the $\frac{1}{1000}$ of a grain, and the contents of the tube in a dry state.



I remove the carbonic acid gas by liquid potassa, and the residual gas, which is left in the small tube which is filled during the latter part of the process, is first transferred into the upper part of the graduated tube represented in the margin*, and where its volume can be read to $\frac{1}{5000}$ of a cubic inch. This I calculate for as nitrogen. The other residual gas is then transferred into the same tube, and the volume of the two read in the centre part to the $\frac{1}{200}$ of a cubic inch. If no nitrogen is present in the compound under examination, the volume of the residual gas is less than that of the atmospheric air in the oxide, together with that which had filled the interstices of the analysing tube, the caoutchouc collar, and the bent tube, and contains less oxygen.

The nitrogen obtained from the gaseous products which are collected in the small tube, serves as a term of comparison for verifying the results of the experiments which I make expressly for the purpose of determining the quantity of that element; and where its existence is doubtful, as, for instance, in sugar, starch, &c., I first fill the tube with carbonic acid gas, use black oxide of copper which has not been exposed to the air, apply the flame *first* to the end of the analysing tube, and am *especially* careful that no carbonic oxide is formed.

The weight of water, and also that of carbonic acid gas and nitrogen, together with their volumes, being known, this mode of conducting ultimate analysis enables me to determine accurately the quantity of water by weight, and the quantity of carbon and nitrogen, both by weight and by volume, in any given compound. And, further, I have the data for a very accurate recapitulation of all the products, so as to be able to speak with tolerable certainty as to the correctness or incorrectness of any experiment so made, and also for testing the correctness of data already received as regards the weight and volume of the different elements contained in the compound under analysis.

The experiments that I have made with this simple apparatus are very numerous. I have before me at this time more than five hundred substances, which I have analysed with a view to discover the chemical changes which occur during the preparation of the earth for the growth of vegetables, the germination of seeds, the vegetation of plants, the formation of vegetable products, the renovation of the atmosphere as regards both nitrogen and oxygen, and the various decompositions of vegetable matter; and many additional experiments will be required to complete the course of analysis which I find to be necessary to the purposes I have in view. The whole inquiry has reference more particularly to agriculture, to horticulture, and to some of those manufactures in which vegetable products are employed. ROBERT RIGG.

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* Several tubes of this kind are required in order to measure the quantities of nitrogen contained in different compounds.

Walworth Road, Dec. 6, 1837.

X. *On additional Fossil Species of the Order* Quadrumana *from the Sewálik Hills. By* H. FALCONER, *Esq., M.D., and Captain* P. T. CAUTLEY.*

[With Figures: Plates I. and II.]

IN the November number of the Journal, (of the Asiat. Soc. of Bengal,) vol. v. p. 739, Messrs. Baker and Durand have announced, in the discovery of a quadrumanous animal, one of the most interesting results that has followed on the researches into the fossil remains of the Sewálik Hills. The specimen which they have figured and described comprises the right half of the upper jaw, with the series of molars complete; and they infer that it belonged to a very large species†. In the course of last rains we detected in our collection an *astragalus*, which we referred to a quadrumanous animal. The specimen is an entire bone, free from any matrix, and in a fine state of preservation from having been partly mineralized with hydrate of iron. It corresponds exactly in size with the *astragalus* of the *Semnopithecus Entellus* or *Langoor*, and the details of form are so much alike in both, that measurement by the callipers was required to ascertain the points of difference. We have forwarded the specimen with a notice to the Geological Society of London, after keeping it some months in reserve, having been diffident about resting the first announcement of fossil *Quadrumana* on any thing less decisive than the cranium or teeth‡.

This *astragalus*, in conjunction with Messrs. Baker and Durand's specimen, satisfied us of the existence of at least two distinct fossil *Quadrumana* in the Sewálik Hills. We have lately become possessed of several fragments, more or less perfect, belonging to the lower jaws of two species, both smaller than Messrs. Baker and Durand's fossil. These we shall now proceed to notice.

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* From the Journal of the Asiatic Society of Bengal, vol. vi. p. 354.

† For Lieuts. Baker and Durand's paper, see Phil. Mag., vol. xi. p. 33.

‡ See our report of the proceedings of the Geological Society, p. 393 of our last volume.—EDIT.

The principal specimen is represented in fig. 1. [Pl. I.] It consists of both sides of the lower jaw; a great portion of the right half is entire, with the whole series of molars; the left half is broken off to the rear of the antepenultimate molar. The two middle incisors are present, and also the left canine broken across at its upper third. The right canine and the lateral incisors had dropt out, leaving but the alveoli. The molars of the left side are destroyed down to the level of the jaw. The right ramus is wanting in more than half its width, together with the articulating and coronoid processes, and a portion of the margin at the angle of the jaw is gone. The specimen is a black fossil, and strongly ferruginous; the specific gravity about 2·70. It was incased in a matrix of hard sandstone, part of which is still left adhering to it.

The jaw had belonged to an extremely old animal. The last molar is worn down so as to have lost every trace of its points, and the three teeth in advance of it have been reduced to hollowed-out discs, encircled by the external plate of enamel. The muscular hollow on the ramus for the insertion of the temporal muscle is very marked, being ·35 inches deep upon a width of ·55.

The dimensions contrasted with those of the *Langoor* or *Semnopithecus Entellus* and the common Indian monkey, or *Pithecus Rhesus*, are as follow :—

Dimensions of the Lower Jaw.	Fossil Sewâlik Monkey.	Semnopithecus Entellus.	Pithecus Rhesus.	Ratio of the Sewâlik fossil to the Entellus.
	inches.	inches.	inches.	
1. Extreme length from the anterior margin of the ramus to the middle incisors	3·6	2·85	2·5	4 3·2
2. Extreme length of jaw (calculated in the fossil)	5·3	4·	3·6	4 3·02
3. Height of jaw, under the second molar measured to the margin of the alveoli	1·35	1·05	·85	4 3·1
4. Ditto at the rear molars	1·2	1·1	·95	4 3·6
5. Depth of symphysis	1·9	1·4	1·1	4 3·
6. Space occupied by the molars	2·3	1·9	1·5	4 3·3
7. Interval between the first molars	·9	·75	·65	4 3·2
8. Antero-posterior diameter of the canine	·5	·4	·3	4 3·2
9. Width of jaw behind the chin under the second molar	1·15	1·05	·95	4 3·7

As in all other tribes of animals in which the species are very numerous, and closely allied in organization, it is next to impossible to distinguish an individual species in the *Quadrumana* from a solitary bone. In the fossil, too, the effects of age have worn off those marks in the teeth by which an approximation to the subgenus might be made. It very closely resembles the *Semnopithecus Entellus* in form, and comparative dimensions generally. The differences observable are slight. The symphysis is proportionally a little deeper than in *Entellus*, and the height of the body of the jaw somewhat greater. The chin, however, is considerably more compressed laterally under the second molar than in the *Entellus*, and the first molar more elongated and salient. So much of the canine as remains has exactly the same form as in the *Entellus*, and its proportional size is fully as great. As shown by the dimensions, the jaw is much larger than in the full-grown *Entellus*: in the former the length would have been about 5·3 inches, while in the latter it is exactly 4 inches. The fossil was a species of smaller size than the animal to which the specimen described by Messrs. Baker and Durand belonged, but less so than it exceeds the *Entellus*.

Our limited means for comparison, restricted to two living species, besides the imperfection of the fossil, and the few characters which it supplies, do not admit of affirming whether it belongs to an existing or extinct species; but the analogy of the ascertained number of extinct species among the Sewálík fossil mammalia, makes it more probable that this monkey is an extinct one than otherwise. There is no doubt about its differing specifically from the two Indian species with which we have compared it.

The next specimen is shown in fig. 5. [Pl. I.] It is a fragment of the body of the right side of the lower jaw, containing the four rear molars. The teeth are beautifully perfect. It had belonged to an adult, although not an aged animal, the last molar having the points a little worn, while the anterior teeth are considerably so. The dimensions, taken along with age, at once prove that it belonged to a different and smaller species than the fossil first noticed.

The dimensions are as follow:—

Dimensions of the Lower Jaw.	Smaller fossil Sewálík species.	Larger fossil Sewálík species.	Semnopithecus Entellus.	Pithecus Rhesus.
	inches.	inches.	inches.	
1. Length of space occupied by the four rear molars	1·48	1·7	1·48	1·25
2. Height of jaw at the third molar	·95	...	1·1	·9

The length of jaw, therefore, estimated from the space occupied by the teeth, would be 4 inches, while in the larger fossil it is 5·3 inches, a difference much too great to be dependent merely on varieties of one species. Besides, we have another fragment, also belonging to the right side of the lower jaw, and containing the last molar, which agrees exactly in size with the corresponding tooth in the figured specimen*. This goes to prove the size to have been constant. The fossil, although corresponding precisely in the space occupied by the four rear molars with the *Entellus*, has less height of jaw. There is further a difference in the teeth. In the *Entellus* the heel of the rear molar is a simple flattened oblique-surfaced tubercle, rather sharp at the inside. In the fossil, the heel in both fragments is bifid at the inside. The same structure is observable in the heel of the rear molar of the common Indian monkey *P. rhesus*. It is therefore probable that the fossil was a *Pithecus* also. It was considerably larger, however, than the common monkey, and the jaw is more flattened, deeper, and its lower edge much sharper than in the latter. This difference in size and form indicates the species to have been different.

It would appear, therefore, that there are three known species of fossil *Quadrumana* from the Sewálík Hills: the first a very large species, discovered by Messrs. Baker and Durand; the second, a large species also, but smaller than the first, and considerably larger than the *Entellus*; the third, of the size of the *Entellus*, and probably a *Pithecus*; and further, that two of the three at least, and most probably the third also, belonged to the types of the existing monkeys of the old Continent, in having but five molars, and not to the *Sapajous* of America.

There are at present upwards of 150 described species of existing *Quadrumana*, and as the three fossil ones all belonged to the larger-sized monkeys, it is probable that there are several more Sewálík species to be discovered. We have some specimens of detached teeth, of large size, which we conjecture to be quadrumanous; but their detached state makes this conjecture extremely doubtful.

Besides the interest attaching to the first discovery in the fossil state of animals so nearly approaching man in their organization, as the *Quadrumana*, the fact is more especially interesting in the Sewálík species, from the fossils with which they are associated. The same beds, or different beds of the same formation, from which the *Quadrumana* came, have yielded species of the camel and antelope, and the *Anoplo-*

[* We presume that fig. 4. of Pl. I. represents this third fragment.—Edit.]

therium posterogenium, (nob.): the first two belonging to genera which are now coexistent with man, and the last to a genus characteristic of the oldest tertiary beds in Europe. The facts yielded by the reptilian orders are still more interesting. Two of the fossil crocodiles of the Sewáliks are identical, without even ranging into varieties, with the *Crocodilus biporcatus* and *Leptorynchus Gangeticus*, which now inhabit in countless numbers the rivers of India; while the *Testudinata* are represented by the *Megalochelys Sivalensis* (nob.), a tortoise of enormous dimensions, which holds in its order the same rank that the *Iguanodon* and *Megalosaurus* do among the *Saurians*. This huge reptile (the *Megalochelys*) —certainly the most remarkable of all the animals which the Sewáliks have yielded—from its size carries the imagination back to the æra of gigantic Saurians. We have leg bones derived from it, with corresponding fragments of the shell, larger than the bones in the Indian unicorned rhinoceros!

There is, therefore, in the Sewálik fossils a mixture in the same formation of the types of all ages, from the existing up to that of the chalk; and all coexistent with *Quadrumana*.

P.S. Since the above remarks were put together, we have been led to analyse the character presented by a specimen in our collection, which we had conjectured to be quadrumanous. The examination proves it to be so incontestably. The specimen is represented in figs. A, B, and C [of Pl. II.] It is the extra-alveolar portion of the left canine of the upper jaw of a very large species. The identification rests upon two vertical facets of wear, one on the anterior surface, the other on the inner and posterior side, and the proof is this. The anterior facet *b* has been caused by the habitual abrasion of the upper canine against the rear surface of the lower one, which overlaps it, when the jaws are closed or in action. This facet would prove nothing by itself, as it is common to all aged animals in the carnivora and other tribes in which the upper and lower canines have their surfaces in contact. The second facet *c must* have been caused by the wear of the inner and rear surface of the canine against the outer surface of the first molar of the lower jaw. But to admit of such contact, this molar must have been contiguous with the lower canine, without any blank space intervening; for if there was not this contiguity, the upper canine could not touch the lower first molar, and consequently not wear against it. Now this continuity of the series of molars and canines without a diasteme or blank interval, is only found, throughout the whole animal kingdom*, in man, the *Quadrumana*, and the

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* Cuvier, *Ossemens Fossiles*, tome iii. p. 15.

Anoplotherium. The fossil canine must therefore have belonged to one of these. It were needless to point out its difference from the human canine, which does not rise above the level of the molars. In all the species of *Anoplotherium* described by Cuvier, the canines, while in a contiguous series with the molars, do not project higher than these, being rudimentary, as in man. Of the Sewálík species, *Anoplotherium posterogenium* (nob.), we have not yet seen the canines; but it is very improbable, and perhaps impossible, that the fossil could belong to it. For if this species had a salient canine, it must have been separated from the molars by an interval, as in the other *Pachydermata*; otherwise the jaws would get locked by the canines and molars, and the lateral motion required by the structure of the teeth, and its herbivorous habit, would be impracticable; and if there was this interval, the upper canine could not have the posterior facet of wear. The fossil canine must therefore have belonged to a quadrumanous animal. This inference is further borne out by the detrition of the fossil exactly corresponding with that of the canines of old monkeys.

The dimensions are:—

Length of the fragment of canine . .	1·75 inches.
Antero-posterior diameter at the base .	·8
Transverse ditto	·7
Width of the anterior facet of wear .	·6

The two diameters are greater than those of the canine of the Sumatra Orang-otang described by Dr. Clarke Abel* as having been 7½ feet high. The *Cynocephali* have large and stout canines, more so comparatively than the other *Quadrumana*. But to what section of the tribe our fossil belonged, we have not a conjecture to offer. We may remark, however, that the tooth is not channeled on three sides at the base, as in the *Entellus*. Does the fossil belong to the same species as the jaw discovered by Messrs. Baker and Durand, or to a larger one?

Note by the Editor of the Journal of the Asiatic Society.—We have sketched Dr. Falconer's highly curious fossil tooth in position with the lower jaw of the Sumatran Orang-otang from the Society's Museum, in fig. C [of Pl. II.] There is a third facet of wear at the lower extremity *d*, which on reference we find Dr. Falconer attributes, like *c*, to attrition against the first molar, being observable, he says, in many aged animals. The worn surfaces *c* and *d* are uniformly polished, and have evidently originated from attrition against a tooth; but with regard to the principal facet *b*, we confess we have a degree of scepticism, which can only be removed by a certainty, that the fossil had been seen extracted from the matrix. In the first place, the great extent of the worn surface and its perfect flatness could hardly be caused by attrition against the lower canine, which should produce a curvature measured by the length of the jaw as radius. In the next place, the enamel of the tooth is less worn than the interior and softer part of the fossil; and, thirdly, on examination with a magnifier, numerous scratches are visible in divers directions: all these indicating that the facet may have been produced *on the fossil*, by grinding it on a file, or some hard flat surface. On showing the fossil to Madhusudana, the medical pandit of the Hindu College, he at once pronounced that the tooth had been ground down to be used in medicine, being a sovereign specific in the native pharmacopœia. This circumstance need not necessarily affect the question, for it is probable that the native druggist would commence his rubbing on the natural plane, if any presented itself to his choice; but Dr. Falconer and Captain Cautley, to whom we have returned the fossil with a communication of our doubts, assure us in reply that the fossil tooth was brought in along with a large collection, so that there is every improbability of its having been in possession of a native druggist. At any rate, it is not on the front wear that they so much rest their argument of its origin, as on the posterior abrasion, which could only happen in the jaw of a quadrumanous animal. In fact, they have recent quadrumana showing precisely similar wear on a small scale, and no other head will do so. We find only one exception in the Society's Museum, viz., the tapir, whose right upper incisor (or non-salient canine) falling between the two lower ones, is worn nearly in the fashion of the fossil; but it is less elongated.

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* Asiatic Researches, vol. xv. p. 498; [or Phil. Mag. and Annals, N. S. vol. i. p. 219.]

XI. *Notice of additional Fragments of the Sivatherium.*[*]

[With Figures: Plate II.]

BEFORE Colonel Colvin's departure for Europe, we requested permission to take a cast of the beautifully preserved lower jaw of the *Sivatherium* which he exhibited at the Government House scientific party in January last [1837]. In further token of his zeal for science, and of his ever-readiness to oblige, he has, even in the hurry of embarkation, favoured us with the accompanying lithographic drawings of the same jaw, and of the larger fragment of the occiput, also on its way to adorn some cabinet of fossil osteology in his native land. This fragment is the more valuable on account of its being perfect in the parts deficient in Dr. Falconer's specimen, published in the Asiatic Researches, vol. xix.* We subjoin the Colonel's note, explanatory of the drawings [Plate II.]

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[*] From the Journal of the Asiatic Society of Bengal, vol. vi. p. 152; being a communication by the Secretary, James Prinsep, Esq., F.R.S.

"I herewith send you figures of the *Sivatherium*; one of the portion of the head I was fortunate in having brought in from the lower hills below and west of Náhan, just before I left Dádúpur. It arrived encumbered with a good deal of hard sandstone matrix, most of which I had cleared away. This specimen is valuable, though it has no teeth, from having the occiput very entire, and from its proving the accuracy of Dr. Falconer's assumption, founded on examination of the original head, that the animal had four horns with bony cores, as this has the offset of one of the back branched horns very clearly marked; suitable to which I may mention, that Capt. Cautley has found in his collection a large flat horn. In this plate, fig. 1; is a view of the occiput appearing, partly distorted from occurrence of a shift. For the left lower jaw of the *Sivatherium*, delineated in the same plate, I am indebted to Conductor W. Dawe, of the Canal Department, for whom it was brought in, inclosed in a mass of similar sandstone, from near the sources of the Sombe river, north of Dádúpur, and east of Náhan, shortly before I came away. It is a very perfect and beautiful specimen, with its molars, four in number, almost quite entire, and is the specimen which you have moulded.

"Fig. 2 is of the outside of the left lower jaw.

"Fig. 3, ditto crown of the teeth, in which I have endeavoured to be accurate in drawing the flexures of the enamel.

"In fig. 2, I have hardly had the jaw perpendicular when drawing it, as it does not sufficiently express the great height of the inner range of the molars over their outer edge, which a cross section would have better shown; but as the specimen is gone on board, I cannot now make it†."

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* See Journal of Asiatic Society, vol. v., January; [or Lond. & Edinb. Phil. Mag., vol. ix. p. 193.]

† The specimen represented in figures 2 and 3 was exhibited at the meeting of the Geological Society on Dec. 6, 1837. We have slightly altered Col. Colvin's description, in order to make it correspond with the figures in Plate II, which are a part only of those given in the Journal of the Asiatic Society.—Edit.

XII. *Meteorological Observations for Portions of the Years 1836 and 1837, made at Bermuda; and a Notice of an Aurora Borealis seen in low Latitudes. By Lieut.-Col.* A. Emmett, *Royal Engineers.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE great attention now given to meteorology induces me to forward for the Philosophical Magazine a summary of the observations taken by me within the period of twelve months*. Every attention has been given to their accuracy; and from the character of the instrument-maker, Mr. Newman, I think they may be received with confidence. I am well aware that the observations of twelve months are insufficient for establishing many data, yet there are two or three points which merit attention, particularly that of the horary changes.

I shall not at present trespass further on your pages than to notice that a most splendid aurora borealis was seen here on the 25th of January, as also even down to the tropics, respecting which I transcribe an extract from the journal of Captain Willis of H.M.S. Cruiser, which he was kind enough to send me from Jamaica:

"In lat. 22° north, long. 57° west, at 8 p.m., the aurora borealis was observed very brightly; it began at 8 by a red flush suddenly spreading itself over a great part of the northern hemisphere, extending in altitude to about 30°; then a number of streamers showed themselves shooting upwards with great brilliancy. They continued about fifteen minutes, then ceased, leaving that part of the sky for a time blood red."

The appearance here was very similar, but more extensive and of longer continuance.

I remain, &c.

Bermuda, July 11, 1837.

A. Emmett,
Lieut.-Col., R. E.

P.S. This aurora appears to have been seen over a large extent of North America.

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* Lieut.-Col. Emmett's observations for the period from July 1 to September 30, 1836, having already appeared, in the paper prepared by Dr. Dalton, inserted in our last volume, p. 449, we have omitted them in the tables which follow; retaining, however, the means for the entire six months, from July 1 to December 31, 1836.—Edit.

Meteorological Observations at St. George's, Bermuda, from 1st of October to 31st of December, 1836.

Months.	Barometer.				Temperature.				Temperature of Sea.	Hygrometer.		Rain.			Storms or Gales.	Prevailing Winds.	
	Mean.	Max.	Min.	Diff.	Mean.	Max.	Min.	Diff.		Dew Point.	Vapour.	Day.	Night.	Total.			
October.	29·978	30·282	29·774	·508	73·07	78·5	62	16·5	78·5 75	64	·597	6·16	11·08	17·24	11	1st SW. 2nd SE.	Lightning 11.
Novemb.	29·990	30·320	29·530	·790	65·9	73·	53	20	67·5 62·5	54·8	·440	·77	2·53	3·30	11	1st NW. 2nd SE.	Lightning 2.
Decemb.	30·150	30·620	29·840	·780	60·55	70·25	51	19·95	64 61	54	·429	·43	2·02	2·45	4	SE. SW. NW. equ.	Bar. 30·620 on 20th. Wind E. to SE. & strong.
Means July 1 to Dec. 31.	30·042	30·375	29·760	·615	71·6	77·27	62·3	14·9	73·1	64	·597	7·36	15·63	22·99	32		

Barometer 60 feet above the sea, for which no correction is made. The *monthly means* are corrected for capacity, capillary, &c. and then reduced to 32° Fahr. The mean of 9 a.m. and 4 p.m. (those having been found the periods of greatest and least elevation) are taken for the mean of the day; and if not then taken 1 p.m. is substituted. Maximum and minimum heights are not corrected. Read off by a magnifier on the convexity of the column; light just excluded.

Temperature: means taken from self-registering thermometer, once in twenty-four hours.

Dew-point, perhaps too high, taken by moist-bulb thermometer compared occasionally with Dalton's method.

Summary from 1*st of January to* 30*th of June* 1837.

Months.	Barometer.				Temperature.				Temperature of Sea.	Hygrometer.		Rain.			Gales.	Prevailing Winds.	
	Mean.	Max.	Min.	Diff.	Mean.	Max.	Min.	Diff.		Dew Point.	Vapour.	Day.	Night.	Total.			
January.	29·762	30·280	29·394	·886	57·9	67·5	47	20 5	61 64	52·9	·413	·92	2·56	3·48	20	1st NW. 2nd SW.	Lightning, 5 days or nights.
February	30·065	30·482	29·732	·750	58·89	68·5	47·5	21	64 62	54	·429	·58	3·06	3·64	10	1st NW. 2nd SW.	Lightning, 2.
March.	29·999	30·320	29·444	·876	59·3	64·5	53	11·5	62 63	53·8	·426	1·67	2·38	4·05	6	1st NE. 2nd NW. 3rd N.	Lightning, 1.
April.	29·989	30·430	29·598	·832	62·8	71	55	16	62 68	56·2	·462	1·74	2·02	3·76	4	1st NW. 2nd SW.	Lightning, 1.
May.	30·139	30·350	29·936	·414	69·13	75	58·5	16·5	67 74	61·7	·554	3·03	·83	3·86	2 mod.	1st NE. 2nd SE.	Lightning, 4.
June.	30·006	30·390	29·762	·628	73·2	79·5	66·5	13	75 79·5	67·8	·672	3·18	5·35	8·53	2 mod.	1st SW. 2nd NW. 3rd SE.	Lightning, 10.
Mean.	29·993	30·375	29·644	·731	63·54	71	54·6	16·3	66·9	57·7	·485	11·12	16·2	27·32	44		
1837. Halfyear.	30·042	30·375	29·760	·615	71·6	77·27	62·3	14·9	73·1	64	·597	7·36	15·63	22·99	32		
1837.	29·993	30·375	29·644	·731	63·54	71	54·6	16·3	66·9	57·7	·485	11·12	16·2	27·32	44	Rain, last	9 months only.
Mean of year.	30·017	30·375	29·702	·673	67·5	74·13	58·45	15·6	70	60·8	·538	18·48	31·83	50·31	76		

Comparison of Barometer at 9 *a.m.*, 4 *p.m.*, *and* 9 *p.m.*

Month. 1836.	No. of Observations.	9 A.M.	4 P.M.	Diff.	No. of Observations.	9 P.M.	4 P.M. higher than 9 A.M.
July.	24	30·162	30·149	·013	24	30·183	7
August.	23	30·138	30·108	·030	23	30·132	6
Sept.	18	30·140	30·107	·033	18	30·132	2
October.	18	30·066	30·034	·032	18	30 053	1
Nov.	26	30·062	30 031	·031	...	...	6
Dec.	30	30·224	30·188	·036	27	30·215	5
Mean.	139	30·132	30·103	·029	110	30·145	27
1837. Jan.	30	29·892	29·832	·060	24	29·865	5
Feb.	27	30·132	30·101	·031	14	30·118	7
March.	30	30·075	30·032	·043	25	30·048	7
April.	23	30 027	30·007	·020	17	30·052	5
May.	22	30·136	30·128	·008	6	30·104	5
June.	24	30·075	30·054	·021	22	30·071	5
	156	30·057	30·025	·032	108	30·043	34
1836	139	30·132	30·103	·029	110	30·145	27
1837.	156	30·057	30·025	·032	108	30·043	34
Total Mean.	295	30·094	30·064	·030	218	30·094	61

All the observations at 9 a.m. and 4 p.m. were taken on the same days; those at 9 p.m. not always so. No corrections are made. Occasionally, that is in the proportion of 1 to 5, the barometer is higher at 4 p.m. than at 9 a.m.

Time at Bermuda is not correctly kept, but during the hot season the critical hours appear to be between 8 and 9, and about 4; at other times half an hour nearer noon.

The horary difference exceeds in a very trifling degree that at Paris.

Various Memoranda.

Barometer used, Newman's iron cistern portable.

Thermometer, Newman's compared with one furnished as a standard, with his own.

Dew-point. The difference between the attached thermometer and the moist bulb multiplied by 2, and subtracted from the latter, gives that point nearly; but less dependence is to be placed on these columns than on any other.

Winds: relative Proportions.

1836.	N.	NE.	E.	SE.	S.	SW.	W.	NW.
July.	...	...	1	19	9	44	10	10
August.	3	8	1	15	2	39	11	14
Sept.	4	24	8	24	11	10	3	6
Oct.	11	17	1	22	8	25	2	7
Nov.	4	12	4	18	3	10	2	32
Dec.	6	16	4	21	8	21	...	21
	28	77	19	119	41	149	28	90
1837. Jan.	...	3	2	1	3	27	10	47
Feb.	9	5	...	7	8	22	4	29
March.	19	26	1	...	4	16	1	24
April.	12	7	3	5	9	28	8	18
May.	4	23	9	19	12	9	8	8
June.	9	6	...	18	8	24	6	19
	53	70	13	56	44	126	37	145
1836.	28	77	19	119	41	149	28	90
	81	147	32	169	85	275	65	235

The winds are taken at sun-rise, noon, and sun-set, and collected and classed therefrom. They are often very irregular, and on many occasions have been at every point of the compass within twenty-four hours. They often freshen about 9 a.m., moderate after noon, and again freshen towards sunset; but these changes are not general.

Calms of 24 or 36 hours occasionally occur in the months from June to September, but not often so long; but they are frequent for shorter periods, mostly in the afternoon.

General Observations on the Year.

Barometer. Greatest height, corrected and reduced, 30·664; least ditto, 29·414; greatest range, 1·25: highest with NE. and SE. winds; lowest, with NW. and SW.

Thermometer. Greatest observed power of common black bulb, 106°; greatest observed heat of ground, thermometer barely covered, 142°; sun's greatest force about 1 p.m.; hour of greatest heat 4 to 4½ in the hot season, and 3 to 3½ at other times. Dew-point, accords with the latter. Clear days, proportion 1 to 4 or 5. Halos, rare.

A. Emmett,
Lieut.-Col. Royal Engineers.

XIII. *On the Wave-surface in the Theory of Double Refraction.*

By J. W. Lubbock, *Esq., F.R.S.*[*]

MR. TOVEY remarks, p. 524 of the last Number, that I have taken for granted that the differential equations of molecular attraction may be reduced to the form

$$\frac{d^2\xi}{dt^2} = m\Sigma\left\{\phi r + \psi(r)\Delta x^2\right\}\Delta\xi.$$

I confess that the reasoning of Fresnel connected with this matter is to me by no means clear, but I presume the reduction will at all events be admitted to be possible in the manner pointed out by M. Cauchy in the *Nouveaux Exercises*, p. 11, the *axes of elasticity* being the principal axes of the curve of the second order given by the equation (36.), p. 12.

Instead of taking

$$\Delta\varrho = \frac{d\varrho}{dr}\Delta r + \frac{d^2\varrho}{1.2\,dr^2}\Delta r^2 + \&c.,$$

I might have taken, in the manner of Mr. Kelland and of M. Cauchy,

$$\varrho = A\cos(nt-kr) \qquad \frac{d^2\varrho}{dr^2} = -k^2\varrho.$$

$$\Delta\varrho = -A\cos(nt-kr)(1-\cos(k\Delta r)) + A\sin(nt-kr)\sin(k\Delta r)$$

$$= -2\sin^2\left(\frac{k\Delta r}{2}\right)\varrho + A\sin(nt-kr)\sin(k\Delta r).$$

Neglecting the terms multiplied by sin $(k\Delta r)$, for the reasons given by M. Cauchy, p. 10, if

$$a^2 = 2m\Sigma\left\{\phi r + \psi(r)\Delta x^2\right\}\frac{\sin^2\left(\frac{k\Delta r}{2}\right)}{k^4}$$

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[*] Communicated by the Author.

$$b^2 = 2\,\mathfrak{m}\,\Sigma\left\{\phi r + \psi(r)\,\Delta y^2\right\}\frac{\sin^2\left(\frac{k\,\Delta r}{2}\right)}{k^2}$$

$$c^2 = 2\,\mathfrak{m}\,\Sigma\left\{\phi r + \psi(r)\,\Delta z^2\right\}\frac{\sin^2\left(\frac{k\,\Delta r}{2}\right)}{k^2}$$

$$\Delta\xi = \Delta\varrho\cos X \qquad \Delta\eta = \Delta\varrho\cos Y \qquad \Delta\zeta = \Delta\varrho\cos Z$$

$$\frac{d^2\varrho}{d t^2} = \left\{a^2\cos^2 X + b^2\cos^2 Y + c^2\cos^2 Z\right\}\frac{d^2\varrho}{d r^2}$$

$$= v^2\,\frac{d^2\varrho}{d r^2} \qquad v^2 = a^2\cos^2 X + b^2\cos^2 Y + c^2\cos^2 Z,$$

if $\varrho = A\cos(nt - kr)$, this expression represents a wave of light, moving with a velocity $v = \frac{n}{k}$, the length of the wave being $\frac{2\pi}{k}$.

In my paper in the last Number, p. 492, l. 4, *for* "a function of those coefficients," *read* "a function of those constants."

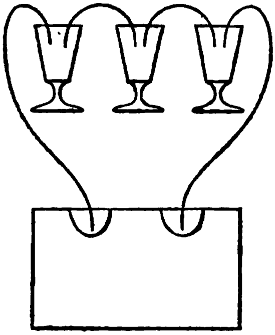
XIV. *On the peculiar Voltaic Conditions of Iron and Bismuth.*

By H. M. Noad, *Esq.*

To the Editors of the London and Edinburgh Philosophical Magazine.

Gentlemen,

I PERCEIVE by the last number of your Journal that I have been anticipated in some of the remarks I had intended to make on the "Chemical peculiarity of Bismuth" by Dr. Schœnbein. My last communication which you have requested me to incorporate with the present related to phænomena observable when iron wire is exposed under certain circumstances to the action of diluted nitric acid sp. gr. 1·14 or 1·2. The experiments described were these:—Dr. Schœnbein, in referring to a short letter of mine in the Philosophical Magazine of April last, (vol. x. p. 276) says, that "the facts there described are quite the same as those previously described by himself in a letter to Dr. Faraday;" but there is this difference between them,—the Professor's experiments were made with a strong acid, mine with an acid much diluted with water; in the one case, as is now well known, the peculiar state of iron is called forth by a variety of methods, by heat, by oxidation, by previous immersion in very strong acid, by association with platina, palladium, gold, &c., and, lastly, as Dr. Schœnbein has shown, by association with peroxide of lead; but in a diluted acid none of these methods have a similar effect: indeed, by associating iron wire with platina, and immersing it in a glass of nitric acid sp. gr. 1·14, I have frequently observed the action to be increased; but though the peculiar state is not called forth by *direct* association with platina, it is when the wire is connected with that metal in a certain manner through the medium of a galvanometer; in proof of which I beg to call the attention of Dr. Schœnbein and other readers of the Philosophical Magazine who may be interested in the subject to the following facts, by which also it will, I think, be shown, that when in the peculiar state it is *absolutely incapable* of conducting voltaic electricity in a low state of intensity.



1, 2, 3, represent three glasses half filled with diluted nitric acid: from 1 proceeds a platina wire connected with one of the mercury cups of a delicate galvanometer, and from 3 an iron wire of equal length, and dipping into the other cup; the glasses are connected by bent iron wires. Now, in acid of this strength (sp. gr. 1·14 or 1·2), iron wire, however thickly coiled with platina or with any other metal that I have tried, is strongly acted on, the brown oxide being instantaneously and copiously deposited; neither is the action prevented if the platina is associated with its upper end, and dipped into the acid in such a manner that the point of junction shall be above the surface of the fluid: but if the platina wire is connected with the galvanometer, and immersed in the glass of acid, and the iron wire united *first with the instrument*, and then dipped into the acid, it is brought to the peculiar state, and is not in the slightest manner acted on in any length of time, nor does the galvanometer evince any signs of electrical action. Any other mode of completing the circuit is ineffectual, but 3 or 4 glasses containing a similar acid may be united by bent pieces of wire, as shown in the sketch, with a result similar to that when a single glass is employed.

This very interesting experiment is one of delicacy, and without the following precautions will generally fail. 1st, The wire must be in its *natural state*: any attempt to clean the surface by scraping or filing destroys the peculiarity; one drop of acid touching the surface previous to immersion does the same. In repeating the experiments lately before a number of persons, I continually failed in exhibiting the phænomena, till I observed that the pliers with which I cut and bent the wires had become accidentally wetted with the acid, and it was sufficiently evident that this had interfered with the result, for when they were carefully dried the experiment always succeeded. When a wire has been once used, the property is destroyed in it, at least for a long time. The acid should be quite free from nitrous acid vapour, and I have found it better, in order to insure this, to heat it gently for some time, using it when quite cold, after diluting it with the requisite quantity of distilled water. Observing these precautions in all the experiments I shall here describe, I have no doubt that any person who feels inclined to repeat them will arrive at results similar to mine. The iron wire I employed was $\frac{1}{12}$th of an inch in diameter, the platina much less.

Suppose three glasses arranged, as in the sketch, before a galvanometer, the platina and iron wires immersed in their respective glasses, and connected with the instrument in such a manner that the iron shall be *active*. Let the iron glass be united to the one next to it by a bent wire; now take another, similarly bent, clean piece, and dip one end *first* into the *iron* glass, and then gradually bring the other end into the platina glass, and it will be found *inactive*. At the moment of immersion, a slight deflection of the needle will be perceptible, but this will soon cease; and after it has taken up its usual position, rapidly break and renew the contact of the platina or iron extreme wire with its cup: not the slightest further oscillation will ensue, which decisively proves that no electrical current is passing, although a strong one is called forth by the action of the extreme iron wire. If a bent platina wire, not larger than a pack-thread, or a strip of any other metal, is made to connect the two glasses, an immediate deflection of the needle takes place, greater or less in proportion as the metal is more or less acted on. I have repeated this curious experiment fifty times in the presence of various persons with the same result, never having been able to trace the slightest electrical current across the inactive wire, and never having met with any other metal beside that refused the passage. If the connecting wire with both ends active is now removed, and another new and clean one substituted, observing the same order in immersing its ends, the extremity nearest the platina glass will be rendered inactive, and the same will result with any number of arrangements. The galvanometer I have employed is a very delicate one; the needle is astatic and suspended by a single hair; it was constructed for me by Messrs. Knight of Foster Lane.

When the needle of the galvanometer is quite still, let either of the inactive ends be touched at a single point with any metal on which the acid is capable of exerting an action, and it will not only immediately be thrown into action itself, but all the inactive wires in the series after it will turn active also, the needle being strongly deflected; but if it should happen, as it sometimes, but rarely, does, that one termination retains its peculiar state, then there will be *no disturbance of the needle*, which is an additional and irresistible proof of the utter incapacity of the iron in that state to conduct weak current electricity. It will be found impossible to render either end of the connecting wires inactive by completing the circuit by dipping into the *platina glass first*; neither can the terminal iron wire connected with the galvanometer be brought into the same state unless *both ends* of both connecting wires are undergoing chemical action, it being absolutely necessary that there should be a free passage for the slight current which the needle shows to be produced the moment the wire dips into the acid, and on *which current* the development of the peculiar state depends.

Again, supposing the terminal iron wire, or *one* end of one or of both the connecting wires to be inactive, if the platina terminal wire is removed from its glass, and a wire of any other metal on which the liquid exerts chemical action substituted for it, the counter current which that metal calls forth through the liquid instantly destroys the peculiar state of all the wires in the arrangement: the brown oxide appears as if by magic, and the needle is deflected.

From these experiments it appears to me quite clear, that the inactive state of iron is occasioned by a voltaic current of a certain intensity passing through it in a certain direction; and as it is impossible to induce such a state on wire after its surface has been abraded or previously wetted with acid, it is evident that *the state of surface* over which the current passes is closely connected with the phænomenon; it seems that it must bear a certain relation to oxygen, or, in the happy language of Dr. Faraday, be *in a state equivalent to an oxidation.*

That a current *does* pass from it previously to its acquiring the peculiar state, is evident from the slight motion of the needle at the moment the wire first touches the acid; that it afterwards stops the passage of a current altogether, is shown by the subsequent stillness of the instrument even when contact with its cups is rapidly broken and renewed; and that a slight counter current is sufficient to destroy the inactive state altogether, is shown by touching it at a single point for a moment with any metal on which the diluted acid exerts an action, and in a still more interesting manner by making the extreme platina and iron wire change places.

With regard to the "Chemical peculiarity of Bismuth," I quite agree with Dr. Schœnbein that it seems to belong to a distinct class of phænomena. It differs from iron in these particulars: 1st, Platina wire will immediately stop its *effervescing* action in nitric acid sp. gr. 1·2, though it will not prevent its slow oxidation, as is proved by its turning black, and also by appeal to the galvanometer: the same metal will not, under similar circumstances, protect iron, as has sufficiently been shown. 2ndly, I have observed, that when bismuth has once had its action with the acid lessened, it does not regain it for a long time, *even after the surface has been removed by filing*; but by degrees its original character is restored: and, lastly, when substituted for the bent iron wires, as in the experiments above described, it allows the electrical current to pass quite as freely as active iron wire.

These are the principal facts I have thought worth communicating. I have made many other experiments with voltaic piles of various sizes, but the results at which I have arrived have in general coincided so closely with those described by Dr. Schœnbein, that I shall not trouble you with a relation of them. In conclusion, however, I would strongly recommend those who may have an opportunity of procuring cobalt and nickel in a state of purity, and in sufficient abundance, to repeat these experiments, substituting those metals for the iron; and I cannot help thinking, from the general chemical analogy between these three metals, that something interesting may thereby be elicited[*]. I am, yours, &c.

Shawford, Dec. 11, 1837. HENRY M. NOAD.

P. S.—I take the opportunity which this letter affords me to state, that through the kindness of Mr. J. Denham Smith I have been furnished with some crystals of the hydrates of barytes and strontia from the specimens on which his experiments were made; and it is due to him and to the public to state, that the results of my analysis have agreed so closely with his, that there can be no doubt that the constitution assigned by him to these hydrated metallic oxides is perfectly correct. I have, nevertheless, repeated also the analysis of the crystals I obtained, with the same results as stated in my letter on that subject; and I can therefore only attribute the difference to the extreme difficulty of depriving these hydrates thoroughly of uncombined water. [See Lond. & Edinb. Phil. Mag. vol. ix. p. 87; vol. xi. p. 301.]

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[*] It must be recollected, however, that Professors Schœnbein and Degen were convinced, by their experiments, that the peculiar condition cannot be excited either in cobalt or in nickel. See our last Number, p. 547.—EDIT.

XV. *A Report of the Progress of Vegetable Physiology during the Year* 1836. *By* J. Meyen, *Professor of Botany in the University of Berlin.*[*]

[Continued from p. 537.]

On the Structure and Growth of the more perfect Plants.

MOHL[†] has published a very interesting work on the structure and development of the bark in the stems of dicotyledons, in which this subject has been treated comparatively in various plants. The experiments of Mohl are as follows: In the bark of a young branch of the cork-oak (*Quercus Suber*) four distinct layers are to be distinguished. The exterior layer is the epidermis; it consists, as in other cases, of a simple layer of flat thick-sided cells, and is dotted with star-like hairs. (De Candolle, it is true, observes that the epidermis of trees is never covered with hairs.) The second layer lies close under the epidermis, and consists of 3—5 strata of thinner-sided cells, void of colour and of granules, which are for the most part deposited horizontally, and are also like the cells of the epidermis, rather compressed (*i. e.* towards the surface of of the stem). The third layer is a cellular envelope, which appears as a green parenchymatous layer of cells. In this layer of green cells appear single, colourless, rather larger cells, which contain small granules also void of colour; a circumstance which is also to be found in many other plants. The inner or fourth layer is the liber or fibrous layer, which however is only recognised as a distinct layer in branches of some years' growth. In branches from two to three years old of this plant we find the above-mentioned layers of the bark scarcely changed: the epidermis and the second layer are unchanged; the parenchyma, on the other hand, of the cellular envelope is enlarged; the cells have become thicker, and we find dots on the partitions. First, in the third and fifth year, the epidermis, which can no longer follow the expansion of the bark, and in general of the mass of the young branch, acquires small cracks, and now a great change takes place in the layer of cork situated under it. This layer, which at first was so small, enlarges on the inner side by depositions of new layers. The new layers consist, like the old ones, of thin-sided colourless cells, but lie with their longer diameter of their length in the direction of the bark. With this continual increase in size of the interior layers, the exterior ones split, and give to the stem an irregular rugged surface. The substance thus originated is the cork, which, as is well known, is applied to such various uses. We can perceive in every cork that its increase took place in layers; and that at the limits of the two layers the cells become rather smaller, and with thicker membranes, from which circumstance these spots appear darker, just as the external ends of the annual rings of the *Coniferæ.* We may always perceive that the annual rings in the wood of trees also exhibit very various and thick layers, that they are often irregularly deposited in thick masses. In cork this is by far more the case. In the cork-oak the bark falls off every eight or nine years, and is taken off some years sooner for useful purposes. De Candolle is of opinion that it is the cellular envelope which is here developed.

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[*] From Wiegmann's *Archiv für Naturgeschichte*, 1837, Part 3. Translated by Mr. Wm. Francis.

[†] Observations on the development of cork, and of the bark on the rind of arborescent dicotyledons.—*Tübingen*, 1836.

With this development of the cork substance in consequence of age, the development of the third and fourth layer goes on at an equal rate; the cellular envelope however increases but in a small degree, and without the formation of new layers, while the groups of colourless cells, which often contain crystals, increase more and more in circumference. The inner layer develops new fascicles of liber, and the cells situated between the fibres are like those of the cellular envelope, in which, as Duhamel had previously stated, they are immediately continued.

Dutrochet* has published some observations on the formation of the cork substance; he especially directs attention to the fact that the increase of this mass takes place towards the interior, as in the corneous tissue of animals. Dutrochet also finds it very necessary to determine closely the external envelope of the bark, and in this he follows the statements of Brongniart, since he divides the epidermis into the cuticula and the cellular membrane. I stated my own opinion on this subject in a recent memoir in the second part of this *Archiv.*

The development of the cork substance in *Acer campestre* is quite similar; here it arrives at perfection even in the first year, immediately after which the epidermis splits at various points. In this case then the development of the cork proceeds very rapidly, but it also ceases sooner than in the cork-oak, and in later years the two other layers of the bark are then developed in such a manner that there gradually re-originates a certain symmetry between the individual layers.

In other cases, as for instance in *Banksia serrata,* we also find four cortical layers; but here it is especially the cellular envelope that enlarges, while the cork substance and the fibrous layer remain, as in general, quite undeveloped, and here, especially at the base of the trees, the bark is often more than twice as thick as the ligneous body. From these few instances we already see that the increase of the bark in thickness, even in cultivated plants nearly related to each other, may consist of the predominating development of quite different cortical layers.

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* *Formation du Liège.—L'Institut,* No. 192.

The bark of the birch is well known from its peculiar structure and its various colours. The young annual branches of this tree also possess an epidermis, which is covered with fine hairs: under this is situated a small layer of tabular cells, which represents the cork layer, and directly covers the cellular envelope. This cellular layer appears at the surface as soon as the epidermis falls off (in the second or third year); the single cells then become brown, and new layers of cells are deposited on the interior surface of this cellular mass. This mass now forms the well-known birch bark, which consists of thin white lamellæ, which we can peel off one after the other. Mohl proposes to give this cellular mass the name of *Periderma*, while the external layer is known by the name of *Epidermis*.

If we examine the bark from the stem of an old birch we find that it consists of a great number of brown layers, which, as in the leaves of a book, lie one over the other, and are very easily stript off. They are clothed on both surfaces with a white covering, which consists of very thin-sided colourless diametrically deposited cells, which are rather less compressed than those of the brown layer, where the cells are very thick-sided and filled with a brown substance. From the eighth to the tenth year there is for the first time alternately developed in the birch along with every layer of the brown cork tissue, at the same time a white layer, which consists of larger and more delicate cells; until this period the formation of new layers only takes place on the one surface of the Periderma. The white and the brown substance of the bark of the birch seem to be more distinct masses than those in the cork, where the borders of each layer may also be distinguished by their different colours. (See the anatomical difference of these layers in the figure which Link has given in his *Icon. Anat. Bot.* Tab. vi. fig. 13.)

Very remarkable is the difference between the cork substance of the cork-oak and the brown whitish layers of the birch bark, since these remain for a long time attached to the stem without cracking, and gradually peel off, while the cork substance splits and falls off. The inner layers of the birch bark consist of the cellular envelope and the layer of liber: the intermediate parenchymatous cells are very thick-sided. (See the figures of the development of the birch bark which Link has given in the *Icon. Anat. Bot.* Tab. vi. fig. 12, 14, and 15.)

In the very thick bark of old birch stems the afore-mentioned regularity in the position of the brown and white layers is not observable; but the increase in thickness takes place here and there in a higher or lower degree, by which the previous perfectly regular concentric laminæ are bent and torn in various ways.

We have already mentioned those cases which show that the distinct development of the bark consists sometimes in the thickening of the cork substance, at others in the thickening of the cellular envelope; there are however many cases in which the great development of the bark substance consists chiefly in the development of the layer of liber; we may cite for instance the beech (*Fagus sylvatica*). In this tree the bark almost always remains even; the cellular envelope here always remains very small, even when the bark has become of considerable thickness.

The bark also of the plane-tree (*Platanus occidentalis*) which is found in this country must also be specially mentioned. It exhibits the same structure as the bark of the beech, remaining, however, in this state only from the eighth to the tenth year. About this time there forms in the layer of liber, *i. e.* only at some places, a delicate layer of tabular cells which agree exactly with that of the periderma. This new layer of periderma is so situated that a part of the bark substance is completely separated by it, which then gradually dries, and after gradual disunion, actually falls off. These new formations of new layers of periderma are repeated, and thus follows the continual delamination, by which the tree still retains a very even bark. The great scales of bark, which fall off, consist, however, of the cellular envelope, and of a portion of the substance of the liber. The scales of the bark in *Prunus*, *Pyrus*, *Cratægus*, *Quercus Robur*, *Tilia europæa*, &c., are said to originate in the same manner as in the plane tree. Mohl, with other botanists, distinguishes these thick inner layers of the bark of the cork, which are formed in quite a different manner, and calls the inner layer the rugose bark (*rhytidoma*, from ῥυτις a wrinkle).

The results of these observations are, that the origin of the scales of the surface of the bark of dicotyledonous plants is not to be sought for in a desiccation of the bark layers, and in a mechanical splitting of them, but that it depends on the later development of distinct cellular layers, which disunite the single bark scales, or prepare for their disunion, or even themselves form the scales.

Upon the whole, we may suppose two main differences in the later development of the cellular tissue of the bark; in the first case the layers are developed outside the cellular envelope, and in the other, the becoming thicker arises from the development of a stratum of cells under the cellular layer; in the first case it is generally cork substance which is formed in the second bark (*rhytidoma*).

There are besides a number of plants in which a new layer of liber is annually formed, while the old layer dies away and peels off, for instance *Vitis vinifera*, *Lonicera Caprifolium*.

The bark of dicotyledons consists therefore, as has been demonstrated in the cases specially examined by Mohl, of three distinct layers, of very different structure, besides the epidermis. The exterior stratum of cells, which in many cases change into a thick corky substance, is called by Mohl the cork layer, *stratum suberosum seu phlœum*. Link* calls this layer *Epiphlœum*, outer rind (*oberrinde*); while he designates the intermediate rind *Mesophlœum* and the inner rind *Endophlœum*. The latter may evidently be compared with the layer of liber of other botanists, and the intermediate rind with the green cellular layer, the so-called cortical pith of many botanists.

Mohl † has also published some very interesting observations on the occurrence of suberose tissue in the stems of monocotyledons. Link and Dutrochet have also in their late works before cited, admitted the occurrence of the suberose tissue in the rhizoma of *Tamus Elephantipes*. According to Mohl's microscopical observations it appears that the brown layer of cork in *Tamus Elephantipes* perfectly agrees in its structure with the cork of dicotyledonous trees. The layer of cork on the basis of the stem consists only of a few layers of tabular cells, which form regular rows perpendicular to the surface of the stem. The exterior layers are brown, and have died off; the inner layer situated near to the rind is full of sap, colourless or yellowish.

The thick layer of cork which surrounds the convex part of the stem is composed in the same manner as the cork of the cork-oak, of thin-sided cells which form regular rows perpendicular to the basis of the rind, etc. A distinction between the rind and the cork can only be made in so far that the rind is living, whereas the cork on the contrary is dry and dead;

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* *Phil. Bot.*, p. 282.

† Observations on the Rhizoma of *Tamus Elephantipes* L.—*Tübingen*, 1836.

the cork does not consist here, as in the dicotyledons, of a distinct layer, but rather of the layers of rind which have died off.

Many excellent memoirs have again appeared upon the structure and design of the peculiar formations of rind, which are now known under the name of lenticular glands. Mohl * has enlarged his former observations on this subject, and has especially noticed the relation of the lenticular glands to the different layers of rind. The lenticular glands are evident on branches of even one year's growth beneath the uninjured epidermis; at a later period, sometimes towards the end of the first year, at other times after some years, the epidermis over the lenticular glands splits open in a longitudinal direction, and the lenticular glands then make their appearance as little warts. Subsequently they extend, and they then appear as diagonal stripes; where however the rind is thrown off, the lenticular glands also fall off. The lenticular gland, says Mohl, lies between the epidermis and the green parenchyma of the rind, and consists of greenish or colourless cells, (sometimes it has a different colour, as for instance, a yellow in *Berberis*, and a red one in *Sambucus nigra*) which lie in rows, having a position perpendicular to the axis of the branch, are for the most part smaller than the cells of the green parenchyma of the rind, and unite towards the interior with it. In many plants the cork layer of the rind, or its exterior parenchyma, is said to take a collateral part in the formation of the lenticular glands, so that it consists, properly speaking, of two layers, that is to say, of one belonging to the green parenchyma of the rind, and of one which consists of the exterior parenchyma of the rind, or combines with it. Hence, as well as from various other circumstances, Mohl places the formation of the lenticular glands parallel with the production of cork; nay, he supposes that the lenticular gland is a partial cork formation, which owes its existence to the concretion of the inner parenchyma of the rind.

For my own part I cannot agree with these views. Observations on this subject have shown me that the lenticular gland always consists in a concretion of the green layer of rind, and that this concretion is only surrounded by the exterior parenchyma of the rind; it is true, however, that there also takes place a disjunction in the parenchyma, which forms the exterior, and almost always reflexed margins of this enveloping brown layer of rind. The cells of the lenticular glands, which lie exactly in the middle, and which are distinguished from all others by their length, generally lose by degrees their green colouring, and at last become quite white, as the green contents gradually disappear. These middle cells stand with their extended longitudinal axis quite horizontal; whereas the cells of the lenticular gland, which form its exterior layers, generally retain not only their usual form, but also more or less their green colouring. If the entire formation gradually dries, its cellular membranes become more or less coloured; and in this colouring only has the tissue of the lenticular glands any resemblance to the formation of cork.

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* Observations on the Lenticular Glands.—*Tübingen*, 1836, 4to.

Mohl once more notices in this memoir the opinion which De Candolle has diffused so generally, that the lenticular glands were to be considered as it were root-buds, an opinion which we find in almost all the recent popular writings on vegetable physiology, although this position ought long since to have been abandoned. Unger also, in his very interesting dissertation on the design of lenticular glands*, states that these organs only occasionally stand in connection with the cortex; but they are not in any respect, according to Unger, "especially the exterior flat-pressed cells of the cortex only, *i.e.*, those which are united by a gelatinous mass (*materia intercellularis*) to a kind of integument (*i. e.* the external layers of the cortex) those which take any part in this metamorphosis," but the whole formation proceeds from the green layer of cortex, and breaks right through the external integument, just as Unger has correctly figured it in the above-mentioned memoir. Unger thinks that the first momentum of the formation of the lenticular glands is a concretion of the wide-pressed cells of the exterior layer of cortex. The concretion begins with the increase in size of the single cells; the increase in size causes a loosening of the concretion, and its final consequence is a complete separation. A nominal increase of cells is said to take place from the intercellular matter (!), and in this may principally be the proximate cause of the bursting of the upper layers of cells. Unger has very well observed that the cells which form the interior of the lenticular gland separate from one another, and seem as it were to make themselves independent. (Where then in this case has the intercellular matter remained, which is said to inclose these cells?) When the concrete masses are very great, and do not pulverize, they form great warts, such as we can point out on *Euonymus verrucosus* and others.

Unger enumerates various other vegetable formations, where he recognised an analogue to the formation of lenticular glands, in order perhaps in this way to be able to unravel their real design. In the first place are mentioned as such analogous formations, those remarkable organs which Von Martius discovered on the stems of the tree ferns, and of which mention was made in our Year's Report for 1834, *in which I already suggested that it was possible to explain the cells of these organs as imperfect nuclei* [?], (Brutkörner.) In the Lichens it is the *Sorediæ*, and in the *Jungermanniæ* the leaves bearing the reproductive granulations, which are regarded as formations analogous to the lenticular glands of the more perfect plants. "The design of the lenticular glands evinces itself," says Unger, "undoubtedly in the clearest manner in the formation of the imperfect buds of the *Jungermanniæ*, and hence we might form the supposition of explaining the lenticular glands as efforts to continue on the cortex of dicotyledons the formation of imperfect buds." Unger however thinks that a far greater design lies at the bottom of all this; he observed that the lenticular glands develop themselves on young shoots of *Prunus Padus* and *Syringa vulgaris* exactly at those places where the stomata rarely occur, and therefore the lenticular gland may stand in some way in connection with the respiratory process; nay, he would even consider them as obliterated respiratory organs. I must also express a similar opinion as to the design of the lenticular glands; I consider them not as obliterated respiratory organs, but as formations, by means of which an open communication is made, intermediate between the exterior air and the intercellular passages of the green layer of cortex. In this latter tissue intercellular passages are very frequent; but the firm combination of the cells in the exterior layers of cortex do not allow in the old state of the plant of any uninterrupted communication.

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* *Flora* of 1836, pp. 577 to 604.

Link* also contends that the lenticular glands belong to the cortical formation, that the temporary roots, on the contrary, originate from subjacent wood; yet it cannot be denied that they break out for the most part near to these warts, as also do the shoots.

Very interesting is an observation of Eudes-Deslongchamps† on the effect which the circular decortication produces upon the vegetation of a tree; similar experiments it is true have been previously made with the same results; but the present one of Eudes-Deslongchamps, which was performed on a beech, has been observed very carefully. The wound of the cortex which went round the whole circumference of the stem was nearly a foot wide, and the vigorous tree seemed not to suffer in the least by it. On the surface of the decorticated wood were to be seen many irregular exudations which had a similar appearance to the cortex. The upper edge of the wound exhibited towards the end of the summer a large swelling, while that of the lower edge of the wound had considerably diminished. In the next year the leaves developed themselves earlier on this tree than on such as had not been wounded. In the beginning the tree was still very strong, but in the course of the summer it shrunk, the leaves remained small, and the development of the shoots was very inconsiderable. The exudations on the surface of the decorticated ligneous body became drier, and in the third year were quite dry. In the beginning of the third year the tree once more shot forth, but the leaves remained small, etc. In the beginning of the fourth year the tree was dead. I have made the same observation on the hardy stem of an alder tree, which also died in the fourth year, but did not exhibit any exudations on the cleansed surface of the ligneous body, which seems chiefly to take place when the decortication has been performed very late, for instance in July*.

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* *Elem. Phil. Bot.*, p. 281.

† *Effets de la décortication circulaire sur un Hètre.—L'Institut de* 1836, p. 314.

Dutrochet† has published some new observations on the growth of coniferous stems; the notices, however, on this subject which we have seen in the journal cited are too short for us to be able to judge of it with any certainty. We hope that Dutrochet will soon describe this interesting subject more in detail.

Henslow‡ has described two cases in which the dead ligneous bodies of dicotyledons have been gradually inclosed by new annual rings, similar to those cases which have been described by Du Petit-Thouars and Lindley. In one of the cases described, namely, in the stem of a poplar, it was only one half of the surface of the stem, which had probably died from decortication, and the ligneous layers of the next annual ring had gradually deposited themselves laterally over the decorticated place, so that so soon as the fifth year the wound was closed, and the new ligneous ring again surrounded the whole stem. Cases of this sort are however extremely frequent, especially in willows, when in trimming, some branches are cut off, the ligneous body of which is then covered with new ligneous layers by a side branch.

Some new observations of Giron de Buzareingues § on the composition of the young ligneous layer have been published, the results of which I cannot exactly determine; we shall however notice this more fully after the publication of the entire memoir.

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* A notice of Mr. Nevin's recent experiments on the same subject was given in our last volume, p. 553.—Edit.

† *Accroissement en diamètre du* Pinus picea.—*L'Institut de* 1836, p. 427.

‡ On the disunion of contiguous Layers in the Wood of Exogenous Trees. Jardine's, Selby's, and Johnston's Magazine of Zoology and Botany. London, 1836, i. p. 32.

§ *Mém. sur l'accroissement en grosseur des Exogènes.—Compt. Rendues,* 1836.

Corda* has published a general treatise on the stem of plants: "the work," says the author, "was written in the year 1833, and laid before the Royal Academy of Sciences of Berlin in the beginning of 1834. It originated from Mohl's splendid work on palms, and from the truths disclosed in this, compared with my previously (!) made observations." Corda received from the Royal Society of Berlin the honourable request to demonstrate how, and in what manner, palms and the plants related to them grew†. In order to solve this question Corda proposed to himself a series of problems, which he has endeavoured to answer one after the other in the present memoir. For the solution of the first question, whether the externally evident formations and anomalies of the stem are continued towards the interior, or whether and how the inner condition exercises any influence on the formation of the external form; Corda treats of the growth of *Coniferæ*, *Cycadeæ*, and ferns, etc. He compares the ligneous body in very different anamorphoses of the dicotyledonous stem, and also finds that it agrees in structure. Corda does well in remarking how in *Cactus Rogeni*‡ a ligneous cylinder originates from a blending of the ligneous bundles, similar to that in the stems of arborescent ferns, which was fully mentioned in our last year's report. Corda thinks he is able to say of *Pelargonium zonale*, that the ligneous body of the youngest branches is similarly constructed to that of the herbaceous ferns, that of the older branches to that of firs, and that of the basis of the stem to that of deciduous trees. I would however here observe, that the ligneous body in young coniferous stems, or in young branches of these plants, is circumstanced exactly as in the young branches of *Pelargonium*; for the single ligneous bundles stand in both perfectly separate. Corda, after having referred in *Dracæna*, *Elais* and other palms to a ligneous cylinder similar to that in *Coniferæ*, formed by a blending of the ends of the ligneous bundles, answers the first question negatively.

The second question, whether all forms of vegetation can occur in one and the same plant, Corda answers quite as we should expect, and shows that in all plants a peripherical and terminal vegetation takes place. This however was known to Mohl, since he represented the *vegetatio terminalis* as different from the *vegetatio peripherica*; and he took these ideas quite in a different sense from that indicated by Corda; Mohl appeared merely to err in so far as he ascribed only a *vegetatio terminalis* to the *Cycadeæ*, whilst they are circumstanced exactly as the *Coniferæ*.

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* On the structure of the vegetable Stem. Prague, 1836.

† In our last volume, p. 553, will be found a notice of some experiments, to ascertain the internal structure of the wood of palms, by Mr. Gardner. —Edit.

‡ It takes place in all woody *Cacteæ*. Meyen.

The third question, what relation does the shoot of one year bear to the stem of many years' growth, and the fourth question, whether all annual and perennial plants of the same class grow similarly, have found their answer in the preceding ones.

The fifth question, whether all exogenous or peripherically growing plants push forth the new-formed parts like new plants, between the liber and the ligneous layer of the older, is treated very fully, and the answer is: "All plants growing peripherically push forth their new parts in a fissure of the liber, and never between the liber and the wood; the side of the liber (the inner side of the fissure) produces new liber; whilst a part of the old liber becomes actually part of the wood, and produces new wood on its exterior side." In regard to this statement, I would refer only to the demonstrations of some celebrated phytotomists, that the structure of the cells of the liber and that of the cells of the wood is very different, and that hence alone this position falls to the ground, whilst it can be positively refuted in various other ways.

The sixth question, whether the young stem or part thereof grows differently from the old; and the seventh, whether and how the terminal vegetation of Mohl exists and goes on, are also answered in the first replies; yet the eighth question, whether a consistent and universally applicable distinction of the vegetation of monocotyledonous and dicotyledonous plants can be demonstrated, is answered negatively.

The ninth question, How do mosses, lichens, algæ, and fungi grow, and can the above questions be in any degree applied to them, has also been previously answered in part; and Corda remarks, that every new cell is formed at the exterior surface of the older ones, which, however, as I have in the beginning of the present memoir explained, is not correct. Finally, Corda has formed thirty conclusions, which he offers to physiologists for their opinion and critical examination. I will here only mention those which differ from the present prevalent views, as,

1. All wood must be formed in a parenchymatous tissue, which tissue is separated, by means of the originating ligneous mass, into two parts, at first alike, subsequently of an opposite nature, the interior one of which we call wood, the external one cortex.

2. All wood consists of a combination of liber and vessels which belong to the air-productive system. The liber is the osseous system, the spiral and dotted vessels are the tracheal system of the vegetable organism.

3. The liber is always formed earlier than the vessels.

16. It has also been supposed and taught, that the wood of *Coniferæ* consists in the older annual rings entirely of vessels; however, we find in each, even in the oldest annual ring, a very thin layer of liber, which has been overlooked on account of its thinness.

19. Liber and wood independently, and the combination of both parts in their yet soft condition, is called alburnum.

20. There also originates, with every new ligneous layer, a new thin stratum of parenchyma, at the exterior surface of the new liber and interior side of the old, which at first is full of sap, and subsequently passes over into suberose tissue, and imparts to the dead rind the brown colour; whence we also find in the cortex layers formed, which consist alternately of liber and cork, &c.

Link* has published a series of excellent observations on the uninterrupted and interrupted growth of wood in the stem, as also on the growth of the leaves and root, which conclude with a treatise on the anamorphoses of the stem and of the root, which forms one of the most excellent parts of this new edition of the *Philosophia Botanica*. This subject has never been treated of so specially and with such a profound knowledge.

In a beautiful work also of G. Meneghini† several species of monocotyledonous stems are anatomically characterized with great accuracy, and explained by figures. I must however content myself with calling attention to these treatises, as their contents are too voluminous to be given in this place. I will here only give in full the results from this work of Meneghini, which he himself has given in p. 77—86.

"Two ascertained facts," says the author, "in the vital activity of monocotyledons, led me, in the observation of their structure, to the conclusions, 1st, that, where certain currents of vital saps exist, there also are vascular fibres formed; and that, 2ndly, certain curvatures are impressed on the interior vascular fibres, by means of the displacements of the appendages of the stem from which these fibres are suspended."

Meneghini proposed to himself for solution the following problems:

1. Which is the arrangement of the vascular fibres that is common to all monocotyledonous stems?

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* *Elem. Phil. Bot.* Ed. alt., p. 288—299.

† *Ricerche sulla Struttura del Caule nelle Piante Monocotiledoni.* Padua, 1836, fol. min.

In every monocotyledonous plant there may be detached from the basis of each leaf a greater or smaller number of vascular bundles, which run, in their manifold, slanting and extended course, near to any point of the axis, and from thence, separating from one another towards the horizontal side, continue to descend right and left with various contortions, continually returning in a slanting direction towards the periphery. They end by taking a perpendicular course, which allows of their condensing themselves into a peripherical zone of various firmness and thickness, in which, however, the same order of superposition is always retained; in return for which the most recent bundles are always placed upon the others.

2. What invariable laws govern this general arrangement?

Since each leaf, at its origin from the stalk, comes forth with a circular basis in the middle of the bud, and is carried in its growth like a spiral line to a higher and peripherical spot, as it continues to surround the whole circumference of the stalk; and since, in consequence, it can only embrace a gradually smaller arc, it necessarily must follow that the lower course of each vascular bundle represents the place which it occupied during the time when the leaf was still inclosed in the bud; and the upper organized course, during the progression of the leaf itself, gradually depends on the conditions, as modifications of an invariable law, which are observed in this process.

3. To what particular modifications can the general and constant type of this organization be subjected?

The bud, which gives origin to new individuals, ceases to develop itself as soon as it has arrived at a certain limit, or continues in an indefinite manner its progressive development. The limit of the first is fixed by the terminal position of the inflorescence, which in the second is an axillary one. The florescent part of the stalk is held fixed by the upper courses of the vascular fibres, and enjoys therefore the conditions inherent in them, which are those of endogenitiveness. The centripetal or centrifugal characters of the inflorescence itself cause in the structure of the inflorescent part only a slight modification, which is still less evident in the lower parts of the stalk, and is connected with the period of the development of the axillary bud, whence originate the inflorescent branches. The separation and displacement of the leaves is effected as it were in a single or in two spiral lines, which run round at the same time in opposite directions. The greater or less perpendicular distance, and the greater or less lateral divergence of the leaves, (setting aside the relation of the basis to the circumference of the stalk,) uniformly retained or gradually diminished, and the constant order of their succession round the stem, are properties which modify by their change these two general cases. The greater the perpendicular distance of the leaves is, the less is the slanting course of the vascular bundle. If the proportion of the basis of the leaf to the periphery of the stalk has remained, then the horizontal inclination of the fibre only is uniformly and constantly impressed at the very time of the displacement of the leaves. But when the insertion is limited to a single arc, this inclination becomes by so much the greater as that is lessened; since the fibres must deviate, some to the right, others to the left, while they remain diffused with the lower courses over the whole periphery. The shorter however the insertion is, and the smaller the vertical distance, the less is the lateral divergence of the leaves, which even goes so far as to imitate a whorl, and even to form one. If, on the contrary, the original proportion is preserved, the lateral divergence depends only on the perpendicular distance, and then the distichous arrangement often remains, which is the natural one in the monocotyledons. Thus it happens in the case of the double spiral lines, and the changes of this peculiarity alone afford proof of the diversity of the structure from the continuous stem to the articulated, from the solid culm to the tubular.

4. What part do the branches take in the structure and growth of the stalk?

The branches that constitute the axillary inflorescence, and which originated and grew at the same time with the leaves, have their vascular bundles also in the same direction, and contribute very little to the growth of the common stem. The scanty data which science possesses respecting the ramifications of *Pandanus* justify the supposition that they have the same origin as the inflorescence. If, however, a fresh system succeeds the first, on account of the terminal inflorescence, whether it be that it proceeds from a single branch or from several kinds around the same horizontal surface, it inclines itself upon the old one, and there forms all around a layer, which may be compared with the annual vegetation in dicotyledonous stems.

Independent of this, branches may originate on the already developed parts of the stem, in regard to which two different properties may be noticed. For it may happen that the vegetation of the main axis is completed or interrupted, and the productions of these branches externally belong to the fibrous ligneous body of the old stem; or that this continuous growing and the new productions combine and interlace with those of the branches. These several kinds of ramification also contribute in various ways to the enlargement of the stem. It must be entirely ascribed to this, if they follow the already completed vegetation of the main axis; they only take a small part in it, when they rise from the inflorescence to the angle of the leaves then present.

We must establish similar distinctions with regard to the roots; for when they hang down from the basis of the stem, their vascular bundles are continued; when, on the other hand, they break out from the lateral parts, they send forth their formations of vascular bundles between the ligneous body and the exterior layer of cortex.

5. What new distinctive characters are established by means of these organic properties between the stems of the two great classes of phanerogamic vascular plants?

A parenchymatous cellular tissue, through which vascular bundles run longitudinally, forms the organization of the stem of a dicotyledonous, as well as of a monocotyledonous plant in the first periods of life. The internal structure and the relative arrangement of these fibres must remain subjects of comparison. As to the structure, Mohl proved that it is the same in both classes. We find in the monocotyledons as in the dicotyledons, on the inner side of the vascular bundle, which is directed towards the axis of the stem, a chain of vessels, which form a part of what is called by Hull *corona*; by botanists of the present day medullary sheath in the wood of dicotyledons. The exterior side of the bundle is on the other hand occupied by prosenchymatous cells, and these are those which in dicotyledons form the liber. Lastly, between the inner ligneous layers and the exterior bundles of liber is another bundle of distinct vessels, which in its proportion is variable, and at times is even wanting in dicotyledons. In this the indicated structure is still the same in the whole course of the single ligneous bundles; different, however, in the various progressions of its course in the stems of monocotyledons.

The direction also of the ligneous bundles in these plants is different at different points of the stem, while in the dicotyledons they descend perpendicularly, and always parallel with one another. Great diversity may however be remarked during the progress of vegetation. In the monocotyledons the constant isolation of the fibres allows of every one of them repeating, with each fibre, in their two courses, the same peculiarity reversed; the more recent the upper course, the nearer is it placed to the axis of the stem, and the lower to the periphery.

In the greater number of dicotyledons the isolation of the vascular bundles is retained, and consequently the integrity of the original proportions only up to a certain period. According to the genera each bundle ends more or less quickly, in such a manner that they arrange themselves with their sides upon each other, and the circle of vascular bundles becomes now a firm tube, which is traversed solely by radial laminæ, formed by series of horizontal cells. The new bundles, which continue to be organized, after this tube is closed, increase its size, so long as the vegetation of the year continues. If, therefore, we divide the apex of a young germ, we see that the vascular bundles which pass into the leaves constantly proceed from the internal layer of wood. These fibrous vascular formations were distinguished by Girou de Buzareingues according as they belonged to the leaves of the young germ, or to the buds which are developed in the angles of those leaves. He showed that these buds, notwithstanding their apparently more interior position to that of the leaves, rise out of the summit of a more projecting medullary production; and that their vascular bundles, allowing a passage to those of the leaves, descend on the exterior side of the first fibrous body. Both of the two zones, therefore, are formed by several small concentric layers; those of the external ring are always arranged so that those that are for the most part peripherical belong to the lowest buds; the most interior, on the contrary, to the highest ones. Thus it is also with the central zone in annual plants in the shoots of the *Rhizocarpæ*, and for the greater part also in the new shoots of trees; but in some of the latter the arrangement is just the reverse, by which the fibres of the upper leaves are exteriorly over the others, and those, above all, nearest to the central point, are those which belong to the inferior leaves. The column of pith in this case takes an obverse conical form, while it has that of a straight cone in the first instance. Mohl does not distinguish these two cases, nor the two zones as distinct and exclusive productions of the leaves and of the buds. He concedes that in the apex the recent fibres are organized in the interior of the older ones; and he brings forward various data in order to overthrow Alph De Candolle's explanation of the *crampons* (?) (*Wurtzel-fassens*), which would preserve, on that ground only, to the monocotyledons the name of Endogens. But in the inferior parts, he found in the dicotyledons a nature so different, that it would serve as the most certain character to distinguish them from the monocotyledons. He saw constantly that the upper bundles between the vascular part and the prosenchymatous part always force the others inward, and thus isolate the one from the others. Each new fibre in this manner occupies the place of one of the old ones, itself in turn to undergo the same lot. Thus it happens that the prosenchymatous fibres, which are continually forced back to the periphery, form the liber; and the vascular fibres, which constantly deposit themselves on the exterior of similar older ones, form the wood; this is the cause why he has named these two parts of each bundle wood and liber, which always remain in the monocotyledons as they were first formed, undivided and unchanged.

Dutrochet's beautiful observations on the interior formation of the ligneous bundle coincide entirely with Mohl's discovery. He observed and figured in *Clematis Vitalba* this bipartition of each bundle, which separating itself from its parts, leaves the place clear for the one coming over it. And if he did not notice which of the elementary parts it was which constantly separated from the others, yet it did not escape him, that the change from the very beginning is divided into two layers, which become organized at the same time, the interior one into wood, the exterior one into liber.

Although we cannot at first distinguish two separate zones in the shoots of some species of *Smilax*, yet it has been demonstrated that the fibrous formations of the leaves occupy the central point, and those of the bud the periphery; just as Girou de Buzareingues found in the dicotyledons. But in this the woody part alone helps to form those two systems, while the part of the liber is forced back to the periphery; in *Smilax*, however, and in other monocotyledons, the fibres retain their perfect integrity. It must therefore be remarked, that those for the most part peripherical proceed from prosenchymatous tissue alone, as Mirbel has figured it, and as we can see it in diagonal sections on the side opposed to that of the bud.

The separation of the bundles which is mentioned by Dutrochet, and described by Mohl, in the dicotyledons, answers as a very convenient character for distinguishing the doubtful cases. Thus for instance, in the stem of *Piper*, where some vascular bundles remain in the parenchyma, even when a ligneous zone is organized near to the periphery, by which it is inclosed, it is provided with medullary rays, and increases annually by new layers. These bundles do not increase in number; but if we examine them at different heights, we find them in less number at the base and at the end, but in greater number in the intermediate parts, as was also observed by Meyer. This is most easy to examine in those species of *Piper* where the stem is herbaceous and fibrous, &c.

From a combined consideration of these observations we obtain the following positions:—

The bipartition of the vascular bundles by means of the intermediate formation of new fibrous vascular bundles, and the subsequent increase of the stem in breadth, belongs solely to the dicotyledons. On the other hand, the increase in thickness which is formed by the superposition exteriorly of new fibrous layers on those already present, is quite independent of the medullary rays, and belongs in common to the monocotyledons. In the dicotyledons the fibres immediately cease to stand in relation to the leaves to which they belong, and never remain connected by their rudiments. Each fibre loses very soon its own individuality, as it divides itself into its elements, which then form a part of the two systems, &c. But in the monocotyledons each fibre retains permanently and invariably its individuality. It remains independent of the leaf, and follows all its movements as long as it has life. When that is destroyed, it remains dependent on the cicatrix which has been left on the exterior surface, and continues permanently in connexion with it, as it extends itself gradually quite through the new productions, which are constantly augmenting the thickness of the stem.

6. What is to be added to the discoveries of Mohl with respect to vegetable anatomy?

Mohl observed the course of the ligneous bundles in the various stems of palms, by determining their deviations in the vertical direction. He showed that all vascular bundles which belong to a *flabellum* (?) (*Wedel*), while it occupies the outer end, forms an elongated cone on the outer surface of the stem, the apex of which opens at the development of the new leaf, as the vascular bundles now diverge from one another to the periphery, from whence they intersect with the latest formed ones, &c. In order to inquire into the cause of this circumstance, we must follow the leaves in their successive changes of place, apply it to the changes of place which are imparted to the fibres, and we must recognise the constant relation of the division of the external organs to those of the inner vascular bundles. We must, above all, distinguish the cases, in which the partition of the leaf-stalk retains its original relations to the periphery of the stem, from those of an uncommon swelling of the latter, by which the basis of the leaf-stalk is reduced to one, more or less restricted, arc. Causes of this modification, if we take it well into consideration, explain all differences which we may find in the structure of stems, &c. In order, however, to complete their history, to determine their degree of similarity, which was only indicated by Mohl, we must distinguish in each stem the florescent part from the rest, which very frequently is reduced to the smallest dimensions. By means of this distinction only are we able to explain the structure of the stem, which Mohl called tubular, because it is peculiar to the palms of the genus *Calamus*, which cannot be compared, as regards the inner structure, with any other plant of that family, except at its lower part, which serves as the common axis, from whence these new germs proceed.

Mohl has said nothing respecting the structure of the perennial shoots, in which Mirbel thought he was able to observe a double vegetation. In fact we must distinguish the fibrous productions of the leaves in them from those of the buds. Both are circumstanced similarly to the upper course of the vascular bundles of all other monocotyledonous stems. At the basis of the chief axes only of the root-stalk, and of the secondary one at the angles of the leaves, are found the under courses of these fibres, and the constant separation which such courses invariably preserve opposed to the first. Then only, when the leaves continue to surround the stem in its entire circumference, or when they are rolled together in more than one circle, and when at the same time the one is brought to some distance from the other, can it happen that the fibres at the insertion of the leaf become peripherical; although they are all inclined towards the same direction, as in the *Juncaceæ*, *Cyperaceæ*, &c. This peculiarity is still more evident in the culms, on account of the double spiral lines which regulate the motions of the leaves. Moldenhawer had already taught that the bundles of the older leaves pierced deeper into the fibrous body of the culms; but the structure and the origin of the nodi remained hidden. Led by the above observations, I succeeded by elucidating this case, which is the most difficult of all, in giving a plainer explanation of this principle, by which in the monocotyledons the displacements of the external organs may be regarded as the cause of the inner arrangement of the vascular fibres.

One of the most influential works of last year is that of Link *, in which he intends to publish a great series of phytotomical plates. In the preface to this work, Link says that the anatomy of the human body has made great progress since learned men have begun to cause drawings to be made by skilful artists of what they have seen. This example Link intends to follow, and in this way all those who are not able to make microscopical observations themselves, will still have the means of teaching themselves and others; for drawings are quite as necessary for the study of vegetable physiology as for the study of anatomy. The great success which this work has already experienced from its excessively moderate price proves its usefulness. From the numerous beautiful and interesting figures, we will only notice a few, which must attract the attention of all botanists; as the very successful representation of the interlacement of the ligneous bundles in the internodia of monocotyledons. Tab. ii. fig. 6. exhibits the inward growing and interlacement of the ligneous bundles, which descend from the branch or bud of *Saccharum officinarum*. The germinating plants of various monocotyledons, the diagonal sections from various anamorphoses of the monocotyledonous stem, the figures of the thickened cellular masses from the bark of the birch, &c. exhibit at the same time much that is new and which had never before been published.

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* *Icones anatomico-botanicæ ad illustranda elementa philosophiæ botanicæ. Fasc.* I. *cum Tabulis lithographicis* viii. Berolini 1837. fol. Latin and German.

I have also a dissertation of my own to mention, sent in as an answer to the prize question made by the Teyler's Society of Haarlem on the 1st of January 1834, and which appeared at the end of last year as the 22nd part of the *Verhandelingen witgegeven door Teyler's Tweede Genootschap* (Haarlem, 1836, 4to.). Although this work was not yet perfected for publication, yet I must return my thanks to the Society, as they have published on this occasion a great number of my microscopical, for the most part phytotomical, drawings, which were given with this publication on twenty quarto plates, and which it would have been difficult to have done in any other way. This dissertation has the title: "On the recent progress of the Anatomy and Physiology of Vegetables"; it was, however, written in 1834, and one part of the plates executed in 1833. I might recommend the plates of this memoir for use; for although made for the most part with an old English microscope, they might yet rank among some of the most correct which have hitherto appeared for vegetable anatomy. The new data which are contained in this memoir will be pretty completely found in the book which lately appeared in Berlin under the title: "New System of Vegetable Physiology*."

[To be continued.]

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* See the notice of this work, vol. ix. p. 481, of Phil. Mag., 1837.

XVI. *Analytical Development of Fresnel's Optical Theory of Crystals.* *By* J. J. SYLVESTER, *Member of St. John's College, Cambridge.*

[Continued from vol. xi. p. 541.]

ADDENDUM.

If in the equation of Prop. (6), viz.

$$\frac{(\cos\varpi)^2}{a^2-v^2}+\frac{(\cos\phi)^2}{b^2-v^2}+\frac{(\cos\psi)^2}{c^2-v^2}=v$$

we change a, b, c, v into $\frac{1}{a}\,\frac{1}{b}\,\frac{1}{c}\,\frac{1}{v}$, and consider v to be the length of a line drawn perpendicular to the plane

$$\cos\varpi\,.\,x+\cos\phi\,.\,y+\cos\psi\,.\,z=0,$$

the $=^{n}$ to the extremity thereof must be

$$\frac{a^2r^2(\cos\varpi)^2}{a^2-r^2}+\frac{b^2r^2(\cos\phi)^2}{b^2-r^2}+\frac{c^2r^2(\cos\psi)^2}{c^2-r^2}$$

when ϖ, ϕ, ψ denote the $<$es between the radius vector r, and the axes of x, y, z, so that the $=^{n}$ may be written

$$\frac{a^2x^2}{a^2-r^2}+\frac{b^2y^2}{b^2-r^2}+\frac{c^2z^2}{c^2-r^2}=0,$$

which has been found to be that of the wave-surface.

But we have seen that

$$v^2=c^2\left(\cos\left(\frac{\iota_{,}\pm\iota}{2}\right)\right)^2+a^2\left(\sin\left(\frac{\iota_{,}\pm\iota}{2}\right)\right)^2$$

$\therefore$ the $=^{n}$ to the wave surface may be written

$$\frac{1}{r^2}=\frac{\left(\cos\frac{\iota_{,}\pm\iota}{2}\right)^2}{c^2}+\frac{\left(\sin\frac{\iota_{,}\pm\iota}{2}\right)^2}{a^2}$$

where $\iota_{,}$ $\iota_{,,}$ denote the $<$es between the radius vector v and the two lines which would be the optic axes if a, b, c were changed into $\frac{1}{a}$, $\frac{1}{b}$, $\frac{1}{c}$ so that if e be the inclination of either to the mean axis of elasticity

$$\cos e=\sqrt{\left(\frac{\frac{1}{a^2}-\frac{1}{b^2}}{\frac{1}{a^2}-\frac{1}{c^2}}\right)}=\frac{c}{b}\sqrt{\left(\frac{a^2-b^2}{a^2-c^2}\right)}$$

$$\sin \varepsilon = \sqrt{\left(\frac{\frac{1}{b^2} - \frac{1}{c^2}}{\frac{1}{a^2} - \frac{1}{c^2}}\right)} = \frac{a}{b}\sqrt{\left(\frac{b^2 - c^2}{a^2 - c^2}\right)}$$

These lines I shall call by way of distinction the prime radii*.

Cor. (1.) If r_1 r_2 be the two values of r corresponding to the same values of $\iota_{,}$ $\iota_{,,}$ we have

$$\frac{1}{r_1^2} - \frac{1}{r_2^2} = \frac{1}{c^2}\left(\left(\cos\frac{\iota_{,} - \iota}{2}\right)^2 - \left(\cos\frac{\iota_{,} + \iota}{2}\right)^2\right)$$

$$+ \frac{1}{a^2} \cdot \left(\left(\sin\frac{\iota_{,} - \iota}{2}\right)^2 - \left(\sin\frac{\iota_{,} - \iota}{2}\right)^2\right)$$

$$= \left(\frac{1}{c^2} - \frac{1}{a^2}\right)\sin \iota_{,} \,.\, \sin \iota_{,,},$$

which proves the celebrated problem of *two rays* having a common direction in a crystal.

Cor. (2.) The intersection of any concentric sphere with the wave surface is formed by making r constant. Hence $\iota_{,} \pm \iota_{,,}$ becomes constant, and $\therefore$ $r\iota_{,} \pm r\iota_{,,} =$ constant. Hence the curve of intersection is the locus of points, the sum or difference of whose distances from two poles when measured by the arcs of great circles is constant; the poles being the points in which the prime radii pierce the sphere.

In three cases these spherico-ellipses or spherico-hyperbolæ become great circles:

1°. When $\iota_{,} \pm \iota =$ the angle between the two poles, in which case the curve of intersection is the great circle which comprises the two poles.

2°. When $\iota_{,} - \iota = 0$ when the locus is a great circle perpendicular to the former and bisecting the angle between the optic axes.

3°. When $\iota_{,} + \iota = 180$ when the locus is a great circle perpendicular to the two above, and bisecting the supplemental angle between the two axes.

Various other properties may be with the greatest simplicity deduced from the radio-angular equation. The hurry of the press leaves me time only to subjoin the following

Proposition.

"To find the inclination of the radius vector to the tangent plane, in terms of the angles which the radius vector makes with the prime radii."

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* Upon the authority of Professor Airy I have appropriated the term optic axes to the lines normal to the fronts of single velocity.

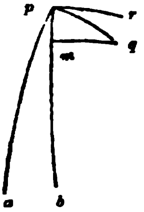
Let O be the centre of the wave-surface O A, O B the two prime radii, O P any radius vector. Let O P $= v$, P O A $= \iota_{,}$, P O B $= \iota_{,,}$, and let the inclination of the planes P O A, P O B $= \mu$

then $\dfrac{1}{r^2} = \dfrac{\left(\sin\dfrac{\iota_{,} + \iota}{2}\right)^2}{a^2} + \dfrac{\left(\cos\dfrac{\iota_{,} - \iota}{2}\right)^2}{c^2}$

(taking only the positive sign for the sake of brevity.)

Let O Q, O R be the two adjacent radii vectores, so assumed that

$$\text{Q O A} = \text{P O A} \quad \text{Q O B} = \text{P O B} + \delta\,\iota_{,,}$$
$$\text{R O B} = \text{P O B} \quad \text{R O A} = \text{P O A} + \delta\,\iota_{,}$$



and let p, q, r, a, b be the projections of P, Q, R, A, B on a sphere of which O is the centre, then it is clear that

$$q\,p\,a = 90^\circ \quad r\,p\,b = 90^\circ$$

draw $q\,m$ perpendicular to $p\,b$. then $p\,m = \delta\iota_{,,}$

and $\therefore\ p\,q = \dfrac{p\,m}{\sin p\,q\,m} = \dfrac{p\,m}{\sin a\,p\,b} = \dfrac{\delta\,\iota_{,,}}{\sin\mu}$

In like manner $p\,r = \dfrac{\delta\,\iota_{,}}{\sin\mu}$

Now the angle Q P O

$$= \tan^{-1}.\ \frac{r\,.\,\text{P O Q}}{\text{O Q} - \text{O P}} = \tan^{-1}.\ \frac{r\,.\,p\,q}{\dfrac{d\,.\,r}{d\,\iota_{,,}}\,.\,\delta\,\iota_{,,}}$$

Now $\dfrac{d\,.\,\dfrac{1}{r^2}}{d\,\iota_{,,}} = d_{\iota_{,,}}\left\{\left(\dfrac{1}{c^2} - \dfrac{1}{a^2}\right)\left(\cos\dfrac{\iota_{,}+\iota_{,,}}{2}\right)^2\right\}$

$$= -\left(\frac{1}{c^2} - \frac{1}{a^2}\right)\left(\sin\frac{\iota_{,} + \iota}{2}\right)\left(\cos\frac{\iota_{,} + \iota}{2}\right)$$

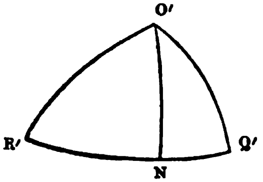
$$\therefore\ \frac{d\,r}{r\,d\,\iota_{,,}} = \tfrac{1}{4}\,r^2\,.\left(\frac{1}{c^2} - \frac{1}{a^2}\right)\sin\,(\iota_{,} + \iota)$$

$$\therefore \cot.\,\text{Q P O} = \frac{r^2}{4}\,.\,\left(\frac{1}{c^2} - \frac{1}{a^2}\right)\sin\,(\iota_{,} + \iota)\,.\,\sin\mu$$

In like manner

$$\cot.\,\text{R P O} = \frac{r^2}{4}\,.\,\left(\frac{1}{c^2} - \frac{1}{a^2}\right)\sin\,(\iota_{,} + \iota)\,.\,\sin\mu$$

$\therefore$ Q P O = R P O.



Also it is clear that $r\,p\,q = a\,p\,b = \mu$. And to find the inclination of O P to R P Q, we have only to describe a sphere of which P is the centre, and intersecting P Q, P R, P O in Q′, R′, O′.

Then P′ O′ Q′ $= \mu$, and O′ Q′ = O′ R′

$$= \cot^{-1}.\left\{\frac{r^2}{4}\left(\frac{1}{c^2}-\frac{1}{a^2}\right).\sin(\iota_{,}+\iota)\sin\mu.\right\}$$

Draw O′ N perpendicular to R′ Q′, then O′ N measures the inclination of the radius vector to the tangent plane *.

And Q′ O′ N $= \frac{\mu}{2}$

$$\therefore \cos\frac{\mu}{2} = \tan \text{O}'\text{N} . \cot \text{O}'\text{Q}'$$

$$\therefore \cot \text{O}'\text{N} = \frac{\cot . \text{O}'\text{Q}'}{\cos\frac{\mu}{2}}$$

and $\therefore \cot \text{O}'\text{N} = \frac{1}{2}r^2.\left(\frac{1}{a^2}-\frac{1}{c^2}\right)\sin\frac{\mu}{2}.\sin(\iota_{,}+\iota_{,,})$,

let A O B the angle between the optic axes $= 2\,e$, then by mere trigonometry

$$\sin\frac{\mu}{2} = \sqrt{\frac{\sin\left(e+\frac{\iota_{,}--\iota}{2}.\sin\left(e-\frac{\iota_{,}-\iota}{2}\right)\right.}{\sin\iota_{,}.\sin\iota_{,,}}}$$

$\therefore$ the tangent of the inclination between the radius vector and the normal

$$= \frac{1}{2}r^2.\left(\frac{1}{a^2}-\frac{1}{c^2}\right)\sin(\iota_{,}+\iota_{,,}).\sqrt{\frac{\sin\left(e+\frac{\iota_{,}-\iota_{,,}}{2}\right)\sin\left(e-\frac{\iota_{,}-\iota_{,,}}{2}\right)}{\sin\iota_{,}.\sin\iota_{,,}}}$$

Q. E. F.

In like manner the inclination between the same radius vector and the normal at the other point of the wave-surface pierced by it

$$= \frac{1}{2}(r_{,})^2\left(\frac{1}{a^2}-\frac{1}{c^2}\right)\sin(\iota_{,}-\iota_{,,}).\sqrt{\frac{\sin\left(e+\frac{\iota_{,}+\iota_{,,}}{2}\right)\sin\left(e-\frac{\iota_{,}+\iota_{,,}}{2}\right)}{\sin\iota_{,}.\sin\iota_{,,}}}$$

---

* O′ is the projection of the ray and R′ O′ of the tangent plane. Therefore O′ N being perpendicular to R′ Q′ represents their inclination.

We may, in the same way, find the inclination of the tangent plane to either of the prime radii, and to the plane which contains them both, in terms of $\iota_{,}$ and $\iota_{,,}$; the former by a remarkably elegant construction; but the final expressions do not present themselves under the same simple aspect.

If we call ϕ the angle between the ray and the front, we may still further reduce by substituting for r^2 its values in terms of $\iota_{,}\ \iota_{,,}$ and we shall obtain

$$\cot\phi = \frac{2\,(c^2 - a^2)}{c^2 \,.\, \tan\frac{\iota_{,} \mp \iota_{,,}}{2} + a^2 \cot\frac{\iota_{,} \mp \iota_{,,}}{2}}$$

$$\times \sqrt{\sin\left(c + \frac{\iota_{,} \pm \iota_{,,}}{2}\right) \sin\left(e - \frac{\iota_{,} \pm \iota_{,,}}{2}\right) . \operatorname{cosec}\iota_{,} \,.\, \operatorname{cosec}\iota_{,,}}$$

And if $\pi_{,}\ \pi_{,,}$ be the inclinations of the normal to the two prime radii, it may be shown that

$$\cos\pi_{,} = \cos\phi \sin\iota_{,} \mp \sin\phi \cos\iota_{,} \sin\frac{\mu}{2}$$

$$\cos\pi_{,,} = \cos\phi \sin\iota_{,,} \pm \sin\phi \cos\iota_{,,} \sin\frac{\mu}{2}$$

Cor. (1.) For uniaxal crystals $\frac{\mu}{2} = 90$ and $\iota_{,} \pm \iota_{,,}$, so that tan of the inclination of normal to radius vector

$$= r^2 \,.\left(\frac{1}{a^2} - \frac{1}{c^2}\right) \sin 2\,\theta \text{ for one point,}$$

and $= 0$ for the other.

Cor. (2.) For every point in the circular section which passes through the poles $\sin\frac{\mu}{2} = 0$, and for the other two circular sections $\iota_{,} \pm \iota_{,,} = 0$ or 180°.

∴ Every point in the three circular sections is an apse.

Cor. (3.) When a nearly $= c$ $\frac{1}{a^2} - \frac{1}{c^2}$ is very small; and ∴ the normal and radius vector very nearly coincide.

Cor. (4.) Referring to the last figure we see that O′ N′ bisects the angle R′ O′ Q′. Now R′ O, Q′ O are respectively perpendicular to the planes passing through O and the optic axes; and therefore the meridian plane as we may term it, i. e. the plane containing both the ray and the normal, always bisects the angle formed by the two planes drawn through the ray and the two optic axes.

Cor. (5.) When $\iota_{,}$ or $\iota_{,,} = 0$

$$.\ \iota_{,,} \text{ or } \iota_{,} = e$$

And $\therefore\ \phi$ assumes the form $\frac{0}{0}$, which indicates that the extremities of the four prime radii are singular points.

In concluding for the present it behoves me to state that one step has been omitted in the foregoing paper, viz. the actual performance of the eliminations which lead to the rectilinear equation to the wave-surface. But Mr. Archibald Smith's elegant and brief Memoir in the Cambridge Philosophical Transactions of last year leaves nothing to be desired further on that head.

That I have not exhibited it in its proper place (prop. 6.) arises only from my respect to the principle of literary property. With this important blank supplied the Analytical Theory may be pronounced to be complete.

For all errors and imperfections in what precedes my excuse must be press of time and a total want of the materials to be derived from consulting works of reference.

Since writing the above I have had an opportunity of reading the paper of our living Laplace inserted as part of the Third Supplement to his System of Rays in the Transactions of the Royal Irish Academy, in which the principal foregoing results are obtained by aid of a more refined and transcendental analysis.

The nature of the four singular points is there discussed and the existence of four circles of plane contact demonstrated.

The former may be very easily shown thus: when $\iota_{,}$ is very small $\iota_{,,} = 2\,e - \iota_{,} \cos\psi$ very nearly ψ denoting the inclination of the plane in which e is reckoned to the plane in which $\iota_{,}$ is reckoned.

Hence

$$\left(\frac{1}{r}\right)^2 = \frac{1}{2}(a^2 + c^2) - \frac{1}{2}\left(\frac{1}{a^2} - \frac{1}{c^2}\right)\cos.\left\{2e - \iota_{,}(\cos\psi \pm 1)\right\}$$

$$= \frac{1}{2}(a^2 + c^2) - \frac{1}{2}\left(\frac{1}{a^2} - \frac{1}{c^2}\right)\cos e$$

$$- \frac{1}{2}\left(\frac{1}{a^2} - \frac{1}{c^2}\right)\sin 2e\,.\,(\cos\psi \pm 1)\,\iota_{,}$$

$$= \frac{1}{b^2} - \frac{1}{b^2 a c}\sqrt{(a^2 - b^2)(b^2 - c^2)}\,.\,(\cos\psi \pm 1)\,\iota_{,}$$

$$\therefore\ r = b\,.\left(1 + \frac{1}{2}(\cos\psi \pm 1)\left(1 - \frac{b^2}{a^2}\right)^{\frac{1}{2}}\left(\frac{b^2}{c^2} - 1\right)^{\frac{1}{2}}\iota_{,}\right)$$

Take ψ constant and let the abscissæ and ordinates be reckoned respectively along and perpendicular to the prime ray.

Then $\iota_{,} = \frac{y}{x}$ nearly, and $r = \sqrt{y^2 + x^2} = x$,

or, if we change the origin to the other extremity of the prime ray,

$$\iota_{,} = \frac{y}{b} \quad r = b - x,$$

so that the =ⁿ becomes

$$-\frac{x}{y} = \frac{1}{2}(\cos\psi \pm 1)\sqrt{\left(1 - \frac{b^2}{a^2}\right)\left(\frac{b^2}{c^2} - 1\right)}$$

Hence at each singular point the surface is touched by a cone, the =ⁿ to the generating line of which is given by the above, the extreme angle between it and the prime ray being

$$\cot^{-1}.\left\{\left(\sqrt{\left(1 - \frac{b^2}{a^2}\right)\left(\frac{b^2}{c^2} - 1\right)}\right)\right\}$$

When $b = a$ ψ always $= \frac{\pi}{2}$ and the cone returns into a plane.

Again, let us suppose that the position of any perpendicular from the centre is given, and that of the corresponding radius vector required.

Let O A, O B* denote what we have termed the optic axes, but which it will be more agreeable to analogy to term the prime perpendiculars from centre, and let O P be the given normal. Take O Q, O R contiguous perpendiculars from centre in planes P O Q, R O Q, perpendicular to P O A, P O B respectively, then the inclination of the two former will be the same as that of the two latter, and may be termed μ.

Let $\iota_{,}$ $\iota_{,,}$ now denote the angles P O A, P O B respectively, then

$$\text{QOA} = \iota_{,} \qquad \text{QOB} = \iota_{,,} + \delta\iota_{,,}$$
$$\text{ROA} = \iota_{,} + \delta\iota_{,} \;\; \text{ROB} = \iota_{,,}$$

The ray will be found by joining O with the intersection of three planes drawn at P, Q, R, perpendicular to O P, O Q, O R, respectively.

Now from Prop. 9 it appears that

$$\text{OP} = \sqrt{a^2\left(\sin\frac{\iota_{,} + \iota}{2}\right)^2 + c^2\left(\cos\frac{\iota_{,} + \iota}{2}\right)^2}$$

using only one sign for the sake of simplicity, which we may do by throwing the ambiguity upon the *way* in which $\iota_{,}$ or $\iota_{,,}$ is measured, also

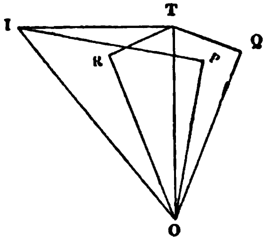
$$\text{OQ} = \text{OP} + \frac{d.\text{OP}}{d\iota_{,,}}\delta\iota_{,,}$$

$$\text{OR} = \text{OP} + \frac{d.\text{OP}}{d\iota_{,}}\delta\iota_{,}$$

---

* O A, O B are not expressed in the figure.

Let $\delta\iota_{,} = \delta\iota_{,,}$, then it is clear that O Q = O R, and the intersection of the two planes perpendicular to O Q, O R is therefore a line perpendicular to the plane Q O R, and to the line which bisects the angle Q O R.



In fact if we draw Q T, R T perpendicular to O Q, O R respectively in the plane Q O R, the intersection in question passes through T and is perpendicular to O T; also

$$\text{O T} = \text{O Q} \,.\, \sec\left(\tfrac{1}{2}\,\text{R O Q}\right) = \text{O Q}$$

to the first order of smallness.

Now it is easy to see (just as in page 57) that

$$\text{R O P} = \frac{\delta\iota_{,}}{\sin\mu},$$

and also

$$\text{Q O P} = \frac{\delta\iota_{,,}}{\sin\mu},$$

$\therefore$ P O P = Q O P and $\therefore$ P O T is perpendicular to Q O D.

Hence the problem is reduced to finding I the intersection of two lines T I, P I drawn in the *same* plane P O T.

Now because O T I, O P I are each right angles, a circle may be made to pass through I, T, P, O.

Hence the angle

$$\text{P I O} = \text{P T O} = \tan^{-1} . \frac{\text{O P} \times \text{P O T}}{\text{O T} - \text{O P}}$$

$$= \tan^{-1} . \frac{\text{O P} \times \text{P O R} . \cos\tfrac{1}{2}\mu}{\dfrac{d . \text{O P}}{d\iota_{,,}}\,\delta\iota_{,,}} = \tan^{-} \frac{\text{O P} \times \dfrac{\delta\iota_{,,}}{\sin\mu}\cos\tfrac{1}{2}\mu}{\dfrac{d . \text{O P}}{d\iota_{,,}}\,\delta\iota_{,,}}$$

$$\therefore \tan \text{P O I} = \sin\tfrac{1}{2}\mu \,.\, \frac{d\,\text{O P}}{d\iota_{,,}}$$

$$\text{and O I} = \text{O P} \,.\, \sec \text{P O I}.$$

Also the position of the plane P O I is known, and $\therefore$ the radius is completely determined in magnitude and position.

It may be worth while also to remark that the above constructions enable us to form a series of equations between the magnitude of the radius and its inclinations to the two prime perpendiculars.

In fact, if we call $\varpi_{,}$ $\varpi_{,,}$ the two inclinations in question

$$\cos\varpi_{,} = \cos \text{P O I} \cos\iota_{,} \pm \sin \text{P O I} \sin\iota_{,} \,.\, \sin\frac{\mu}{2}$$

$$\cos\varpi_{,,} = \cos \text{P O I} \cos\iota_{,,} \mp \sin \text{P O I} \sin\iota_{,,} \,.\, \sin\frac{\mu}{2} \,.$$

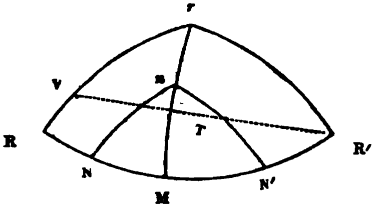
and of course if we call the angle between the two prime normals 2 E

$$\sin\frac{\mu}{2} = \sqrt{\frac{\sin\left(E + \frac{\iota_{,} + \iota_{,,}}{2}\right)\sin\left(E - \frac{\iota_{,} + \iota_{,,}}{2}\right)}{\sin \iota_{,} . \iota_{,}}}$$

Cor. (1.) When $\iota_{,}$ or $\iota_{,,} = 0$ tan . P O I assumes the form $\frac{0}{0}$ which may be interpreted analogously to the method used in the reverse problem, but may be more elegantly illustrated by

Cor. (2.) Which is that the meridian plane P O T (i. e. the plane in which both normal and radius bisects lie) the angle formed by R O P, Q O P, and therefore that formed by the planes drawn through the normal and the two prime normals to which these two are perpendicular.

Now we have found (Cor. 4, page 23) that it also bisects the angle formed by the two planes passing through the radius and the two prime radii. Hence when the ray is given, we may find by the easiest geometry the normal and the tangent plane, and *vice versâ.*

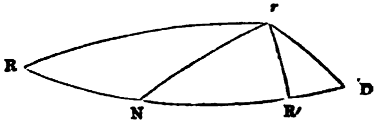


Thus suppose (N, N′) (R, R′) to be the projections of the prime perpendiculars and prime radii on a sphere concentric with the wave surface.

Let *n* be the projection of any given perpendicular on the same sphere, join *n* N, *n* N′, bisect N *n* N′ by *n* M, which will be the meridian plane.

Draw from R′, R′ T V perpendicular to *n* M and make R′ T = T V produce R V to meet M *n* in *r*, then R *r* M = R′ *r* M, and therefore *r* is the projection of the radius. Just in the same way when *r* is given we may find *n*.

Now suppose *n* to come to N, then the position of the meridian plane *n* M becomes indeterminate, and *r* from a point becomes a locus, subject to the condition that R′ *r* N = R *r n*



from *r* draw *r* D perpendicular to *r* N.

Then it is clear that because *r* N bisects R *n* R′

$$\frac{\sin.\ R\,D}{\sin.\ R'\,D} = \frac{\sin.\ R\,r}{\sin.\ R'\,r} = \frac{\sin.\ R\,N}{\sin.\ R'\,N}$$

and ∴ D is a fixed point and N D a fixed length, and

$$\cos.\, r\,N\,D = \tan.\, r\,N \cdot \cot.\ N\,D$$

∴ the projection of the locus of r upon a plane drawn at N perpendicular to the line joining N with the centre O is given by the =ⁿ

$$\rho = \mathrm{O\,N} \,.\, \cot \mathrm{N\,D} \,.\, \cos.\ \theta\,,$$

N being the origin and the projection of N D the prime radius; which is the = ⁿ to a circle passing through N, and whose diameter = O N . cot . N D.

Hence at the extremity of each prime perpendicular the tangent plane meets the surface in a circle passing through that extremity and whose radius = $\frac{1}{2} b \,.\, \cot \alpha$, α being to be found from the equation

$$\frac{\sin(2\,\mathrm{E} + \alpha)}{\sin \alpha} = \frac{\sin(\mathrm{E} + e)}{\sin(\mathrm{E} - e)}$$

$$\text{i. e. } \tan.(\mathrm{E} + \alpha) = (\tan \mathrm{E})^2 \,.\, \cot e$$

Just in the same way it may be shown that the traces of the perpendiculars to the tangent planes of the surface at the point where it is pierced by any prime radius upon a plane perpendicular to that radius at its extremity, is also a circle passing through it, and curved in an opposite direction from the circle of plane contact nearest to it.

Hence the enveloping cone at these points may be described as being perpendicular to the circular cone, formed by drawing lines from the centre to the above described circle; i.e. every generating line of the one will be perpendicular to the generating line which it meets of the other.

More generally it easily appears from the last figure but one that if a series of great circles (representing meridian planes) be taken intersecting the great circle N R R′ N′ in a fixed point, a plane perpendicular to the radius passing through that point, will intersect the cone of rays as well as the cone of perpendiculars corresponding to those meridian planes, in two *circles*. So that there exist an indefinite number of *circular* cones of rays corresponding to *circular* cones of perpendiculars touching each other in a line lying in the plane containing the extreme axes, and having their circular sections perpendicular to that line.

The cusps are explained by the cone of *rays* degenerating into a right line, and the circles of plane contact by the cone of perpendicular so degenerating.

Furthermore I observe in conclusion that when a ray is given it follows from the general geometrical construction above that there will be two meridian planes according as we take R with R′, or with a point 180 degrees from R′, and consequently these two planes will be perpendicular to each other.

And similarly when a *normal* is given there will be two meridian planes perpendicular to each other.

Thus the planes passing through any radius and the two normals at the points where it pierces the wave surface, are *perpendicular* to each other, as are also the two planes passing through any normal and *its* two corresponding radii.

Moreover a glance at the figure in vol. xi. p. 541., will show that the two lines of vibration corresponding to any front, lie respectively in the two meridian planes passing through the perpendicular to that front, or, in other words, the intersection of a plane drawn through either ray belonging to a front perpendicular thereunto is *always* a line of vibration in that front.

This has been noticed, I think, by Sir William Hamilton for the particular case of the singular points.

As two fronts belong to every ray, so two rays pertain to every front. And from what has been said above it appears that the two lines of vibration in any front are the projections of its two rays upon its own plane.

XVII. *Notice on the Bichromate of the Perchloride of Chrome.*

By M. WALTER[*].

I HAVE constantly succeeded in the preparation of this compound by employing the quantities and process following: I placed in a tubulated glass retort 100 parts of sea-salt dissolved, and 168 parts of neutral chromate of potash, the whole being well mixed and reduced to a very fine powder; I then fixed to the retort a tube and a receiver with two apertures, and poured by degrees, through a tube in the form of an S, which was fixed in the aperture of the retort, 300 parts of concentrated sulphuric acid.

The liquor thus obtained is of a beautiful blood-red colour; it is volatile, and sends forth vapour copiously; when placed in contact with a quantity of water, it falls to the bottom in drops of an oily appearance, and changes into chlorhydric acid and chromic acid; its boiling point is fixed and takes place at 118° Cent. under the pressure of $0^{m}\cdot76$; its specific gravity at the temperature of 21° Cent. is 1·71; it attacks mercury with great activity, for this reason all contact with this metal must be avoided; it is decomposed by sulphur, detonates with phosphorus, dissolves chlorine and iodine, and combines with ammonia with a disengagement of light. A small quantity mixed with concentrated alcohol combines with a violent explosion, and the inflamed alcohol is scattered with force. This unexpected action very nearly deprived me of my sight, and has burnt me in a most dreadful manner.

The bichromate of the perchloride of chrome is composed according to my analyses, of

Chlorine.........	45·14
Chrome	35·58
Oxygen	19·28

This result agrees with that obtained by H. Rose, and with a combination calculated according to the formula $2\ Cr\ O^3 + Cr\ O^6$.

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[*] From the *Comptes Rendus,* No. 22, 2nd Ser. 1837.

$$\begin{aligned} Cr^3 &= 1055{\cdot}457 = 35{\cdot}37 \\ Ch^6 &= 1327{\cdot}950 = 44{\cdot}51 \\ O^6 &= 600{\cdot}000 = 20{\cdot}12 \end{aligned}$$

The density of the vapour deduced from observation gives $D = 5{\cdot}9$; the value obtained by calculation by means of the formula $2\ Cr\ O^3 + Cr\ Ch^6$, is equal to $D = 5{\cdot}48$.

3 vol. of chrome......	11·6433
6 vol. of chlorine ...	14·6760
6 vol. of oxygen......	6·6156
	32·9349

$$\frac{32{\cdot}9349}{6} = 5{\cdot}48$$

The analysis and the density of the vapour of the bichromate of the perchloride of chrome coincide therefore in representing this body as a combination of chromic acid and perchloride of chrome; its constitution may however be regarded in a different light, which, without being in contradiction either to the composition or to the density found, explains in a certain degree better its remarkable characters and its little stability. Dr. Thomson having subjected this body at the time to analysis, had already offered quite a peculiar opinion on its constitution; he regarded it as being formed of chromic acid and of chlorine, and called it *chloro-chromic* acid;[*] but this opinion could not withstand the objection of H. Rose, that with this supposition the combination must contain 10 per cent. more chlorine than obtained by analysis. But if instead of representing this combination as formed of chromic acid and chlorine, we look upon it as being formed of $Cr\ O^2$ and chlorine, the hypothetical radical $Cr\ O^2$ of chromic acid (itself expressed by the formula $Cr\ O^2 + O$), acting the part of a simple body similar to the oxide of carbon and to benzoyl, this combination becomes analogous to the chloro-oxicarbonic acid, the chlorine occupying the place of the oxygen, which is not found in the radical of chromic acid. We may therefore represent this body by the formula $Cr O^2 + Ch$, which agrees both with the analysis and with the density found. Indeed the analysis calculated according to this formula gives the following result:

1 atom of chrome	351·819	= 35·37
2 atoms of chlorine......	442·650	= 44·51
2 atoms of oxygen	200·000	= 20·12
	994·469	100·00

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[*] A notice of Dr. Thomson's researches will be found in Phil. Mag. and Annals, N. S. vol. i. p. 452.—EDIT.

And as to what concerns the density, calculated according to the same formula, we find

1 vol. of chrome	3·3811
2 vol. of chlorine ...	4·8920
2 vol. of oxygen	2·2078
	10·9809

$$\frac{10{\cdot}9809}{2} = 5{\cdot}49$$

But here each atom of the compound represents only two volumes of vapour. This body may therefore be regarded as a distinct acid, which might be named *chloro-oxi-chromic acid*. Recollecting that the perchloride of chrome does not exist in an isolated state, that analogous compounds are only produced by acids, which for one atom of radical contain three atoms of oxygen, which are isomorphous with each other, and which may all be expressed after the hypothesis of M. Persoz, by the formula $R\,O^2 + O$, in taking into consideration the facility with which this body is decomposed when brought into contact with other bodies, and its little stability; this manner of regarding the constitution of this body, which explains its various actions, offers the appearance of much probability.

XVIII. *Observations on the Meteors of the 12th of November. Communicated by Prof.* FORBES.[*]

A COMMITTEE appointed by the Physico-Mathematical Society (University of Edinburgh) were employed on the nights of the 12th and 13th of November, in watching for the annual fall of meteors, expected to take place about that time. During the earlier part of the night of the 12th there was a fine coloured Aurora: at $7^h\ 30^m$, a red arch extended from E. to W. passing about 15° S. of the zenith and within 15° of the full moon. This continued with slight variations till 9^h, when it stretched from E. to SW., there being a beautiful red spot at the point of radiation a little S. or SE. of the zenith. The red colour continued with variations till $10^h\ 15^m$, when the whole northern half of the sky was covered with bright greenish streamers. Soon after the sky became covered with clouds and mist, through which the brightest stars only were visible. There was scarcely any wind. Till late in the morning no meteors of any importance were seen. At $9^h\ 35^m$ one meteor shot from E. toward NNE., and at $10^h\ 20^m$ another from the zenith along meridian. At $0^h\ 25^m$ one passed from NNW.

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[*] See our last volume, pp. 261, 567.

to SE., and at 1^h 45^m another shot nearly due S. About 4^h however the sky cleared, and between that and 5^h 10^m four bright meteors were seen in the immediate vicinity of Leo, of which the three last had paths inclined so as to meet in that constellation. All these were inclined towards the north, and more inclined the higher in the sky. Throughout the night of the 13–14th, clouds, mist and sleet prevailed and nothing was seen. It may be added that Professor Nichol of Glasgow mentions, that about five o'clock of the morning of the 13th he saw three meteors, at intervals of two seconds, shoot from the centre of Leo towards the south, from which he concludes that a shower did take place at an earlier period of the night *.

Heriot Row, 7th Dec., 1837.

XIX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xi. p. 394.]

Nov. 1, 1837. — A LETTER "On Fossil Fishes in the Lancashire Coal Field," by W. C. Williamson, Esq., Curator of the Manchester Natural History Society, was first read.

The author first refers to his account of the Ardwick Limestone, published in the Philosophical Magazine for 1836 (L. and E. Phil. Mag., vol. ix. p. 241.), where short descriptions are given of the ichthyolites which had been then met with, consisting of scales of Megalichthys, scales and teeth of Palæoniscus and coprolites. Mr. Williamson, in conjunction with Professor Johnstone, has since come to the conclusion, that the bed in which these remains occur is entirely a coprolitic mass, the portions preserved being such as would not be destroyed by the action of the stomach. With the above remains was also described a tooth of *Diplodus gibbosus*, (Agassiz), numbers of which of various sizes have been found at Bradford, near Manchester, in the roof of the great mine, a coal four feet thick, almost the highest in the series that is worked; and the roofstone is almost entirely composed of Entomostracous remains. The teeth resemble one figured by Dr. Hibbert in his Memoir on the Limestone at Burdie-house, and referred by him to Gyracanthus. The author has met with no traces of the thorny ray of this fish. The coprolites contain 72·5 phosphate of lime, 12·5 carbonate of lime, 12·5 bitumen, 2·5 insoluble matter; resembling the analysis given by Dr. Hibbert.

The author, in examining, at Peel, near Worsley, the "Black and White Mine," a coal 6 feet 6 inches thick, and about 1000 yards below the Rodte Todte Liegende, found, in its black roofstone, remains of *Palæoniscus Egertonii*. The fine blue colour of the scales forms a curious characteristic. Two other forms of scales have been met with, evidently belonging to distinct species of fish; one is small, rhomboidal, and coated with a bright enamel; the other is peculiar from the mucronate and grooved character of one extremity, and the cycloid outline of the other. At the same place were found scales resembling those of Megalichthys, several small teeth of *Diplodus gibbosus*, and several osseous portions of some large fish not yet determined. Near Ringley, about five miles from Peel, the same roofstone was found in another pit, which the author has not yet further examined; but in both pits one or two species of Unio occur, as well as remains of *Stigmaria ficoides*, *Calamites nodosus*, and other plants.

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* In the *Comptes Rendus* of the 27th Nov. are some accounts of observations on the night of the 12th and 13th at Paris, Montpellier, Geneva, and Marseilles.

A paper "On the geology of the island of Zante," by Hugh Edwin Strickland, Esq., F.G.S., was next read.

The author commences by stating that his observations were the the result of only a few days' residence. The structure of Zante is more simple than that of the other Ionian islands; and seems to present an epitome of their component rocks in an almost unbroken succession.

The geological phenomena of Zante may be arranged under the three heads of, 1. Apennine Limestone; 2. Tertiary deposits; and 3. Mineral Springs.

1. *Apennine Limestone.*—This name is adopted as being the most convenient appellation for the vast deposit of compact, light-coloured limestone in the south of Europe, especially on the shores of the Adriatic; it is uniform in character for many thousand feet of vertical thickness, and many hundred miles of horizontal extent. Its fossils, though rare, show it to be the equivalent of the cretaceous, and perhaps also of the oolitic series of Northern Europe. The formation constitutes an anticlinal ridge, extending along the west coast of the island. Its direction is about N.N.W. and S.S.E., and it seems to be the continuation of a similar ridge which passes through Cephalonia. Along the E. side of this ridge, the strata dip from 30° to 45° E.N.E. but to the west of Point Skinari an opposite dip commences, and prevails, with local exceptions, to near Point Cheri, where it resumes the eastwardly inclination.

The tertiary strata occur only on the eastern side of this mountainous ridge; which on the west forms a series of almost perpendicular cliffs, upwards of 600 feet high.

This white limestone often assumes the character of the hard chalk of England. No flints were noticed, though they are not unfrequent in this limestone in Corfu. Organic remains are occasionally found, and consist of Nummulites, fragments of Hippurites, &c. It abounds in faults and fractures, as well as caverns, subterranean rivers, and thermal and mineral springs. In these respects no less than in mineral structure the Apennine limestone presents a close analogy to the carboniferous limestone of Northern Europe, for which it has often been mistaken.

2. The Tertiary beds repose on the eastern flank of the limestone range, and extend thence to the east coast. They form several detached hills rising through the alluvial matter, which forms the cen-

-tral plain of the island. They have partaken of the same elevation which has raised the limestone range, from which they dip away regularly to the eastward. The uppermost strata resemble those which occur near Lixouri in Cephalonia. In Zante, near the Castle-hill, they consist of an aggregate of calcareous and arenaceous particles, forming a pale yellow, porous stone, which is easily worked. Few fossils are found except on the east coast, where numerous casts of Cerithia and other Mollusca occur. These strata are succeeded by a thick deposit of blue clay and marl, containing *Pectunculus auritus*, Broch., *Buccinum semistriatum*, Broch., and *Natica glaucina*, Lam.

The gypseous beds which succeed the argillaceous at Lixouri, are not here visible, but in the south coast of Zante they form the commencement of a section which extends much further down in the series than the lowermost beds examined at Lixouri.

The strata above the gypsum clearly belong to the Pliocene epoch; many of their fossils being identical with those of the subapennine hills. Whether the strata which underlie the gypsum, in the section on the north of the Bay of Cheri, belong to the same, or to an older epoch, is not so clear. They contain but few fossils, as crushed Echini and obscure bivalves; but in one situation a bed of indurated bluish marl contains great abundance of the shells of the two genera of pteropodous mollusca, Hyalea and Creseis, but the species are larger than the *Hyalea cornea*, and *Creseis spinifera*, now living in the Mediterranean.

The argillaceous beds are succeeded by yellowish calcareous sandstone and loosely aggregated limestone, which dip 18° S.W., but no traceable sequence could be observed, in consequence of a great subsidence, which appears to have taken place between this point and the range of secondary limestone forming the marshy plain of Port Cheri, towards which the tertiary strata dip on both sides. Some of the calcareous beds are fine-grained, and approach the texture of Portland-stone. Minute Foraminifera are abundant in it, and two small species of Pecten were observed.

On the west side of Port Cheri is a low cliff of blue marl and clay, containing a few scales and vertebræ of fishes, and a few species of *Vermiculum*, Mont., (Quinqueloculina, D'Orbigny). This argillaceous mass has probably been brought down from some higher part of the tertiary series, by the subsidence which seems to have formed the valley and bay of Port Cheri, and of which a striking proof maybe seen in a fault in the Apennine limestone, where a smooth surface of rock descends to the sea, and which is scored with numerous parallel striæ, not perpendicular, but at an angle of 65° to the horizon. The enormous friction and pressure of the descending mass has imparted to this surface of rock a remarkable degree of hardness, and a darker colour. This change penetrates about two or three inches from the surface; at a greater depth the rock is softer and white, much resembling the compact chalk of Yorkshire.

3. *Mineral Springs.*—The sources of bitumen for which Zante has been celebrated since the time of Herodotus rise in the midst of the marshy plain at Port Cheri. The principal is a well 5 feet deep; and

the bitumen oozes up from the bottom, and above it the well is filled by a spring of clear, cool, and tasteless water. No bubbles of gas were observed to be given out by the bitumen. About forty barrels are produced here annually.

From the different situations in which bitumen is produced, and from there being nothing in the composition of either the tertiary or secondary rocks to account for its production, as well as from its rising where there has been a great dislocation of the strata; the author is induced to infer that it is derived from the region of volcanic action, which may be almost demonstrated to underlie the Ionian islands. On the northern coast there is another mineral spring, which rises on the line of a considerable fault in the Apennine limestone, about half a mile to the north of the junction of the tertiary and secondary rocks. It consists of turbid water, resembling diluted milk in appearance, and issuing at the foot of the cliffs, flows on the surface of the sea-water, in a stratum a few inches thick. Flakes of a slimy white substance abound in this water, and may be seen floating in the sea for a considerable distance. A strong smell of sulphuretted hydrogen is diffused around. The spring indicated a temperature of 65°, which is near the mean temperature of the latitude of Zante. This, therefore, cannot be reckoned among thermal springs, though from its close resemblance to the mineral waters of many volcanic regions, as the Aquæ Albulæ near Rome, its origin may be referred to some analogous cause.

A paper was afterwards read "On the Formation of Mould," by Charles Darwin, Esq., F.G.S.

The author commenced by remarking on two of the most striking characters by which the superficial layer of earth, or, as it is commonly called, vegetable mould, is distinguished. These are its nearly homogeneous nature, although overlying different kinds of subsoil, and the uniform fineness of its particles. The latter fact may be well observed in any gravelly country, where, although in a ploughed field, a large proportion of the soil consists of small stones, yet in old pasture-land not a single pebble will be found within some inches of the surface. The author's attention was called to this subject by Mr. Wedgwood, of Maer Hall, in Staffordshire, who showed him several fields, some of which, a few years before, had been covered with lime, and others with burnt marl and cinders. These substances, in every case, are now buried to the depth of some inches beneath the turf. Three fields were examined with care. The first consisted of good pasture land, which had been limed, without having been ploughed, about twelve years and a half before: the turf was about half an inch thick; and two inches and a half beneath it was a layer or row of small aggregated lumps of the lime forming, at an equal depth, a well-marked white line. The soil beneath this was of a gravelly nature, and differed very considerably from the mould nearer the surface. About three years since cinders were likewise spread on this field. These are now buried at the depth of one inch, forming a line of black spots parallel to and above, the white layer of lime. Some other cinders, which had been scattered in another part of the same field, were either still lying on the surface, or entangled in the roots of the grass. The second field examined was remarkable only from the cinders being now buried in a layer, nearly an inch thick, three inches beneath the surface. This layer was in parts so continuous, that the superficial mould was only attached to the subsoil of red clay by the longer roots of the grass.

The history of the third field is more complete. Previously to fifteen years since it was waste land; but at that time it was drained, harrowed, ploughed, and well covered with burnt marl and cinders. It has not since been disturbed, and now supports a tolerably good pasture. The section here was, turf half an inch, mould two inches and a half, a layer one and a half inch thick, composed of fragments of burnt marl (conspicuous from their bright red colour, and some of considerable size, namely, one inch by half broad, and a quarter thick), of cinders, and a few quartz pebbles mingled with earth; lastly, about four inches and a half beneath the surface was the original, black, peaty soil. Thus beneath a layer (nearly four inches thick) of fine particles of earth, mixed with some vegetable matter, those substances now occurred, which, fifteen years before, had been spread on the *surface*. Mr. Darwin stated that the appearance in all cases was as if the fragments had, as the farmers believe, worked themselves down. It does not, however, appear at all possible, that either the powdered lime or the fragments of burnt marl and the pebbles could sink through compact earth to some inches beneath the surface, and still remain in a continuous layer. Nor is it probable that the decay of the grass, although adding to the surface some of the constituent parts of the mould, should separate, in so short a time, the fine from the coarse earth, and accumulate the former on those objects which so lately were strewed on the surface. Mr. Darwin also remarked, that near towns, in fields which did not appear to have been ploughed, he had often been surprised by finding pieces of pottery and bones some inches below the turf; in a similar manner on the mountains of Chile he had been perplexed by noticing elevated marine shells, covered by earth, in situations where rain could not have washed it on them.

The explanation of these circumstances, which occurred to Mr. Wedgwood, although it may at first appear trivial, the author did not doubt is the correct one, namely, that the whole is due to the digestive process, by which the common earth-worm is supported. On carefully examining between the blades of grass in the fields above described, the author found, that there was scarcely a space of two inches square without a little heap of the cylindrical castings of worms. It is well known that worms swallow earthy matter, and that having separated the serviceable portion, they eject at the mouth of their burrows, the remainder in little intestine-shaped heaps. The worm is unable to swallow coarse particles, and as it would naturally avoid pure lime, the fine earth lying beneath either the cinders and burnt marl, or the powdered lime, would, by a slow process, be removed, and thrown up to the surface. This supposition is not imaginary, for in the field in which cinders had been spread out only half a year before, Mr. Darwin actually saw the castings of the worms heaped on the smaller fragments. Nor is the agency so trivial as it, at first, might be thought; the great number of earth-worms (as every one must be aware, who has ever dug in a grass-field) making up for the insignificant quantity of work which each performs.

On the above hypothesis, the great advantage of old pasture land, which farmers are always particularly averse from breaking up, is explained; for the worms must require a considerable length of time to prepare a thick stratum of mould, by thoroughly mingling the original constituent parts of the soil, as well as the manures added by man. In the peaty field, in fifteen years, about three inches and a half had been well digested. It is probable, however, that the process is continued, though at a slow rate, to a much greater depth; for as often as a worm is compelled by dry weather or any other cause to descend deep, it must bring to the surface, when it empties the contents of its body, a few particles of earth. The author observed, that the digestive process of animals is a geological power which acts in another sphere on a greater scale. In recent coral formations, the quantity of stone converted into the most impalpable mud, by the excavations of boring shells and of nereidous animals, is very great. Numerous large fishes (of the genus *Sparus*) likewise subsist by browsing on the living branches of coral. Mr. Darwin believes, that a large portion of the chalk of Europe was produced from coral, by the digestive action of marine animals, in the same manner as mould has been prepared by the earth-worm on disintegrated rock. The author concluded by remarking, that it is probable that every particle of earth in old pasture land has passed through the intestines of worms, and hence, that in some senses, the term "animal mould" would be more appropriate than "vegetable mould." The agriculturist in ploughing the ground follows a method strictly natural; and he only imitates in a rude manner, without being able either to bury the pebbles or to sift the fine from the coarse soil, the work which nature is daily performing by the agency of the earth-worm*.

* Since the paper was read Mr. Darwin has received from Staffordshire the two following statements:—1. In the spring of 1835 a boggy field was so thickly covered with sand that the surface appeared of a red colour; but the sand is now overlaid by three quarters of an inch of soil. 2. About 80 years ago a field was manured with marl; and it has been since ploughed, but it is not known at what exact period. An imperfect layer of the marl now exists at a depth, very carefully measured from the surface, of 12 inches in some places, and 14 in others, the difference corresponding to the top and hollows of the ridges or butts. It is certain that the marl was buried before the field was ploughed, because the fragments are not scattered through the soil, but constitute a layer, which is horizontal, and therefore not parallel to the undulations of the ploughed surface. No plough, moreover, could reach the marl in its present position, as the furrows in this neighbourhood are never more than eight inches in depth. In the above paper it is shown, that three inches and a half of mould had been accumulated in fifteen years; and in this case, within eighty years (that is, on the supposition, rendered probable from the agricultural state of this part of the country, that the field had never before been marled) the earthworms have covered the marl with a bed of earth averaging thirteen inches in thickness.

LINNÆAN SOCIETY.

Nov. 7.—Amongst the numerous donations presented on this occasion were an extensive collection of dried plants from Swan River and King George's Sound, bequeathed to the Society by the late Alexander Collie, Esq., F.L.S., Surgeon R.N., and Surgeon to the Colony of Western Australia; and another collection of dried plants, chiefly from Gongo Soco, in the province of Minas Geraes, Brazil, presented by Charles James Fox Bunbury, Esq., F.L.S.; a collection of the skins of quadrupeds and birds, presented by the Committee of the Australian Museum at Sydney, New South Wales; another, consisting of skins of quadrupeds, birds, reptiles, fruits of *Strychnos toxifera*, *Lecythis grandiflora*, *Curataria guianensis*, and other productions from British Guiana, presented by Mr. Robert H. Schomburgk.

Mr. Pamplin, A.L.S., exhibited a plant of the *Cystopteris regia* obtained from the original station at Low Layton, Essex, in 1835, and a specimen and a drawing of a curious variety of *Ophrys aranifera*, from the vicinity of Dover.

A flowering plant of the *Azara integrifolia* of Ruiz and Pavon was exhibited from the Chelsea Botanic Garden by Mr. William Anderson, F.L.S. Mr. Gould, F.L.S., exhibited a drawing of the *Apteryx australis*, a remarkable bird of the family of *Struthionidæ*, from New Zealand. Mr. Newman, F.L.S., exhibited British specimens of the *Cantharis vesicatoria*, the blister-fly of the *Pharmacopœia*; the insect, previously considered extremely rare, and almost doubtful as British, having appeared during the past autumn by millions in the neighbourhood of Colchester on the ash trees.

Read a paper entitled "Systema Vertebratorum," comprising a systematic arrangement of the vertebrate animals. By Charles Lucien Bonaparte, Prince of Musignano, F.M.L.S.

In this paper the learned author, whose extensive knowledge in zoology is universally admitted by naturalists, has subdivided the *Vertebrata* into the five following classes, which are thus characterized:—

Classis

I.—Mammalia. Sanguis calidus: pulmones liberi: genitalia ab ano exterius discreta: mammæ. *Vivipara.*

II.—Monotremata. Sanguis calidus: pulmones liberi: cloaca. *Ovipara.*

III.—Aves. Sanguis calidus: pulmones affixi: alæ. *Ovipara.*

IV.—Amphibia. Sanguis frigidus: pulmones liberi. *Ovipara* vel *Ovo-vivipara.*

V.—Pisces. Sanguis frigidus: pulmones nulli: branchiæ. *Ovipara* vel *Ovo-vivipara.*

Each of these classes are divided into sub-classes, which are again subdivided into orders and families, which are successively characterized. The first class, mammalia, for example, is divided into two sub-classes, named *Quadrupedia* and *Cetæ*; the first comprising ten orders, arranged under the sections *Unguiculata* and *Ungulata*; and the second two orders, namely *Sirenia* and *Hydraula*. [We regret that our limits will not admit of our following farther the details of the arrangement of the learned author.]

Read also, a paper communicated by the Secretary from Dr. Hancock, on the Angostura bark tree.

Nov. 21.—Specimens of *Erica ciliaris*, *Statice spathulata*, *Spartina alterniflora*, *Rhynchospora fusca*, *Isolepis Savii*, and a remarkable variety of *Erica tetralix* with broader leaves, and the axis of the inflorescence more elongated than usual, were presented by Mr. Joseph Woods, F.L.S., by whom they were collected in the South and West of England during the past summer.

Dried specimens of *Cereus senilis*, and of various species of *Echinocactus* and *Mammillaria* gathered in Mexico by M. Deschamps, were presented by Mr. George Charlwood, F.L.S.

Read a notice on the discovery of *Cucubalus baccifer* in the Isle of Dogs. By Mr. George Luxford, A.L.S.

Specimens of this remarkable plant were collected by Mr. Luxford in August last, on the banks of a ditch near the road leading from Blackwall to the Ferry-house, where the plant was growing in considerable plenty, and with every appearance of being truly wild; and Mr. Luxford suggests that it may have been overlooked in other places for *Cerastium aquaticum*, which it very much resembles when not in flower or fruit.

The plant was introduced, but, as afterwards appeared, on very slender grounds, into the third edition of *Ray's Synopsis* by Dillenius, on the authority of a Mr. Foulkes, of Llanbeder, who had been informed that some herbalist had met with it in Anglesea; but neither Mr. Foulkes nor any one since has been fortunate enough to confirm its discovery in that island. Mr. Luxford's paper was accompanied by specimens.

Read likewise "Observations on the family of *Fulgoridæ*," with a monograph of the genus *Fulgora* of Linnæus. By John O. Westwood, Esq., F.L.S.

The *Fulgoridæ* constitute a small but remarkable group of the class *Hemiptera*, having their head often produced anteriorly into a lengthened and frequently singularly dilated rostrum, which was supposed to serve the creature as a lantern, from the phosphorescent light which it was said to give forth in the dark; but this appears to be a mere fable, originating, like the tale of the bird-catching spider, with Madame Merian, who seems to have credulously registered all the strange stories that were told her; for none of the naturalists who have visited the countries where the lantern-fly (*Fulgora laternaria*), abounds have witnessed any phosphorescent property in the rostrum of that singular insect, so that the real function of that organ remains still unknown.

The *Fulgoridæ* were originally included by Linnæus with the grasshoppers in the genus *Cicada*; but he afterwards separated his *Cicada laternaria* and some other species (agreeing in having an enlarged snout,) into a distinct genus, which he named *Fulgora*, from the phosphorescent property ascribed to the rostrum of the lantern-fly by the lady above-mentioned. From the other genera of the family *Fulgora* is distinguished by having the second joint of the antennæ globose, and the forehead produced into a rostrum or snout.

The monograph before us contains 25 species, of which eight are natives of Æquinoxial America, one of Mexico, nine of India and the Islands, one of Nepal, two of New Holland, two of Guinea, and one of the Cape of Good Hope. The geographical distribution of the family is equally remarkable, and to the other regions already mentioned we must add those of Europe and the United States. We shall conclude our brief notice of this interesting paper by giving the names and characters of the new species, viz.:

Sp. 9. *F. apicalis*, fronte rostratâ thoracis longitudine gracili fulvo-fuscescenti, hemelytris fulvis apice hyalinis fusco-maculatis, basi fusco et miniato variegatis.
Long. corp. cum rostro lin. 12; rostr. lin. 3½; expans. alar. lin. 20.
Hab. in Manilâ. *D. Cuming.*

10. *F. decorata*, fronte rostratâ adscendente corporis ferè longitudine, capite thoraceque viridibus, metathorace abdomine aliisque sanguineis: his apice nigris, hemelytris ferrugineis apice fuscis.
Long. corp. cum rostro lin. 12½; rostr. lin. 5½; expans. alar. lin. 21.
Hab. in Javâ. *Mus. Reg. Paris.*

11. *F. oculata*, fronte rostratâ adscendente corporis longitudine griseo-fulvescenti, hemelytris ocellis fulvis, alis albis basi viridibus margineque antico roseo tinctis.
Long. corp. cum rostro lin. 16½; expans. alar. lin. 30.
Hab. in Indiâ orientali. *Mus. Reg. Paris.*

16. *F. affinis*, fronte rostratâ ferè corporis longitudine apice truncato luteo-griseâ, thorace pedibus hemelytrisque punctis nigris adspersis, abdomine nigro, alis albis: venis pallidis.
Long. corp. cum rostro lin. 16; expans. alar. lin. 26.
Hab. in Nepaliâ. *D. Hardwicke.*

17. *F. cognata*, griseo-fulvescens; abdomine concolori, hemelytris pallidioribus nigro-punctatis, alis albis: venis pallidis.
Expans. alar. lin. 14.
Hab. . . . .

19. *F. dilatata*, capite rostrato: rostro dimidii corporis longitudine apice attenuato griseo-fuscescenti, abdomine fulvo apicibus segmentorum nigris, hemelytris pallidè cinereis singulo ocellis 12 roseis et nigris: venis nigris et roseis.
Long. corp. cum rostro lin. 8; rostr. lin. 3; expans. alar. lin. 17.
Hab. in Novæ Hollandiæ orâ occidentali, ad ripas fluminis Cygnorum. *Mus. D. Hope.*

20. *F. nobilis*, capite rostrato griseo-virescenti tincto nigro-punctatissimo: rostro ferè corporis longitudine recto, tuberculis acutis nigris in lineas sex dispositis, hemelytris punctis fulvis, alis albis.

Long. corp. cum rostro lin. 30; rostr. lin. 12; expans. alar. lin. 55.
Hab. in peninsulâ Malacensi. *Mus. D. Hope.*

Dec. 2.—At the Special Meeting held this day, Edward Lord Bishop of Norwich was elected President of the Society, in the room of His Grace the Duke of Somerset, who has resigned.

Dec. 5.—The Secretary read a letter from the President, in which he nominated Mr. Brown, Mr. Forster, Dr. Horsfield, and Mr. Lambert, Vice-Presidents for the present year.

The Chairman read a letter addressed to the President, from Lord John Russell, Her Majesty's Principal Secretary of State for the Home Department, in answer to the Address to Her Majesty, voted at the preceding ordinary Meeting, and announcing that Her Majesty had been graciously pleased to become the Patroness of the Society.

A number of casts and impressions of recent ferns and other plants were placed on the table for exhibition by Mr. Morris, Chemist, Kensington. In a note which accompanied the specimens Mr. Morris stated that he found it impossible in many cases to determine from the casts whether the divisions of the frond were continuous with the rhachis or midrib, or elevated on a partial stalk, as in *Pteris falcata*, *Lindsæa trapeziformis*, and many others; and that this character, which is also dependent in some instances upon whether the cast is taken from the under or upper surface, has been a source of error in the distribution of fossil species. Some of the specimens, previously soaked in metallic solutions, such as sulphate of iron, &c., having been placed between layers of clay, and afterwards subjected to the heat of a furnace, presented complete casts of the plants. In most cases the whole of the carbonaceous matter was found to have been removed, leaving merely white ashes, in which the original structure was occasionally retained, as in the case of *Equiseta* and some *Coniferæ*; but in others where a portion of the carbonaceous matter had been retained, the clay was found coloured for some distance around the specimen. Some of these examples Mr. Morris conceives explain the state in which some fossil ferns are found in the sandstones and shales, associated with the carboniferous and oolitic series, that is partly bituminized and partly mineralized.

Read a notice of certain Australian Quadrupeds belonging to the order *Rodentia*. By William Ogilby, Esq., M.A. F.L.S., &c.

The object of this memoir was to explain the natural relations of the Monodelphine quadrupeds of Australia, and to describe some new forms and species which had lately come to the author's knowledge. Mr. Ogilby commenced by remarking upon the anomalous character of Australian zoology, particularly as regards the mammals of that extensive Continent. With the exception of the native dog, two or three species of rats, and the hydromys, it was observed that all the Australian quadrupeds hitherto discovered belonged exclusively to the marsupial family; and, on the other hand, that this tribe of animals, was, with a very few exceptions, equally confined to the continent of New Holland, and the neighbouring islands. Of the exceptions to this general rule, one of the most singular and important, which regards the geographical distribution of animals, Mr. Ogilby regarded the native dog as an importation, probably contemporary with the original settlement of the inhabitants, and adduced various arguments in support of this opinion. It followed that the remaining exceptions to the purely marsupial character of Australian mammalogy belong exclusively to the *Rodent* type, a fact as singular, and scarcely less important than that to which it forms an exception.

Mr. Ogilby afterwards proceeded to the description of two new species of Australian Rodents, belonging to forms altogether new, or hitherto not known to exist in that country; and expressed his belief that more extensive research would lead to many more discoveries of a similar nature, and tend to diminish the disproportion which at present exists between the members of marsupial and non-marsupial species. The first of the new forms described by Mr. Ogilby formed the type of a new genus, for which he proposed the name of *Conilurus*, in allusion to its external resemblance to a small rabbit with a long tail. The specimens had been originally sent from Sydney by the late Mr. George Coley, under the name of the "native rabbit," and have been long deposited in the Museum of the Linnæan Society. The animal seems to have disappeared from the settled parts of New South Wales since that period, but was found in abundance on the banks of the river Darling, during the late expedition of Major Mitchell, from whose forthcoming journal an interesting extract was read, descriptive of its habits and œconomy. Major Mitchell's party had frequently encountered large piles of small branches and brushwood, of a size sufficient to fill two or three large carts, so ingeniously and compactly woven together, that it was impossible to remove a part without removing the whole fabric. At first they supposed these piles to be collected by the natives for the purpose of making signal fires, but the regularity and compactness of their texture led to a closer examination, and on being broken open, it was discovered that they were entirely the work of the little animal in question, whose instinct thus prompts it to erect a fortress against the attacks of the native dog.

The other animal described by Mr. Ogilby, though not belonging to a new genus, was equally interesting, as illustrative of the laws of the geographical distribution of animals. It was a true Gerboa (*Dipus*) from the central plains of New Holland, and had been found by Major Mitchell near the junction of the Murray and the Morrumbidgee. It is distinguished from the Gerboas of Asia and Africa by having only four toes on the hind feet, namely three normal toes and a smaller one about one third of its distance up the metatarsus, on the inner side. Mr. Ogilby proposed to dedicate this curious animal to its discoverer, and to commemorate the circumstance by the name of *Dipus Mitchellii*.

Dec. 19.—Mr. Ward, F.L.S., exhibited a series of specimens of *Laminaria digitata* (*Fucus digitatus*, L.) showing the curious mode by which the species of that genus renovate their fronds. These specimens Mr. Ward had received from Mrs. Griffiths, by whom they were collected at Torquay, and we subjoin the following extracts from a letter by that distinguished algologist, which accompanied the specimens. "I have (she says,) sent you some plants of *Laminaria digitata*, exhibiting the singular mode by which the plants of this genus reproduce their frond from time to time, and I believe many of them annually. You will observe one of them beginning to form a new frond at the base of the old one, and between it and the stem, the other is further advanced and is preparing to throw off and take the place of its predecessor. In a few weeks the new one would have attained a much larger size, and so on, until at length the root gives way, unable to support the weight. I beg to observe that I do not claim the merit of being the discoverer of the renewing power possessed by these plants, that being noticed by Dr. Greville in his *Algæ Britannicæ*; but a long residence by the sea in favourable situations has enabled me to trace their progress from the earliest period of their existence to their final decay, and therefore I feel competent to speak with confidence upon the subject. I am not certain that the reproduction takes place annually, although I think it probably does so from the clean and fresh appearance of the plants in the months of June and July, and the immense quantity of fragments cast on the shore in April and May, and which are carted off for manure. The same mode of growth prevails also in *Laminaria bulbosa* and *saccharina*, but it is impossible to preserve the former from its great size."

Read a notice on *Succinea amphibia*, and its varieties. By Mr. Daniel Cooper, A.L.S.

Read likewise a paper by Edwin J. Quekett, Esq., F.L.S., entitled "Some observations on the varieties of growth of plants belonging to the genus *Chara* of Hooker."

Sir William Hooker has in the British Flora expressed an opinion that our British *Charæ* may be reduced to two species, namely, *C. vulgaris* and *flexilis*, the former including all those with opake stems, and the latter those with pellucid stems. Mr. Quekett's observations go not only to prove that this opinion is correct, but that even the *vulgaris* and *flexilis* are themselves but different states of one and the same species; for that plants of *Chara hispida* which he had grown in a jar exhibited both states in the same stem, some branches being extremely simple and pellucid like *Nitella*, while others, as well as the main, had an envelope of spiral tubes, and were incrusted, striated and opake, partaking of the characteristics of *vulgaris* and *hispida*. The incrusting material consisting of carbonate of lime, Mr. Quekett supposes to be an accidental circumstance, depending upon the presence of that substance in the water, and that in the process of appropriation of the carbonic acid by the plant, the carbonate of lime becomes precipitated upon the stems in the form of minute crystals.

ROYAL IRISH ACADEMY.

[Continued from vol. xi. p. 136.]

February 13.—Dr. Gregory read a paper, entitled, "Examination of Eblanine, a substance discovered by Mr. Scanlan, and exhibited by him at the Meeting of the British Association*." By Professor Apjohn and Dr. Gregory.

Eblanine is contained in pyroxilic spirit. It is yellow, crystalline, fusible at 318°, volatile in a current of air at 300°, not subliming in a close tube unchanged. It is insoluble in water and alkalies, soluble with a strong yellow colour in alcohol, æther, and concentrated acetic acid. Strong sulphuric acid strikes with it a deep bluish purple colour, soon passing to brownish black. Strong muriatic acid dissolves it sparingly with a very fine and intense purplish red colour, which also slowly passes into brownish black. Nitric acid dissolves it, and from the solution water separates a yellow solid, which, at a certain temperature, is decomposed suddenly with a very feeble explosion. Chlorine converts it into a dark resinous matter.

Eblanine is anhydrous, and contains no nitrogen.

The mean of 4 analyses gave as the composition in 100 parts,

Carbon, ...	75·275
Hydrogen, ...	5·609
Oxygen, ...	19·116

The composition, calculated according to the formula $C_{21}H_9O_4$, would give

Carbon, ...	75·79
Hydrogen, ...	5·30
Oxygen, ...	18·91

But as we have as yet no means of ascertaining the atomic weight of eblanine, this result must be viewed merely as an approximation.

Eblanine cannot be confounded with any known substance, and must rank as a curious addition to the list of compounds produced in the destructive distillation of wood; to which must also be added, aldehyd, a substance lately discovered by Liebig, but first pointed out as existing in pyroxilic spirit, by Mr. Scanlan, who obtained it before the discovery of Liebig was known in Dublin.

Sir William Betham read the first of a series of papers "On the Cabiric Mysteries and Phœnician Antiquities."

February 27.—Professor Lloyd read a note on the Aurora Borealis of the 18th inst., of which the following is an extract:—

"At a quarter past ten o'clock, on the night of the 18th inst., my attention was called to a remarkable ruddy appearance in the eastern part of the sky, which, at first view, seemed to arise from the reflection of a fire. On a more attentive examination, however, it was soon evident that the appearance was purely meteoric. It was, in fact, an auroral phænomenon, though of a very peculiar kind.

"It was bright moonlight, and Mars had just appeared after his occultation by the moon. The sky was entirely without clouds; but the northern, eastern, and western segments were covered with a curtain of diffused Aurora, resembling a luminous vapour. This curtain was lifted from the horizon on the east and west, and exhibited a deep blue sky. But the distinguishing appearance was, that large masses of this light, especially towards the east and north-east, were of a *blood-red* colour, which presented a vivid contrast to the blue of the sky beneath. A large patch of this red light, about 40° from the horizon to the eastward, was the most remarkable. It continued distinctly visible for upwards of half an hour; and its motion was so rapid that in this time it had advanced from about due east to a point nearly south-east.

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* [See Lond. and Edinb. Phil. Mag. vol. vii. p. 395.]

"There was a mass of *white* streamers to the north, which reached nearly to the zenith, and pointed somewhere between the magnetic and due north. At half past ten o'clock, a brilliant and well-defined stream of light of the blood-red colour appeared a little to the south of west, and seemed to be a disjointed portion of the eastern red mass A few minutes after its appearance, a large mass of white auroral light began to rise rapidly from the northern horizon; at the same time the northern streamers became much more vivid, and took a fan-like appearance, converging to a point not far from the zenith. There was no appearance, however, of *Corona*. Shortly after (about $10^h\,40^m$), a portion of the light of these streamers, about midway between α Ursæ and Polaris, assumed the unusual blood-red tint, and continued of this colour for several minutes.

"Before 11 o'clock all the peculiar appearances had nearly gone; and there remained nothing but the faint luminous clouds, with light streamers to the N.N.W. These streamers were still playing at 12 o'clock, and extended from the zenith to within about 30° of horizon.

"The thermometer stood at 38° Fahr., and the barometer at 29·786 inches. The wind was dry and piercing*."

Professor Lloyd read a note on a new electrical phænomenon.

March 16.—This being the day of the annual election, the following Officers and Members of Council were chosen for the ensuing year:—*President*, Rev. Bartholomew Lloyd, D.D.[*]; *Treasurer*, Thomas Herbert Orpen, M.D.; *Secretary*, Rev. Joseph Henderson Singer, D.D.; *Secretary to Council*, Rev. Richard Mac Donnell, D.D.; *Secretary of Foreign Correspondence*, Sir William Betham; *Librarian*, Rev. William Hamilton Drummond, D.D.

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* The following note, by Mr. Bergin, supplies the account of the early part of the phænomenon:—

"On alighting at the Dunleary station at 7 o'clock, (from the Railway,) we observed a magnificently coloured crimson Aurora as a broad mass to the westward; and our first impression for a moment was, that it was the light from one of the engine furnaces reflected from a cloud of steam. It extended from near the horizon towards the zenith, with frequent flashes or streamers within itself. From the main mass, round by the north, and onward to the east, the whole sky had a crimson or carmine tint; and were it not for the brilliant moon (near the full) I do believe the splendour would have equalled any I have ever heard of. * * * * The Aurora assumed the general appearance of an arch; the first observed mass to the westward being one leg which faded away toward the zenith, where there was a steady circular patch of great brilliancy of colour, and from thence, separated by a small interval, was a faint limb descending to the eastern horizon. * * * These appearances continued with scarcely any change till near 8 o'clock. About 9 o'clock the general appearances were much the same, save that the eastern limb of the arch was not visible, and the western much more intensely coloured, and like a steady column. * * * * Throughout, its limits had been well defined; and it was perfectly transparent, stars of the third, and perhaps the fourth magnitude being seen through it." [See Lond. and Edinb. Phil. Mag., vol. x. pp. 206, 494.]

Committee of Science.—Rev. Franc Sadleir, D.D., Rev. Richard Mac Donnell, D.D., Sir William Rowan Hamilton, Rev. Humphrey Lloyd, James Apjohn, M.D., James Mac Cullagh, Esq., Captain Portlock, R.E.

Committee of Polite Literature.—The Archbishop of Dublin, Rev. Joseph Henderson Singer, D.D., Andrew Carmichael, Esq., Samuel Litton M.D., Rev. William Hamilton Drummond, D.D., Rev. Charles Richard Elrington, D.D., William West, M.D.

Committee of Antiquities.—Rev. James Henthorn Todd, Thomas Herbert Orpen, M.D., Hugh Ferguson, M.D., Sir William Betham, George Petrie, Esq., Rev. Cæsar Otway, Dean of St. Patrick's.

Professor Kane read a paper, entitled "Researches on the Combinations derived from Pyroacetic Spirit."

In order to understand the relation between the following bodies and pyroacetic spirit, the atomic weight of the latter must be considered as representing four volumes of vapour, and its formula written $C_6 H_6 O_2$. It has been found to give a series generally analogous to that of ordinary alcohol, and Professor Kane proposes for it the name *Mesitic Alcohol.*

By means of sulphuric acid there is obtained a colourless fluid, of an alliaceous odour, boiling at 276. F. and having the composition $C_6 H_4$, to which is given the name *Mesitylene.*

By acting on mesitic alcohol with perchloride of phosphorus there is generated *phospo-mesitylic acid*, and a compound fluid heavier than water, which has the formula $C_6 H_5 Cl$; and, by the decomposition of the latter by means of potash, a body $C_6 H_5 O$. These may be considered either as containing *Mesitylene*, or a hypothetic radical *Mesityl*, thus:

$C_6 H_5 + O$. Oxide of Mesityl.	$C_6 H_4 + H O$. Hydrate of Mesitylene.
$C_6 H_5 + Cl$. Chloride of Mesityl.	$C_6 H_4 + H Cl$. Muriate of Mesitylene.

By the action of phosphorus and iodine on mesitic alcohol, there is produced an *iodide of mesityl*, having the formula $C_6 H_5 I$.

Oxide of Mesityl unites with sulphuric acid in two proportions, forming the sulphate and the bisulphate of mesityl; both of these are acid, and unite with bases forming well characterized salts.

The salts of the former are called *sulphomesitylates*, and of the latter *persulphomesitylates*; and a very anomalous character in these salts is, that the quantity of the inorganic base is such as could neutralize the whole of the sulphuric acid which they contain. Thus the sulpho-mesitylate of lime has the formula

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[*] Dr. Lloyd having since deceased, Sir W. Rowan Hamilton has been elected President in his place.

$$SO_3 + C_6 H_5 O + Ca O + H O;$$

and the persulphomesitylate of lime

$$2S O_3 + C_6 H_5 O + 2Ca O + H O.$$

When an excess of phosphorus is used in the process for making iodide of mesityl, there is obtained in the retort a white matter in silky crystals, which dissolves in water, is very acid, and forms well characterized salts, which, when heated, take fire and burn with a well marked flame of phosphorus. This acid is termed *hypophosphomesitylous acid*; and the formula of the *hypophosphomesitylate of baryta* is

$$P_2 O + C_6 H_5 O + Ba O + H O.$$

In the decomposition of mesitic alcohol by perchloride of phosphorus there is obtained an acid which gives a soda salt crystallizing in rhombs which contain water of crystallization. Their formula is

$$P_2 O_5 + Na O + C_6 H_5 O + 6H O.$$

Professor Kane stated that he had obtained also the aldehyd of of the mesityl series, as well as bodies procured by the action of chlorine and iodine on mesitylene, and the acids which are generated by the oxidation of mesitic alcohol, the history of which bodies shall form the subject of another paper.

The empyreumatic oil, which is produced in small quantity when mesitic alcohol is prepared by distilling acetate of lime, has been submitted to analysis by Professor Kane, and its composition found to be $C_{10} H_8 O$. It therefore belongs to the family of which oil of turpentine is the base, and is polymeric with camphor, and the pinic, sylvic, and copaivic acids*.

Dr. Apjohn read a paper "On the Specific Heats of the Aeriform Fluids."

The first part of this communication was an analysis of, and some critical remarks upon, the labours of those who had preceded the author in the same investigation, particularly those of Dulong. Dr. Apjohn's own method was then detailed. In a paper read by him before the Academy in April, 1835, the equation† $f'' = f' - \frac{48ad}{e} + \frac{p}{30}$ was proved to include the solution of the dew-point problem. But the factor a in this expression, which is obviously equal (when the air or gas is dry, or in other words, when $f'' = 0$) to $\frac{f'e}{48d} \times \frac{30}{p}$, is the specific heat under a given volume of the gas which is supposed to be the subject of experiment. Hence if f' and d be determined for the various aeriform fluids by observation, their relative capacities for caloric can be compared. Such is the principle of the method.

Two distinct series of experiments were then detailed, from the second of which, as comprehending those which he conceives to be most accurate, the author has deduced the following table of specific heats :

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* In this abstract the atomic weights are taken, Hydrogen = 1. Oxygen = 8. Carbon = 6·13.

† $d = t - t'$ the difference of the temperatures shown by a wet and dry thermometer, and f' is the elastic force of vapour at temperature t'.

Specific Heats of equal Volumes.

Atmospheric Air,	1·000
Nitrogen,	1·048
Oxygen, (by calculation,)	·808
Hydrogen,	1·459
Carbonic Acid,	1·195
Carbonic Oxide,	·996
Nitrous Oxide,	1·193

Dr. Apjohn conceives himself justified in drawing from his researches the following conclusions :

1°. All gases have not under equal volumes the same specific heat.

2°. This law is not even true of the simple gases.

3°. There does not appear to be any simple relation between the specific heats of the gases, and their specific gravities or atomic weights.

A paper was then read "On some remarkable Salts, obtained by the action of Ferrocyanide of Potassium on Sulphovinates and Sulphomethylates." By William Gregory, M.D., F.R.S.E., &c.

When ferrocyanide of potassum is added to sulphovinate of lime, a precipitate appears, which, when heated, gives off hydrocyanic æther. This salt (called A) contains iron, calcium, potassium, cyanogen, and the base of æther.

The mother liquid is found to contain a salt B, very soluble in water and alcohol, which also, on being heated, yields hydrocyanic æther. The ingredients of B are sulphuric acid, potash, æther, and cyanogen.

In order to avoid the confusion which might result from the use of a salt of lime, (as Mosander has shown that ferrocyanide of potassium produces in the salts of lime, generally, a precipitate consisting of iron, calcium, potassium, and cyanogen,) the author next tried sulphovinate of potash. By the action of ferrocyanide of potassium on this salt he got a salt C, corresponding to A, but different; and another salt D, identical with B.

When sulphomethylate of lime was employed, two salts E and F were obtained, exactly analogous to A and B: and by employing sulphomethylate of potash he got G, corresponding to E, and H, identical with F.

As it seemed likely that the study of any one of these reactions would explain all the rest, the author began with the analysis of G and H, of which he had a larger supply than of the others.

G is lemon yellow, transparent, soluble in water, insoluble in alcohol, crystallizing in square tables much resembling those of ferrocyanide of potassium. By exposure to a heat of 212°, it loses 13·5 per cent. water of crystallization, and becomes opake. More strongly heated it is decomposed, giving off hydrocyanate of methylene, $= C_2 H_3 Cy$ or $Me\ Cy$. The analysis corresponds with the formula $4KCy, 3\,Fe\,Cy, M\,Cy, 8\,Aq$.

H is white, very soluble in water and alcohol, crystallizing in square shining tables. It closely resembles sulphomethylate of potash, but differs from it in being anhydrous, in containing cyanogen, and in yielding hydrocyanate of thylene when decomposed by heat. Its analysis agrees with the formula $6\,SO_3, 3KO, MO, MCy$

If 3 equivalents of ferrocyanide of potassium be supposed to act on 3 of sulphomethylate of potash there is the following equation:

3 equiv. Ferrocyanide 3 equiv. Sulphomethylate
$6\,KCy, 3\,FeCy + 6\,SO, 3\,KO, 3\,MO =$
1 equiv. G 1 equiv. H
$= 4\,KCy, 3\,FeCy, M\,Cy + 6\,SO_3, 3\,KO, MO, MCy$
$+ 2\,KO$. *i.e.* 2 equiv. potash. In conformity with this explanation, the liquid in which G crystallizes is alkaline.

If this explanation be admitted, it will of course apply, *mutatis mutandis*, to the salts A B, C D, E F. The author, however, is not yet satisfied that the salts which he analysed may not have been mixtures, perhaps in definite proportions. No doubt can be entertained that new salts have been formed, but the close resemblance between their properties and those of the salts which yield them, renders the task of purifying and analysing them one of great difficulty.

April 10.—A paper was read "On a new variety of Alumn," by James Apjohn, M.D., M.R.I.A., Professor of Chemistry in the Royal College of Surgeons, Ireland.

This paper commenced with a brief description of the physical characters and chemical properties of the mineral in question, which was found about 600 miles to the north of the Cape of Good Hope, near Algoa Bay, where it occurs in strata whose aggregate thickness is about twenty feet. The specimen described is composed of transparent threads or fibres, exhibiting a beautiful silky lustre, and in appearance closely resembling satin-spar or the finer forms of amianthus. In taste, solubility in water, and other properties, it corresponded with ordinary alumn. It was also easily shown to contain sulphuric acid and alumina, but in addition it contained a base which, though precipitated like alumina by potash, was not redissolved by an excess of the alkali. This, upon examination, turned out to be protoxide of manganese. There was no alkali, but about one per cent. of sulphate of magnesia.

In the first attempt at effecting the analysis of the mineral it was found that alumina and protoxide of manganese could not be separated perfectly by potash, as some of the oxide was taken up by the alkali, while a considerable quantity of alumina was left behind with the oxide. The author explained a method of overcoming this difficulty, the particulars of which are given in detail in the paper. The following are the results—the numbers in column (2) being the quotients got by dividing those in column (1) by the respective atomic weights:

	(1)	(2)	(3)
Sulphuric Acid, ...	32·79	·817	4·000
Alumina, ...	10·65	·414	2·026
Oxide of Manganese, ...	7·33	·205	1·003
Sulphate of Magnesia, ...	1·08		
Water of Crystallization, ...	48·15	5·350	26·315
	———100		

The numbers in column (3) being almost exactly the integers, 4, 2, 1, and 26, show that the substance analysed is a true alumn, having, as respects its acid and bases, the same formula

$$(3SO_3, Al_2O_3 + SO_3, MnO + 26HO)$$

with all the known species of that genus, and the same number of atoms of water with soda alumn. It differs from all those previously known in containing no alkali, this being replaced by protoxide of manganese. As an additional peculiarity Dr. A. observed that it did not appear susceptible of assuming the octohedral form.

The paper concluded with some remarks upon the probable existence of an alumn containing no metal but manganese, and upon certain difficulties in the doctrines of isomorphism, suggested by some of the varieties of this class of salts.

Captain Portlock brought under the notice of the Academy some peculiar habits of the *Otus Brachyotos* or short-eared owl, lately observed by Captain Neely, whilst collecting for the Ordnance Survey of Ireland.

This species of the sub-genus *Otus* being migratory, is much rarer than the *Otus vulgaris* or long-eared owl, and it differs from it in many striking respects, such as the small size of the elongated feathers, commonly called ears, (which in this species can only be discerned when the bird is living,) and in its tendency to diurnal habits. But in the instance now recorded it exhibits other peculiarities of habit which afford a still more remarkable line of distinction. The point of Magilligan, forming the Derry side of the opening of Lough Foyle to the sea, is studded at its extremity with numerous sand hillocks, in which the rabbits burrow and the sheldrakes lay their eggs, as in other similar localities. But here a new occupant for the burrows of the rabbits appears in the *Otus brachyotos*. These birds are regular their autumnal appearance, and are seen to sit at the openings of the burrow-holes, and to run into them when disturbed.

Captain Portlock having directed further attention to the fact, and pointed out the necessity of guarding against any source of fallacy, the truth of the first statement was fully established, more than one having been shot on emerging from the holes, and another actually caught in a trap at the mouth of a hole when endeavouring to make his escape. This interesting fact naturally recalls to recollection the *Strix cunicularia* of America, described by Say; and Captain Portlock pointed out the great value of characteristic traits of habit in elucidating classification, and suggested the peculiar importance of those described in his paper, in affording a link of resemblance between the *Strix cunicularia* and the *Otus brachyotos*, and thereby facilitating the determination of the true place, in natural classification, of the former, hitherto considered doubtful.

The Secretary communicated the substance of a paper "On the Conic Sections;" by James Booth, Esq.

The methods hitherto adopted in deducing the central and focal properties of the conic sections, from arbitrary definitions, having appeared to the author defective in geometrical elegance, he has endeavoured in this paper to derive them from new definitions, of which the following may be considered the principal:

1. If two spheres be inscribed in a right cone touching the plane of a conic section, the points of contact are called *foci.*

2. The radical plane of these two "focal spheres" intersects the major axis in a point called the *centre.*

The property from which the definition of a focus here given is derived, although known for several years, has not been hitherto applied further than to show that this point is identical with the focus as usually defined.

By the help of the above definitions, and of the simplest elementary principles, the central and focal properties already known have been deduced, generally in one or two steps, and several new theorems have been likewise discovered in the development of the method.

A paper "On Fluorine;" by G. J. Knox, Esq., and the Rev. Thomas Knox, was read by Dr. Apjohn*.

The authors having taken a summary view of all the researches on fluorine up to the date of the commencement of their experiments in April, 1836, proceeded to describe the vessels of fluor spar which they used in their first experiments, and exhibited those which were latterly found best adapted for examining the gas. These vessels were of fluor spar lapped with iron wire for the purpose of equalizing the temperature, so as to prevent the vessels from splitting on a sudden application of heat. In place of a flat cover for the vessels, fluor spar receivers were used, the cavities of which were filled with ground stoppers of the same material. On moving the receivers over the mouth of the vessel the stoppers fall in, and their places are occupied by the gaseous contents of the vessel. On the top of each of the vessels is placed a flat slab of fluor spar, which answers the purpose of a table, upon which the receivers of the gases can be moved. On the slab are four small depressions, in which are placed the substances upon which the action of the gas is to be observed, and over which the receivers when filled with the gas, can be slid. In opposite sides of these receivers are drilled holes, into which are fitted, air-tight, clear crystals of fluor spar, through which the colour of any gas in the receiver may be distinctly observed. The vessels are supported on a stand over a lamp.

On heating pure fluoride of mercury in these vessels with dry chlorine the authors obtained a colourless gas, (as seen through the fluor,) having a heavy smell not pungent or irritating, and thereby easily distinguished from chlorine or hydrofluoric acid. When exposed to the air it does not fume, as would be the case were the slightest trace of hydrofluoric acid present. The inside of the vessel is found coated with crystals of corrosive sublimate. The gas does not extinguish ignited phosphorus or red-hot iron wire†, and consequently is (as Sir H. Davy conjectured) a supporter of combustion. It detonates with hydrogen, forming hydrofluoric acid. Placed over water, the solution (if such) has all the properties of hydrofluoric acid, *i. e.* acts on glass, reddens litmus, and gives precipitates with lime and barytes. Placed over dry litmus and Brazil wood paper, the former is reddened, and the latter turned yellow; in no instance are they bleached. When a receiver of the gas is placed over wet glass, the glass is strongly acted upon; when the glass is carefully dried, the action is not so strong as before. When a small piece of dry glass is placed in a perforation in the interior of the receiver, the glass is acted upon, but not more so than when fluoride of mercury alone is in the vessel, from which they conclude that fluorine does not act on perfectly dry glass.

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* See Lond. and Edinb. Phil. Mag., vol. ix. p. 107.

† The non-extinction of ignited iron-wire in a gas cannot give evidence of its capability of supporting combustion.—Edit.

To ascertain the action of the gas on metals they found it necessary to try the separate effects of hydrofluoric acid, sublimed fluoride of mercury, and bichloride of mercury, in order to distinguish the action of fluorine from that due to the vapour of these substances. For this purpose bismuth and palladium at a moderate heat, and gold at a high temperature, afforded distinguishing tests. To determine the relative attraction of fluorine for those metals upon which it does not act except at high temperatures, they used as positive poles of a battery of sixty pairs of plates, moistened fluoride of lead, palladium, platinum, gold, and rhodium. The palladium and platinum were always acted upon, the gold occasionally, and the rhodium never; from which they suppose that fluorine might be obtained in an insulated state, by electrolyzing fluoride of lead in a tube of fluor spar, using rhodium as the positive pole.

They were unable to repeat M. Baudrimont's experiments in glass or fluor spar vessels*. Supposing that the gas he obtained was an oxide of fluorine, they heated in a dry glass tube iodic acid and fluoride of mercury; supposing that since iodine decomposes fluoride of mercury, the oxygen and fluorine being set free from their combinations with oppositely electrical bodies (iodine and mercury), would be in the most favourable condition for combining. On the application of a moderate heat a pale yellow vapour rose in the tube, which did not act on the glass, and bleached litmus.

Mr. Mallet read a paper "On an hitherto unobserved Structure discovered in certain Trap Rocks in the County of Galway."

The town of Galway is built upon a part of an immense trap dyke, which extends under the sea and to a considerable distance up Lough Corrib. Large excavations for a dock are now making in this rock at Galway, and afford a convenient opportunity of examining its structure. It separates the limestone on the east (which it tilts up) from the syenite of Cunnemara on the west, (which it overlies or mingles with.)

Many fragments of both adjacent rocks are found in an altered state imbedded in the trap; which with the tilting of the limestone, prove the deposition a true dyke.

The mass of the rock consists of greenstone, Sp. gravity 2·87, of a dark green, but frequently veined and mixed with many other minerals.

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* See Lond. and Edinb. Phil. Mag., vol. ix. p. 149.

In the centre of the exposed portion of the dyke rises a large vein of nearly white hornstone, presenting very interesting characters. It contains no imbedded minerals, and is homogeneous in structure, but with a lamellar or pseudo-crystalline arrangement. Its planes are vertical, and at its junction with the trap it is moulded to it, but not adherent, and appears to have been formed from rocks at a greater depth than the trap, and ejected through it. The minerals found imbedded in this trap rock are various; specimens have been obtained of mica, chlorite, felspar, albite, olivine, augite, amphibole, epidote, apatite, adularia, chalcedony, sulphate of lime, probably anhydrite, baryto-calcite, arragonite, calcareous spar, fluor spar, galena, iron pyrites, sometimes magnetic. Epidote is found also on Mutton Island.

The general mass of this trap rock possesses a hidden nodular structure, only developable by blasting. The nodules consist of precisely the same material as their matrix, and having the same cohesion, they cannot be detached by the hammer.

The nodules are from eighteen inches in diameter to the size of a nut; they are sometimes found pressed together in masses with flat sides, like bubbles. Crystals occurring at the surface of a nodule do not pass into the matrix, but are truncated thereby. In some cases the nodular structure is gradually obliterated, and the usual homogeneous one replaces it.

This nodular formation is essentially different from any hitherto described,—as the orbicular granite of Corsica and South of France, the onion stone of the causeway, &c., in which the nodule and the matrix are of different materials. The present structure would appear to have been produced by the ejection of the trap in a fluid state under the sea; masses of which cooling in their passage fell again into the liquid bed, and being enveloped, were heated nearly to the temperature of the mass, and so adhered without losing their outline. Where several fell together, and were exposed to subsequent pressure, they would present the flattened appearance before described; and when more deeply enveloped, and thus subjected to a higher temperature, the nodular structure would again vanish by their complete fusion.

It is even conceivable that the most capriciously varied parts of this and other trap rocks may owe their origin to the soldering together of nodules of heterogeneous matter, projected from different depths, or at different times, or subjected to successive coolings and heatings.

Professor Kane read a paper entitled "Researches on the Compounds derived from Pyroacetic Spirit." (Second Series.)

When dry chlorine gas is passed into pure mesitylene, $C_6 H_4$, muriatic acid is given off and a compound body, solid, in white prismatic crystals, is formed, giving on analysis the formula $C_6 H_3 Cl$. A yellow substance obtained by the action of iodine on nascent mesitylene, but in too small a quantity for analysis, is considered to be $C_6 H_3 I$.

When mesitylene is treated with nitric acid copious red fumes are given off, and a very heavy thick fluid obtained which gives on analysis the formula $C_6 H_4 O_2$. This fluid absorbs ammonia, and forms therewith a compound soluble in water, and giving with most metallic solutions insoluble precipitates.

If pure mesitic alcohol be heated with nitric acid, there is a very violent reaction, and an explosive decomposition, if distillation be attempted; but by diluting with water a heavy fluid is produced, which gives on analysis, unsatisfactory results, owing, in the first place, to its decomposing with an explosion when heated, and secondly to its being always mixed with some of the substance last described: the results obtained indicate however as very probable the formula $C_6 H_3 NO_4$.

To connect the above results, Professor Kane proposes to assume as radical the body $C_6 H_3$, to which he gives the name of pteleyl. Then

$C_6 H_4 = C_6 H_3 + H$. Hydruret of pteleyl or mesitylene.
$C_6 H_3 Cl = C_6 H_3 + Cl$. Chloride of pteleyl.
$C_6 H_3 I = C_6 H_3 + I$. Iodide of pteleyl.
$C_6 H_4 O_2 = C_6 H_3 O + H O$. Hydrated oxide of pteleyl, the aldehyd of the mesitic series.
$C_6 H_3 N O_4 = C_6 H_3 O + N O_3$. Hyponitrate of pteleyl.

The compound heavy liquid produced by the action of chlorine on mesitic alcohol, was found to differ but little from the description given by Liebig. Its formula, as given by Dr. Kane's analysis, is $C_6 H_3 O_2 Cl_2$; and by the action of bases it yields a metallic chloride, and a salt of a new acid named by Professor Kane *Pteleic Acid.* This has not yet been analyzed, but theory indicates for its composition the formula $C_6 H_3 O_4$.

By the action of permanganate of potash on mesitic alcohol, there is generated a neutral salt of potash containing an acid, to which is given the name of the *Perpteleic*, whose salts generally decompose themselves with facility into carbonates, and a salt of another acid to which the name of the *Acetonic Acid* has been applied. The constitution of these last three acids remains yet to be fixed by other experiments, the author confining himself in the present paper to the suggestion of that view of their composition, which, in the absence of positive analyses, seems to him most likely to be true.

Professor Kane exhibited to the Academy a balance made by a German artist, having some peculiarities of construction and adjustment.

April 24.—A paper was read by Professor Kane "On *Dumasine*, a new Fluid Substance isomeric with Camphor."

This fluid is obtained in very small quantity in the distillation of acetate of lime for preparing mesitic alcohol. It boils at 248°, is colourless, and of a powerful resinous smell. Its composition by analysis is $C_{10} H_8 O$. Thus:

	Experiments.	Theory.	
Carbon,	= 78·82	— 79·30	100·00
Hydrogen,	= 10·46	— 10·35	
Oxygen,	= 10·72	— 10·35	

The specific gravity of the vapour of this liquid was found to be 5·204, air being 1. The theoretical density from the formula above given is 5·315, and one atom forms two volumes of vapour. It has, therefore, the same density as camphor, and like it may be considered as consisting of

1 volume of vapour of oil of turpentine, = 4·7643
$\frac{1}{2}$ volume of vapour of oxygen, = 0·5513

1 volume of vapour of dumasine, = 5·3156

Professor Kane read some passages of a letter from M. Dumas, of which the following is an extract:

"* * * * The researches, of which you have given me an account*, promise the happiest results for science, I cannot too much encourage you to complete them; you will see by the journals that I have communicated your letter to the Academy of Sciences, where it met with the most honourable reception. Allow me to add, that M. Peligot and myself had obtained the carbo-hydrogen, $C_6 H_4$, as well by sulphuric acid as by anhydrous phosphoric acid. We had found that potassium gave the product $C_6 H_5 O$, which you have obtained in another manner, but we were stopped by the composition of the sulpho-mesitylate of baryta, of which you have given the explanation. These researches have been made some time, but other matters caused us to neglect them, and I do not now regret it, since they are in such good hands. * * * *"

"* * * * I announced yesterday to the Academy the existence of the *carbo-vinate of potash*, which is

$$C O_2 + C_4 H_5 O + C O_2.$$

I also obtained, conjointly with M. Peligot, the *carbo-methylate of baryta*, which is

$$Ba. O C O_2 + C_2 H_3 O + C O^2.$$

In these bodies the acid changes very readily into carbonic acid and alcohol, or pyroxylic spirit; and it is remarkable, that to form them it is sufficient to pass carbonic acid into a solution of baryta in spirit of wood, or of potash in ordinary alcohol. I do not doubt but that similar bodies can be obtained with pyroacetic spirit, but I shall leave to you the pleasure of isolating them. * * * *

"* * * * I shall communicate next Monday to the Academy, some observations which may interest you more than any other person; I mean on compounds very analogous to double chlorides, and which I have obtained by means of urea and the alkaline chlorides. Such bodies appear to me decisive on the theory of the amides. * * * *"

Sir William Betham read a paper "On the Affinity of the Phœnician and Celtic Languages, and on the Cabiri and their Mysteries."

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* On pyroacetic spirit. See Lond. and Edinb. Phil Mag., vol. x. p. 488, and the proceedings of the Academy for April 10 above. Prof. Kane's First Series of Researches on the same subject will be found at large in vol. x. p. 45 *et seq.*—Edit.

Sir William Betham then added a short notice of a Hindoo Legend from a paper in the Asiatic Researches, by Captain Wilford, showing that the Cabiric mysteries existed in India, under the names *Cubear* or *Cuvera*, *Asyoruca*, *Asyotcerso*, *Cashmala* and *Carmala*; and that these deities or genii superintended mining and metals.

Professor Mac Cullagh read a paper "On the Chronology of Egypt."

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: MEETING OF 1837, AT LIVERPOOL.

Section of Mathematical and Physical Science, Sept. 12.

Dr. Robinson, of Armagh, read a Report of the Determination of the Constant of Lunar Nutation from a discussion of the Greenwich Observations.

The author commenced by giving a sketch of the commencement of accurate astronomy, under the auspices of Bradley, by his brilliant discoveries of the aberration of light and the nutation of the earth's axis, demonstrating, that a degree of precision, until then deemed unattainable in astronomical observations, was perfectly possible. The impulse then given has not since ceased to effect the movements of astronomical discovery. Yet from this day, it must be acknowledged, that, in regard to both aberration and nutation, nothing was added to the researches of Bradley, until within a few years, when Struve, Brinkley, and Richardson resumed the inquiry. He then sketched the progress of each of these, and stated, that the constant of nutation deduced by Brinkley was that generally adopted by British astronomers. In Germany, however, the authority of Bessel had introduced and given currency to a different value for this important element of calculation, deduced from the calculations of Von Lindenau; and although the two values differ only one-fourth of a second, which is less than the millionth part of the length of the telescopes generally used in observing, yet such is the accuracy required in the modern researches of astronomy, that even this evanescent quantity of error is considered as a disgrace. This stigma, he trusted, was now removed by the work which the aid of the British Association had enabled him to perform, and of which he now intended to give a brief notice.

Dr. Robinson then referred to the labour of reducing observations, as actually taken, in consequence of the refraction of light, the aberration of the stars caused by the progressive propagation of light, the proper motions of the stars, and the united effect of all the movements impressed with the earth upon the actual place of the observer. Of these, the impressions upon the axis of the earth are pre-eminent, and the largest of them in amount has been long known under the name of the Precession of the Equinoxes—this is well known both as to its laws and amount; the remaining three are termed Nutations, of which one completes its course in a fortnight, and is never so large as one-tenth of a second—the theory of this is sufficiently known; a second completes its cycle in half a year, and when greatest may amount to half a second, and has been separately determined by the admirable observations of Dr. Brinkley; the third is the largest in amount, being about 9″, and completing its cycle in the time of a complete revolution of the moon's nodes, or about eighteen years, rather more: the exact determination of this was the object of the discussion of the observations of which he was now giving an account. He then proceeded to give a general description of the method of employing the observations, and the kind of observations to be selected for this determination, showing that it was most important to have a complete series of observations extending through the entire period of the moon's nodes, made with the same instruments, and if possible, by the same observer, or at least with the same system of observation. The observations made at Greenwich under the superintendence of the late Mr. Pond were those selected. The tables of Bessel were used; his values of declination, nutation, and proper motion were used, but Dr. Robinson had used his own values of aberration and refraction. Upwards of 4000 observations of the pole-star above were used, and in the results more than 2000 above and below were combined to give the zero of polar distance; the others were used to watch for and detect any change in the instrument. He then stated the principle on which the other stars were selected, viz. that their altitudes secured them from uncertainties in refraction, and that they should be such, that at least two-thirds of the nutation should exist in the direction of their polar distances; of such stars fifteen were in the Greenwich Observation, but some of these could not be used. They afforded about 8000 results, but 6000 only were used; an accident which occurred to the instrument in 1820 rendered useless about 1000 of these. The mean results of these observations being taken with the precautions which the paper pointed out at length, the results of some required that the value of Lindenau which is 8″·977 should be increased, while others required that it should be diminished; on the whole an increase of 0″·257 was acquired, giving the result 9″·234, which differed only by sixteen thousandths of a second from the number selected by Mr. Baily, and used in his invaluable Catalogue. The learned author then proceeded to notice and remove certain objections which he anticipated might be made to the details of his method of reduction; in the course of these he stated, that as the corrections of Bessel's proper motions which his work has given are all, except in one instance, negative, he inferred that the Greenwich circle was undergoing some progressive change of figure, making it show polar distances too great for about 30° south of zenith. If this were so, he observed, the sagacity of Mr. Airy would not permit it to be long undetected. He then read a table from these observations, showing that the declinations as obtained from his calculations, though they differed materially from those given by Pond himself in the Nautical Almanac for 1834, yet agreed closely with those of Bessel, thus showing, that the difference between these Catalogues arises solely from the different methods of reduction, and exciting the wish, that the British Association might lend its aid in reducing the whole of the Greenwich observations made by Pond.

Mr. Baily congratulated Dr. Robinson on the successful termination of his labours. He stated, that it was a curious fact, that Busch, of Berlin, had, from a series of observations made by Bradley at Wanstead, before he removed to Greenwich, computed the nutation, and given its value 9″·2347, thus differing by only 7 ten-thousands of a second from the value deduced by Dr. Robinson, although the cycle of observations was different, the instruments and places of observations and the observers were all different: this was a coincidence scarcely to be equalled in the annals of science.—Sir William Hamilton asked some questions regarding the manner in which the observations of the pole-star were used by Dr. Robinson as contrasted with the method adopted by Lindenau, and seemed satisfied with the answers which were given. He stated, that he felt great gratification at what he might almost call the complete solution of this important problem, and expressed his concurrence in the conclusion to which Dr. Robinson had come in consequence of the discrepancies between the calculated declinations and those given in the Nautical Almanac.

Mr. Russell then presented the Report of the Committee on Waves.—Mr. Robison and Mr. Russell, of Edinburgh, had been appointed at the Bristol Meeting of the British Association a Committee to prosecute an inquiry concerning the Mechanism of Waves, in which Mr. Russell had been previously engaged, and to extend their observations to the determination of the effect of the form of channel and of the wind upon the tidal wave. Mr. Lubbock and Mr. Whewell had already determined by their investigations the laws of the propagation of the oceanic tide, but it still remained to assign the law of propagation of the tide in those shallow seas and rivers where the bottom and sides of the channel exercise the principal influence on the propagation of the tidal wave. For the purpose of determining the effect of these circumstances upon the form, magnitude, and velocity of the tide wave, Mr. Russell had made in September, 1836, a series of observations on the River Dee, below Chester, where that river has a form and dimensions admirably suited to the purpose. It appears that for more than five miles in length the banks of the Dee are perfectly straight, quite parallel to one another, while the depth of the channel at low water is nearly uniform throughout the whole of that length. Now, in this river there is a tidal wave of from six to fifteen feet, forming, in fact, a tidal canal of large dimensions. On this part of the river the first series of observations was made. A second series of observations was made upon the River Clyde in April and May, 1837, under peculiar advantages. On the application of Sir Thomas Brisbane, a former President of the Association, the Trustees of the River Clyde offered to Mr. Russell all the assistance at their disposal, and every facility for making observations, which they conceived to be equally useful to practical navigation as to the advancement of abstract science; and their engineer, Mr. Logan had contributed much to the success of these observations. Accurate trigonometrical surveys of the channel of the river, with correct levels and transverse sections, were obtained, and a series of simultaneous observations made at nine different stations on the river. A series of observations had also been made on the waves at the surface of the sea, and the series had terminated by a course of experiments made in artificial channels of different forms, for the purpose of determining the nature of the mechanism of the generation and propagation of waves, so as to determine the identity of their nature with the tidal wave.

Of this series of experiments and observations the following are some of the results:—

It appears that there exists a species of wave different from all the others, and which Mr. Russell calls "The Great Primary Wave of Translation," which is generated whenever an addition is made to the volume of a quiescent fluid, in such a manner as to affect simultaneously the whole depth of the fluid, and this species of wave is exactly of the same nature as the tide wave. In a rectangular channel this primary wave moves with the velocity which a heavy body would acquire in falling through half the depth of the fluid, so that

In a channel about 4 inches deep, the velocity of the wave is nearly 2 miles an hour

——	12	——	——	4	——
——	2 feet deep		——	5½	——
——	3	——	——	6⅓	——
——	4	——	——	7⅔	——
——	5	——	——	8⅔	——
——	6	——	——	9⅓	——
——	7	——	——	10 1-5th	——
——	8	——	——	11	——
——	9	——	——	11½	——
——	10	——	——	12 1-5th	——
——	15	——	——	15	——
——	30	——	——	20	——
	&c.			&c.	

It also appears that the breadth of the channel, when the depth is given, does not at all affect the velocity or form of the wave; and Mr. Russell then proceeded to assign a general rule, by means of which the velocity of the wave might be assigned *à priori* for a channel of any form, however irregular.

The manner in which the wave was observed, was by successive reflections from opposite surfaces, so as to make it pass and repass a given station of observation, the interval being noted by an accurate chronometer; and it was stated that in many cases above sixty transits of the same wave had been observed, so as to give a high degree of accuracy to the observations. The instant of the wave's transit had been observed by the reflection of a luminous image thrown down by a series of mirrors, so as to cross micrometer wires with perfect precision. For a mode of determining the length of the wave Mr. Russell acknowledged himself indebted to Professor Stevelly, of Belfast.

These observations having determined the laws of the propagation of waves on a small experimental scale, were then extended to the analogous phænomena of the great tidal wave. In his observations on the River Dee, Mr. Russell found that the tide wave followed precisely the same laws as those in his experimental channel; that its velocity was exactly proportioned to the square root of the depth of the fluid, that its form changed in the same manner, and the existence of the same law was sufficient to account for the different rate of propagation of different tides between two given places, because a tide of fifteen feet deep would travel from one place to another at the rate of fifteen miles an hour, while one of ten feet deep would only proceed at the rate of twelve miles an hour; so that if the places were thirty miles apart, the one would receive the former tide two hours later, and the latter tide two and a half hours later than the other. The creation of a tidal bore in some places was also accounted for on the same principles; and it was evident that the means of improving the navigation of tidal rivers might be satisfactorily deduced from these principles.

Similar observations had been made on the tidal wave of the River Clyde, which was found to move in strict conformity with the laws of the great wave of translation, as determined by Mr. Russell's previous experiments.

The effect of the wind upon the tidal wave had been eliminated by Mr. Lubbock from the Liverpool observations, and had been denied by Monsieur Daussy in his discussion of the Brest observations. Mr. Robison and Mr. Russell had directed their observations to this also, and had ascertained that its effects were of the most decided character. It was, however, probable that during the ensuing year they would be able to determine the nature and the measure of these effects with still greater precision.

Mr. Whewell asked several questions of Mr. Russell respecting the method adopted for taking the level of the rivers in the Dee and in the Clyde.—Mr. Russell replied, that the level of mean tide was used in the Dee, and a fixed line was taken for many miles along the Clyde. Mr. Whewell also asked how the depths were taken, and how the agitation arising from the pressure of the waves was determined at various depths.—Mr. Russell replied, that the depths were taken by actual measurement, but that the pressures at various depths had only been taken on the small scale of the models. Mr. Whewell expressed much gratification at the methods of experimenting adopted by Mr. Russell; and although some of his conclusions seemed at present to be scarcely reconcileable with the theoretic views held on the subject, yet he anticipated that the utmost advantage would result from researches so ably conducted, and trusted that Mr. Russell would continue and extend them.—Sir William Hamilton fully concurred in the expressions of approbation which had fallen from Mr. Whewell.

Prof. Powell read a paper 'On Von Wrede's Explanation of the Absorption of Light by the Undulatory Theory.'

Von Wrede supposes the particles of a transparent body placed regularly at equal distances (b), and the ether being diffused between them, a ray of light is propagated directly through the medium, but a portion of each wave encounters some of the particles, and is reflected backwards, then forwards again, and emerges along with the direct ray; and from the retardation it has undergone it may interfere so as to produce darkness, if the retardation amount to an odd multiple of the half-wave length (λ), but brightness if an even multiple of that quantity. These effects may be confounded together in white light, and by prismatic analysis they will be seen as dark bands.

The author investigated a formula for the intensity of the light so resulting. It was deduced from the ordinary view of undulations, and brought into a form including certain constant terms, together with the factor $\cos 2\pi \frac{2b}{\lambda}$, which is so involved, that the intensity is a maximum when the cosine is $= +1$ or when $2\pi \frac{2b}{\lambda}$ is an even multiple of a semicircumference; and a minimum when the cosine $= -1$, or when the arc is an odd multiple of a semicircumference. Hence, if the medium be such that $2b = \frac{\lambda}{2}$ for any ray, that ray will be deficient. If $2b$ be less than the value of $\frac{\lambda}{2}$ for the violet ray, which is its least value, there will be no absorption: if greater, some ray will be at a minimum. Let us suppose $2b = n\lambda$, then passing from one end of the spectrum to the other, there will be changes of intensity dependent on the changes of the cosine between the limits $\cos 2\pi n$ and $\cos 2\pi n \frac{\lambda r}{\lambda v}$; maxima when $\cos = +1$, and minima when -1: the number depending on (n,) that is, on (b), which may be supposed as large as we please.

This investigation applying to a simple medium, the author showed that the expression for a compound of several media, with different values of (b), will still preserve the condition of depending on the changes of the cosine, and each medium will retain its own set of maxima and minima, which will be superposed in the spectrum.

Thus far the successive reflections had been considered as taking place only between two sets of particles or reflecting surfaces: the case was then investigated where several such were taken into account, and a formula resulted which was more complex, but whose maxima and minima depended on exactly the same conditions.

The author showed that the more regular phænomena of absorption are completely explained by this hypothesis, and, in one instance, even proceeded with success to a numerical comparison. He pointed out also an experimental imitation of the supposed process, which was perfectly successful.*

Sir David Brewster conceived that the theory of Von Wrede was entirely inadmissible. He stated many cases of absorption where there was not an appearance of reflection, as in the case of nitrous gas, which by mere changes of temperature became as black and as impenetrable to light as charcoal. Sir John Herschel had also noticed many cases of absorption without any trace of reflection; and only in the cases of some vegetable colours did he ever experience the contrary.—Professor Lloyd asked whether the changes might not result from partial changes of density caused in the substances by changes of temperature?—Sir David Brewster stated that it was impossible there could be any change of density in the case of the nitrous gas, as the changes in its temperature took place while its volume was secured from enlargement by its being sealed up in glass tubes. At one time he was inclined to think that some chemical change might have been effected upon the glass, but the phænomena did not long warrant this conclusion. The phænomena of absorption could be all had from plates of partially decomposed glass, such as that which had been long buried in the earth, but this was a case of real opalescence.—Sir W. Hamilton conceived that the views of Wrede required the confirmation of more exact numerical examination before they could be adopted; and he trusted that Sir David Brewster would give the inquiry the advantage of his great skill and experience.

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* A translation of the original memoir on this subject by M. Von Wrede will be found in the Scientific Memoirs, vol. i. p. 477.

Sir W. Hamilton then gave an account of his Exposition of the argument of Abel respecting equations of the fifth degree. Sir William stated that the celebrated young Swedish philosopher, Abel, whose labours (unfortunately for the cause of science) had lately terminated with his life, had at one time supposed that he had found a method of solving generally equations of the fifth degree, but soon finding that this solution was illusory, it occurred to him that perhaps under the conditions of ordinary algebra such a solution was an impossibility; as soon as he had started this thought he pursued it through a most intricate argument, and at length achieved what any one upon first hearing it would be apt to consider most chimerical—an *à priori* argument to prove that the solution of an equation of the fifth degree was, under the limitations of ordinary algebra, an impossibility.

The argument of Abel consisted of two principal parts; one independent of the degree of the equation, and the other dependent on that degree. The general principle was first laid down, by him, that whatever may be the degree n of any general algebraic equation, if it be possible to express a root of that equation, in terms of the coefficients, by any finite combination of rational functions, and of radicals with prime exponents, then every radical in such an expression, when reduced to its most simple form, must be equal to a rational (though not a symmetric) function of the n roots of the original equation; and must, when considered as such a function, have exactly as many values, arising from the permutation of those n roots among themselves, as it has values when considered as a radical, arising from the introduction of factors which are roots of unity. And in proceeding to apply this general principle to equations of the fifth degree, the same illustrious mathematician employed certain properties of functions of five variables, which may be condensed into the two following theorems: that if a rational function of five independent variables have a prime power symmetric, without being symmetric itself, it must be the square root of the product of the ten squares of differences of the five variables, or at least that square root multiplied by some symmetric function; and that if a rational function of the same variables have itself more than two values, its square, its cube, and its fifth power have each more than two values also. Sir William Hamilton conceived that the reflections into which he had been led were adapted to remove some obscurities and doubts which might remain upon the mind of a reader of Abel's argument; he hoped also that he had thrown light upon this argument in a new way, by employing its premises to deduce, *à priori*, the known solutions of quadratic, cubic, and biquadratic equations, and to show that no new solutions of such equations, with radicals essentially different from those at present used, remain to be discovered: but whether or not he had himself been useful in this way, he considered Abel's result as established: namely, that it is impossible to express a root of the general equation of the fifth degree, in terms of the coefficients of that equation, by any finite combination of radicals and rational functions.

What appeared to him the fallacy in Mr. Jerrard's very ingenious attempt to accomplish this impossible object had been already laid before the British Association at Bristol, and was to appear in the forthcoming volume of the Reports of that Association. Meanwhile, Sir William Hamilton was anxious to state his full conviction, founded both on theoretical reasoning and on actual experiment, that Mr. Jerrard's method was adequate to achieve an almost equally curious and unexpected transformation, namely, the reduction of the general equation of the fifth degree, with five coefficients, real or imaginary, to a trinomial form; and therefore ultimately to that very simple state in which the sum of an unknown number (real or imaginary) and of its own fifth power is equalled to a known (real or imaginary) number. In this manner the general dependence of the modulus and amplitude of a root of the *general* equation of the fifth degree on the five moduli and five amplitudes of the five coefficients of that equation, is reduced to the dependence of the modulus and amplitude of a new (real or imaginary) number on the one modulus and one amplitude of the sum of that number and its own fifth power; a reduction which Sir William Hamilton regards as very remarkable in theory, and as not unimportant in practice, since it reduces the solution of any proposed numerical equation of the fifth degree even with imaginary coefficients, to the employment, without tentation, of the known logarithmic tables, and of two new tables of double entry, which he has had the curiosity to construct and to apply.

It appears possible enough that this transformation, deduced from Mr. Jerrard's principles, conducts to the simplest of all forms under which the general equation of the fifth degree can be put; yet Sir William Hamilton thinks that algebraists ought not absolutely to despair of discovering some new transformation which shall conduct to a method of solution more analogous to the known ways of resolving equations of lower degrees, though not like them dependent entirely upon radicals. He inquired in what sense it is true that the general equation of the fifth degree would be resolved, if, contrary to the theory of Abel, it were possible to discover, as Mr. Jerrard and others have sought to do, a reduction of that general equation to the binomial form, or to the extraction of a fifth root of an expression in general imaginary? And he conceived that the propriety of considering such extraction as an admitted instrument of calculation in elementary algebra, is ultimately founded on this: that the two real equations,

$$x^5 - 10\,x^3\,y^2 + 5\,x\,y^4 = a,$$
$$5\,x^4\,y - 10\,x^2\,y^3 + y^5 = b,$$

into which the imaginary equation

$$(x + \sqrt{-1}\,y)^5 = a + \sqrt{-1}\,b$$

resolves itself, may be transformed into two others which are of the forms

$$\rho^5 = r, \text{ and } \frac{5\,\tau - 10\,\tau^3 + \tau^5}{1 - 10\,\tau^2 + 5\,\tau^4} = t,$$

so that each of these two new equations expresses one given real number as a known rational function of one sought real number. But notwithstanding the interest which attaches to these two particular forms of rational functions, and generally to the analogous forms which present themselves in separating the real and imaginary parts of a radical of the *n*th degree, Sir William Hamilton does not conceive that they both possess so eminent a prerogative of simplicity as to entitle the inverses of them alone to be admitted among the instruments of elementary algebra, to the exclusion of the inverses of all other real and rational functions of single real variables. And he thinks, that since Mr. Jerrard has succeeded in reducing the general equation of the fifth degree with five imaginary coefficients to the trinomial form above described, which resolves itself into the two real equations following,

$$x^5 - 10\,x^3\,y^2 + 5\,x\,y^4 + x = a,$$
$$5\,x^4\,y - 10\,x^2\,y^3 + y^5 + y = b,$$

it ought now to be the object of those who interest themselves in the improvement of this part of algebra, to inquire whether the dependence of the two real numbers x and y in these two last equations on the two real numbers a and b, cannot be expressed by the help of the real inverses of some new real and rational, or even transcendental functions of single real variables; or (to express the same thing in a practical or in a geometrical form) to inquire whether the two sought real numbers cannot be calculated by a finite number of tables of single entry, or constructed by the help of a finite number of curves: although the argument of Abel excludes all hope that this can be accomplished, if we confine ourselves to those particular forms of rational functions which are connected with the extraction of radicals.

Mr. Peacock observed that the Section were scarcely aware of, and could not be too strongly impressed with the value of an attempt like that of Sir W. Hamilton to render this celebrated argument of Abel intelligible to beginners, and even to advanced students in algebra. The constitution of most minds was such that they were anxious to run away from those subjects on which their labours could be profitably employed, and to engage themselves in the prosecution of curious and sometimes almost useless difficulties. He exemplified the celebrated resolution of the Academy of Sciences of Berlin, that they would in future receive no more communications on the subject of squaring the circle, as a remarkable proof of the extent of this morbid state of mind; for it was a fact that the average number of communications on this subject, when taken for many years, amounted to four annually. The rage for resolving mere algebraic difficulties was pretty much the same, and he, therefore, for one, felt that the gratitude of men of science was due to Sir W. Hamilton for thus giving an *à priori* argument, the obvious tendency of which was to save the laborious exertion of talent in fruitless research; a labour, for the employment of which such vast regions were at present opening before us in rich profusion. As it occurred to him, the chief advantage which he expected from the method adopted by Sir W. Hamilton was this: that whereas from its very intricacy the argument of Abel would be inaccessible to the ordinary algebraist, and a doubt therefore would always remain on his mind of the validity of the conclusion, and consequently he would be tempted even still more strongly to essay the difficulty for himself,—the method of Sir W. Hamilton, besides making the principle and many of the steps of the argument intelligible to all, and therefore giving a high degree of probability to the conclusion, has this peculiar advantage; that by applying the very same mode of arguing to quadratic, cubic, and biquadratic equations, it has not only proved that they are soluble by precisely the modes by which we at present resolve them, but it proves further, that they are insoluble by any other purely algebraic device. This seems to be conclusive, and must carry conviction to every mind.

Sept. 13.—Prof. Lloyd read an "Account of the Magnetical Observatory now in course of erection at Dublin."

In bringing this subject under the notice of the Section in its present stage, Mr. Lloyd said, that he trusted little apology was required. The establishment of permanent magnetical stations has been urged by the powerful recommendation of the British Association; and he was sure that that body would view with interest the progress of an undertaking, the importance of which was sanctioned by its authority.

The magnetical observatory, now in progress at Dublin, is situated in an open space in the gardens of Trinity College, and sufficiently remote from all disturbing influences. The building is forty feet in length, by thirty in depth. It is constructed of the dark-coloured argillaceous limestone, which abounds in the valley of Dublin, and which has been ascertained to be perfectly devoid of any influence on the needle. This is faced with Portland stone; and within, the walls are to be *studded*, to protect from cold and damp. No iron whatever will be used throughout the building. With reference to the materials, Professor Lloyd mentioned, that in the course of the arrangements now making for the erection of a Magnetical Observatory at Greenwich, Mr. Airy had rejected bricks in the construction of the building, finding that they were in all cases magnetic, and sometimes even polar. Mr. Lloyd has since confirmed this observation, by the examination of specimens of bricks from various localities; and though there appeared to be great diversity in the amount of their action on the needle, he met with none entirely free from such influence.

The building consists of one principal room, and two smaller rooms,—one of which serves as a vestibule. The principal room is thirty-six feet in length by sixteen in breadth, and has projections in its longer sides, which increase the breadth of the central part to twenty feet. This room will contain four principal instruments, suitably supported on stone pillars: viz. a transit instrument, a theodolite, a variation instrument, and a dipping circle. The transit instrument (four feet in focal length,) will be stationed close to the southern window of the room. In this position it will serve for the determination of the time; and a small trap-door in the ceiling will enable the observer to adjust it to the meridian. The theodolite will be situated towards the other end of the room, and its centre will be on the meridian line of the transit. The limb of the theodolite is twelve inches in diameter, and is read off by three verniers to ten seconds. Its telescope has a focal length of twenty inches, and is furnished with a micrometer reading to a single second, for the purpose of observing the *diurnal variation*.

The variation instrument will be placed in the magnetic meridian, with respect to the theodolite, the distance between these instruments being about seven feet. The needle is a rectangular bar, twelve inches long, suspended by parallel silk fibres, and inclosed in a box to protect it from the agitation of the air. The magnetic bar is furnished with an achromatic lens at one end, and a cross of wires at the other, after the principle of the collimator. This will be observed with the telescope of the theodolite, in the usual manner; and the deviation of the line of collimation of the collimator from the magnetic axis will be ascertained by reversal. The direction of the *magnetic* meridian being thus found, that of the true meridian will be given by the transit. It is only necessary to turn over the transit telescope, and, using it also as a collimator, to make a similar reading of its central wire, by the telescope of the theodolite. The angle read off on the limb of the theodolite is obviously the supplement of the variation. This use of the transit has been suggested by Dr. Robinson; and it is anticipated that much advantage will result from the circumstance, that the two extremities of the arc are observed by precisely the same instrumental means. With this apparatus it is intended to make observations of the *absolute variation* twice each day, as is done in the observatory of Professor Gauss, at Göttingen,—the course of the *diurnal variation*, and the hours of maxima and minima, having been ascertained by a series of preliminary observations with the same instrument.

A dipping circle constructed by Gambey will be placed on a pillar at the remote end of the room; and will be furnished with a needle, whose axis is formed into a knife-edge, for the purpose of observing the diurnal variations of the dip. Gauss's large apparatus will also be set up in the same room, and will be used occasionally, especially in observations of the *absolute intensity*, made according to the method proposed by that distinguished philosopher.*

The bars are too large to be employed in conjunction with other magnetical apparatus.

It is intended to combine a regular series of meteorological observations, with those on the direction and intensity of the terrestrial magnetic force just spoken of; and every care and precaution has been adopted in the construction of the instruments.

In conclusion, Mr. Lloyd said, that he felt it a duty to allude to the liberality and zeal in the cause of science which had been evinced by the Board of Trinity College on this occasion. The probable expense of the building and instruments is estimated at 1000*l.*; and that sum was immediately allocated to the purpose, when it appeared that the interests of science were likely to be benefited by the outlay.

Mr. Peacock congratulated the Section upon the prospect held out to the scientific world, of having fixed magnetical observatories erected in such places as would afford the surest promise of successful co-operation, particularly when they would be placed under the superintendence of gentlemen so eminently qualified for the task as Professor Lloyd. He informed the Section, that an observatory for magnetical observations had been erected at Greenwich, and that little doubt need be entertained of the rapid advances which the interesting investigations connected with this important science would now receive.—Mr. Ettrick conceived, that bricks would be a very improper material for the construction of a magnetical observatory. He considered the use of metals in any part of the building as highly objectionable; even copper as fastenings or hinges to doors would not be free from injurious effect. He made some inquiries as to the mode of reading off, proposed by Professor Lloyd.—Prof. Stevelly said, that Mr. Ettrick was unquestionably right in the objection urged against the use of bricks, but Professor Lloyd had distinctly stated, that bricks were not to be used, and that experiments had been made to ascertain the precise magnetical influence, if any there was, of the kind of stone which it was proposed to use. It was well however, that Mr. Ettrick's observations should go abroad, for the guidance of persons not conversant with these subjects. Bricks, when built into large edifices, such as the chimneys of factories, were well-known to have acquired magnetic polarity: the material from which they were made must be largely impregnated with iron: the mud of rivers was the detritus from hills, whose rocks were often highly magnetic. The engineers employed on the trigonometrical survey of Ireland had erected a mound of stones composed of basalt, to sustain the signal-staff which they had erected on the highest hill, near Belfast; the effect of that heap of stones on the magnetic needle was so great, that in walking round it the needle would veer round to every point of the compass.

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* Prof. Gauss's description of his apparatus and mode of observing will be found in Lond. & Edinb. Phil. Mag., vol. ii. p. 291.

Prof. de la Rive having read a paper on the interference of electro-magnetic currents, in which, among other facts, it was stated that when wires of platina are employed to transmit the magneto-electric currents into any solution, the abundant evolution of gas which is at first observed diminishes, and after fifteen or twenty minutes disappears; Professor Andrews, of Belfast, observed, that there was one portion of the detail upon which he thought he could throw some light, by mentioning a fact with which he had lately become acquainted. If the poles of a galvanic arrangement of low tension, say a single pair of plates charged with weak acid, were made to communicate with two broad slips of platina immersed in water, no action whatever would take place; but if one of the broad slips was replaced by a fine wire, the pole which it represented would give off the appropriate gas, whether oxygen or hydrogen; but the broad slip at the other pole would give off none whatever—or whichever pole it might be connected with. Now the appearance of the gas at one pole was a clear proof that water was decomposed; and therefore the gas which must be developed at the broad slip must be dissolved in the liquid, or otherwise prevented from assuming the gaseous form. Persons not acquainted with this fact might infer, that there was no decomposing action exerted; when, in fact, as appeared plainly upon a more extended view of the phænomena, there was.—Mr. Lubbock inquired, from Professor Andrews, whether the hydrogen disappeared as well as the oxygen; for, although water can condense oxygen in considerable quantities, he was not aware that any considerable quantity of hydrogen could be so condensed.—Professor Andrews replied that either would be given off at the fine platina wire, according to the pole you made it to represent, and neither would appear at the broad plate, showing that each would in turn be dissolved, or otherwise detained in the fluid.*

M. de la Rive read a second paper 'On an Optical Phænomenon observed at Mont Blanc.' When the sun has set at Geneva, it is observed that Mont Blanc remains illuminated by its direct rays for a much longer time than the surrounding mountains. This phænomenon is owing to the great height of Mont Blanc. But after it has ceased to be illuminated the summit of Mont Blanc sometimes reappears at the end of ten or fifteen minutes, less intensely enlightened than at first, but nevertheless in a manner very decided, and often very brilliant. This phænomenon takes place especially when the atmosphere is very pure—highly charged with aqueous vapour in an invisible state—and consequently very transparent. The author has satisfied himself (by the exact observation of the time which elapses between the two successive illuminations of the mountain, combined with the calculation of the sun's progress) that the phænomenon is due to the rays of the sun which traverse the atmosphere at a distance from the earth less than the height of Mont Blanc, but greater than half that height, and which arrive at rarer regions of the atmosphere under an incidence so great that they are reflected instead of refracted. This interior reflection is facilitated by the humidity of that part of the atmosphere which the rays traverse until they reach the point of incidence. The reflected rays falling on the snowy summit of Mont Blanc produce this second illumination; and the humidity (by augmenting the transparency of the air) renders the illumination more brilliant.

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* On this subject see Mr. Faraday's observations, Lond. & Edinb. Phil. Mag., vol. iv., p. 291.

Sir D. Brewster stated that he had witnessed a similar effect, though on a less magnificent scale, on the Grampian Hills; but he had always observed that on such occasions the sun set in a red west, and that all the clouds in that quarter of the heavens were then red.—M. de la Rive replied, that the phænomenon he spoke of only appeared when the sky was quite free from clouds, and, in truth, it was most brilliant when the air was very transparent in consequence of its being loaded with vapour in its elastic state.—Professor Lloyd said that the distinctness and vividness with which distant objects were seen in some states of the atmosphere was quite astonishing: on one occasion he had seen from the neighbourhood of Dublin the Welsh hills from their very bases, and brought so near apparently, that he could absolutely see the larger inequalities of the surface upon the sides of the mountains. That the atmosphere was at the time very much loaded with vapour in a highly transparent state, was obvious from the fact that immediately after a very heavy fall of rain took place, and continued for a considerable time.—Professor Stevelly wished to confirm what had fallen from Professor Lloyd and M. de la Rive by stating that whenever the Scotch hills appeared with that peculiar vividness and distinctness, from the Lough of Belfast, the fishermen always looked upon it as a sure precursor of heavy rain and wind. A friend had informed him that on one occasion he had noticed this appearance while standing on the beach at Hollywood, and pointed it out to an old fisherman; the old man immediately gave notice to all his friends to whom he had access, who instantly set about drawing up their boats and placing their small craft in more secure places; early the next morning a violent storm came on, which did much damage upon the coast to those who had not been similarly forewarned. Thus we find that the most interesting pursuits of the man of science, and the most important concerns of man in the practical details of life, frequently approach, and each may lend important aid to the other.—Mr. Lubbock was of opinion, that the principal fact mentioned by M. de la Rive would receive a simple solution, if we admit the theory of Poisson regarding the constitution of the atmosphere. That eminent mathematician conceived that analysis led irresistibly to the conclusion that the upper portions of the atmosphere were, by the extreme cold there existing, condensed into a liquid or even into a solid: if this were so, we could easily conceive how the reflection of the light from its under surface would re-illuminate the top of Mont Blanc after the direct light of the sun had ceased to reach it.—Sir David

Brewster expressed much surprise at hearing for the first time of this theory of Poisson, and that he should feel much obliged to Mr. Lubbock if he would give some details of it in a separate communication to the Section; and he had little doubt but that it would be as new and as acceptable to many gentlemen as to himself. He thought that the near apparent approach of distant objects in certain states of the air, as mentioned by Professor Lloyd and Professor Stevelly, might perhaps be accounted for by supposing that on these occasions the intervening air became actually converted into a large magnifying lens.

Section of Chemistry and Mineralogy, Sept. 13.

Dr. Apjohn next exhibited a new variety of Alum, upon the subject of which he had lately read a paper before the Royal Irish Academy.* This mineral, which he received from Mr. Atherton, an African gentleman, was found on the eastern coast of the African continent, about midway between Graham's Town and Algoa Bay. It occurs in fibrous masses very similar to asbestos, having a beautiful satiny lustre, and splitting into threads which would appear to be quadrilateral prisms. In taste, solubility in water, and relation to several re-agents, it closely resembles ordinary alum, but is distinguished from it by containing protoxide of manganese instead of an alkali, and by not assuming the octahedral form. In symbols it is represented by

$$(3\,S_3O+Al_{2\,3}O)+(S_3O+Mn\,O)+25\,H\,O,$$

a formula identical with that which belongs to the entire genus of alum salts. Dr. Apjohn briefly alluded to the other varieties of alum, both those in which the alkalies replace each other, and those in which the alumina is replaced by the deutoxide of iron, chrome, or manganese; and pointed out the theoretical possibility of an alum containing no metal but manganese.

This communication gave rise to much discussion. Dr. Faraday stated, that a specimen of the mineral in question was given to him in London, that he had found it to contain oxide of manganese, and that on this account, and because of the absence of an alkali, he hesitated to admit it as a true alum; and that supposing, as conjectured by Dr. Apjohn, a double salt should be formed, in which the alumina and alkali of ordinary alum were replaced by equivalent quantities of the deutoxide and protoxide of manganese, he could not admit it to be considered as an alum at all.—Dr. Clarke took up a different ground, and objected to the term alum being applied to the salt in question, inasmuch as it could not be made to assume the octahedral form.—To the first objection Dr. Apjohn replied, that he considered the mineral he had examined to be an alum, because, according to his analysis, its composition accorded with the general formula for alum; and to the second, that the other well-known alums presented other difficulties of as great magnitude as respects the laws of isomorphism. That *e. g.* soda alum crystallizes as an octahedron, though soda is not usually considered isomorphous with the other alkalies; and that, though the different varieties of alum assume the same form, they are not by all chemists considered to contain the same amount of water of crystallization.—Professor Johnston concluded the discussion by suggesting, that the difficulties which had arisen might be easily surmounted, simply by gentlemen agreeing upon a definition of what did or did not constitute a true alum.

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* See p. 203.

Section of Geology and Geography, Sept. 13.

A paper by Mr. Henwood was read, 'On some of the Phænomena of Mineral Veins in Cornwall.' This gentleman has for several years examined these phænomena in that important mining district, often at great personal risk, and has from time to time communicated to the geological world many extraordinary facts which he had determined. He brought before the Section his present statement, in the shape merely of a question for solution, as important to the Cornish mining interest. In mines veins are often heaved out of their regular course, and are traversed by cross courses, causing further irregularity. It was a desideratum of the first importance, that a law should be determined by which a heaved vein could again be discovered; and the opinion of geologists was requested on this point. In Cornwall, it is also the opinion among miners, that veins are of contemporaneous formation with the rocks containing them, which opinion is opposed to that of geologists in general, who consider veins, in most cases, as caused by disruption from mechanical force. Mr. Henwood exhibited a diagram of heaves in the mines of Dolcoath and Huel Prudence, which he submitted to the Section for solution by means of mechanical movement. He himself was inclined to consider the veins in these mines as of contemporaneous origin with the rocks in which they are found. The heaves he had ascertained to be in one and the same direction; and he conceived that, had they been caused by a mechanical force, they would be found in different directions.

Mr. Hopkins believed there was no difficulty in solving Mr. Henwood's question, by reference to the operations of mechanical force, but several data were necessary at the outset. He was fully aware of the difficulty of miners recovering lost veins, and that it was highly desirable to have rules laid down for doing so. He entered into a disquisition upon the formation of veins in general, and first stated the theories that had been proposed to account for them. One was, that of the Cornish miners, as above stated. Another, that fissures in the rocks had taken place after their consolidation, which fissures had become filled subsequently with mineral matter: these fissures being either open cracks, or simply discontinuities of the strata. Mr. Hopkins referred to his paper in the Transactions of the Cambridge Philosophical Society, where the subject was treated mathematically.* He considered the idea of the contemporaneous origin of veins as having no claim to the name of a theory, from it assigning no physical cause —it does not even call in the aid of electric agency. Suppose even that a small mass of matter were acted on and modified by fire, it might present veins within it; but still no theory is assigned why these veins are so produced: here there is the action of fire, and consequently, after being acted on, the phænomenon may be regarded as effected by igneous agency;—yet how can this explanation be applied to sedimentary rocks?—to say, therefore, contemporaneous formation, is to use an unmeaning term. Suppose it may be ascribed to molecular attraction, still the cause is wanting; and although it might be of some avail in explaining the formation of globular masses, yet it is of no usé in the present case, as we have to do with masses occurring in planes. In order to solve Mr. Henwood's question, we must know the angles which the veins make with each other, and also with the horizon, and the relative height of the strata on each side of the lines of dislocation. Mr. Hopkins considered that Cornwall was an unfavourable county for explaining these phænomena, as its rocks were generally unstratified; and he would prefer a county like Derbyshire, where the regularity of the stratification offered much facility in finding the necessary data.—Mr. De la Beche considered Cornwall as perfectly suited for explaining these phænomena on Mr. Hopkins's theory. In this county there exists a fossiliferous system, which has manifestly been upheaved by the granite, and is penetrated by the same granite veins which traverse the primary rocks.—Mr. Phillips said that we should not restrict the term contemporaneous, the real point urged by the Cornish miners being, that there was no displacement. Still we must seek for explanation in other districts. In these, the mechanical theory must be at once acknowledged to be true, when we see the disturbance in fossiliferous rocks, in some cases even in an intersection of their organic remains. But this theory ought not only to explain the direct phænomena, but must account for the exceptions; and in such cases, as in Cornwall, we must regard the structure of the rocks—their regular divisions and joints, which are independent of stratification,—veins may even occur in these divisions. In the north of England, the contents of veins are found to vary according to the containing rocks; and he considers the circumstance of spar stuff occurring in the Cornish veins as not opposed to the idea of mechanical force, but as dependent upon electric agency.—Mr. J. Taylor, jun. stated, that, in the course of his experience in practical mining, he had observed certain conditions necessary for the profitable working of metals. In the oldest, or scar limestone, he had observed that the miner was not remunerated; but in newer lead measures he had a better chance of success, as in grits and shales. The best chance was in altered rocks. In Cardiganshire he had observed a remarkable case in a slaty rock: where very schistose, the workings were poor; but where the rock was *diced*, as the workmen call it, they were certain to be rich; the strike of the altered rock being N. and S., and that of the veins E. and W. He had seen remarkable proofs of the mechanical theory in North Carolina, especially in the rich veins of iron ore of that country.—Mr. Sedgwick instanced the discussion now before the Section, as a proof of how much one branch of science was assisted by another: we here saw the application of Physics to Geology; he could record also the assistance rendered by Geology to Physics. This same Dolcoath mine, whose phænomena of veins were so singular, was the one selected by Prof. Airy for determining the density of the earth. Mr. Sedgwick remarked, that fissures caused by crystallization were, in general, very small; and that joints seldom coincided with rents;—that in districts where granite approaches slate rocks, we may be certain of finding the richest metalliferous deposits. Practical miners had often whimsical ideas of the origin of metals. One of them had once gravely informed him that they were caused by *peat*, and had shown what he conceived to be a convincing proof, namely, their existence under a peat bog in his neighbourhood, and occurrence nowhere else in the same vicinity.—Sir W. R. Hamilton testified also to the importance of new applications of mathematico-physical science. Not only would new views open, but even new methods of analysis would arise to assist the investigator.

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* Mr. Hopkins has subsequently given a view of his researches in Lond. and Edinb. Phil. Mag., vol. viii. p. 227.

Dr. W. H. Crook made some observations on the unity of the Coal Deposits of England. The object of this communication was to show that the coal-fields of England and Wales were not *distinct* basins, but that the supposed basins were only portions which had been detached and elevated by the agency of syenitic and trap rocks, of a much larger deposit, spread over the greater part of the districts now covered by the new red sandstone. Of the vegetable origin of coal there is now no doubt: the only question unsettled is, did the plants supplying it grow on the spots where it is found, or were they transported? Dr. Crook inclined to the latter opinion, and conceives that this view may be extended to the coal of Belgium, of the north of France, and the north-west of Germany; the carboniferous beds of those countries having originated, in his opinion, in a drift of vegetable substances from countries lying to the east or E.S.E. of them; and he also thought, that the extent and richness of the English coal-fields, especially in the Midland counties, arose in a considerable degree from the impediments offered to the transit of the drifted matter by the slate and other ancient formations of Wales and Cumberland. He considered, that the Charnwood Forest rocks had elevated the coal-field near it, and a similar elevation had taken place at Nuneaton.

Mr. Greenough considered the idea of Dr. Crook as very probable; but observed, that the deepest of our coal basins had been found to be in South Wales.—Mr. Young, from Nova Scotia, stated, that large deposits of coals had been found in that country.—*Athenæum, No.* 517.

XX. *Reviews and Notices respecting New Books.*

Electricity; its Nature, Operation, and Importance in the Phænomena of the Universe. By W. Leithead, *Secretary to the London Electrical Society.* London, 1837, 12mo.

WE had occasion to notice lately an observation by a distinguished foreign contemporary, in which the writings of some of our countrymen in the department of electricity were treated as having contributed but little to its advancement. We have since, however, in addition to a Journal of Electricity, had the establishment of an

Electrical Society of London; and the work now before us being announced as the production of the Secretary of that Society, we opened it with anxious expectation, to see how the national honour would be sustained by a personage placed in so conspicuous a station. Every one would of course not merely give credit to such a person for knowing something of what he was writing about, but expect from him some addition to our stock of knowledge; public attention having been directed to the work by advertisements, fortified by the following eulogium, from no unfriendly hand, in the Atlas: "This treatise exhibits the first attempts to call attention to the extraordinary relations between the electrical conditions of the atmosphere and the human body" [most extraordinary indeed, as we shall find]; "and it combines the *rapid spirit of composition* with the accuracy of cautious research, vigour and eloquence of expression with the strength of deep thinking." Of one of these subjects of panegyric, "the *rapid spirit of composition*," it might seem that only the author himself could well be cognisant; but as it has been our fortune to obtain a glimpse of Mr. Leithead's method, we shall give our readers the benefit of it. They will no doubt recollect, as we happened to do while perusing Mr. Leithead, the excellent treatise on electricity by Dr. Roget in the Library of Useful Knowledge, making two numbers, or 64 pages, out of which we cannot help suspecting that the worthy Secretary has manufactured the first 120 pages of his book. At least the coincidence between the two is throughout nearly as remarkable as that which the following collation exhibits.

Library of Useful Knowledge.	*Mr. Leithead.*
(§ 63) From what has already been explained of the general laws of electricity, the mode in which these machines act will readily be understood. The friction of the cushion against the glass cylinder produces a transfer of electric fluid from the former to the latter; that is, the cushion becoming negatively, and the glass positively, electrified. The fluid which thus adheres to the glass, is carried round by the revolution of the cylinder; and its escape is at first prevented by the silk flap, &c.	P. 49. From what we have already said respecting the general laws of electricity, the *modus operandi* of these machines will be readily understood. By friction between the cushion and the glass ....a transfer of the electric fluid takes place from the former to the latter; .... the cushion becoming negatively, the glass positively, electrified. The fluid which adheres to the glass, is carried round by the revolution of the cylinder; its escape being at first prevented by the silk flaps, &c.

To exemplify further his *modus operandi* (a favourite expression with the Secretary), we give two other sections from Dr. Roget, interlining them with a few alterations of phrase here and there, which if substituted for what stands under them in the text, will give us precisely the product of Mr. Leithead's "*rapid* spirit of composition, accuracy of cautious research, vigour and eloquence," &c. &c. &c.

Library of Useful Knowledge.

Leithead, p. 61. electricity along
"(§. 86.) The passage of (the electric fluid through) a perfect con-
It when the
ductor is unattended with light. (Light) appears only (where there

course of the electric fluid is impeded by
are obstacles in its path by the interposition of) imperfect conductors;
of its passage, under such circumstances,
and such is the velocity (with which it is transmitted,) that the
light is visible
(sparks appear to take place) at the very same instant along the
This may be illustrated by pasting a row
whole line of its course. (Thus if a row of small fragments) of tin-
discs of a small size (fig. 18.)
foil (be pasted) on a piece of glass (fig. 19.)"

[Here the figure is copied, and so are many of the others, only reversed, or slightly altered.]

Library of Useful Knowledge.

Leithead, p. 28. Now it must be remembered these cases
"(§ 105) (It should be recollected) that in all (the changes we have
thus traced as the effects of induction, there has been) no transfer of
the electric fluid has taken place between
(electricity) (from either of) the bodies (to the other);
which is the experiment in which
(as was sufficiently) proved, (indeed,) by (their taking place equally
the glass plate is
if) (a plate of glass be) interposed. Another proof is afforded by the
circumstance that the mere removal of the bodies to a distance from
each will its own
(one an)other, (is sufficient to) restore each of them to (their) original
in as positive a state
state. The globe remains (as positively electrified) as before; the
resumes state there
cylinder (returns to) its (condition) of perfect neutrality; (nothing)
no loss, no gain
has been (lost,) (and nothing gained) on either side. The experi-
over and over again, the phænomena
ment may be repeated (as often as we please,) (without any variation
will not vary. the effects would be different
in the phænomena). But (this would not be the case) if the cylinder
was
(were) divided in the middle, and one or both of the parts were removed separately, while they still remained under the influence of the globe."

The above are at any rate very amusing specimens of the art of ringing the changes upon words and sentences, sometimes at the expense of style and grammar; but how insufficient for the purpose of disguising wholesale piracy will be quite evident to any who may be disposed to follow up the comparison from page to page throughout the first part of the work, and to admire the gravity with which our Secretary struts about in his borrowed feathers. With the "accuracy of cautious research," he discovers the two above-mentioned sixpenny Numbers in Paternoster Row, and with "the strength of deep thinking" he makes with his pen the erasures and interlineations which we have exhibited above.

The first 120 pages being made up in this way, whence the other portion of the book is derived we have not taken the trouble to ascertain. Before we turn to the second part, we will just quote a passage from the Preface: "That it has no pretensions to the title of a scientific treatise is self-evident—(who will doubt this?)—the author's object in the first part of the work, being simply so to explain the several varieties of electrical action as to enable the non-scientific reader to understand the *modus operandi* (!) of the electric fluid in producing the different phænomena which are treated of in the *second* part." Some may be rather curious to know now of what the second part is made up, after the statement of "one so humble in the ranks of science"; and they will no doubt be astonished to learn that it treats of nothing less than cholera morbus, phænomena of disease, inflammation, fever; the *modus operandi* of the electric fluid in imparting their forms to vegetables and animals! and that it is neither more nor less than an electrical dream of Mr. Leithead's fancy. And all this for non-scientific readers, in a treatise on electricity. The cholera morbus is made to depend on aurora borealis, shooting stars, &c.; and the vital principle to be probably identical with the electric fluid. Mr. Leithead, moreover, sees "no reason why water should not be composed of hydrogen and oxygen in the planet Mercury, as is the case on our earth," for he conceives "that the degree of force with which an atom retains its electro-polarity on any of the heavenly bodies, will bear some exact ratio to their distance from the sun."—p. 397.

We have nothing further to say of this notable production excepting that its price is eight shillings, whilst the two numbers of the Library of Useful Knowledge are sold for sixpence each.

XXI. *Intelligence and Miscellaneous Articles.*

ABSORPTION OF WATER BY EFFLORESCENT SALTS.

MR. HUGH WATSON in a paper read before the Philosophical Society of Manchester, observes, "Hitherto I have considered those salts which are usually described as efflorescent salts, to be such as would effloresce whenever they were left exposed to an atmosphere not saturated with vapour, but capable of evaporating water; and I believe that the same idea is generally entertained on the subject. Now the result of my investigation furnishes the proof that such an idea is not a correct one; it shows that the crystallized sulphate and carbonate of soda, which are generally considered as very efflorescent salts, may be left exposed to the atmosphere for any length of time without efflorescing in the least, or losing a single particle of water of crystallization, though that atmosphere be dry and capable of evaporating water, *so long as its evaporating power is not allowed to extend beyond a certain point*; this point differs for each salt.

In order to find how much water pure anhydrous carbonate of soda is capable of absorbing, I, on the 20th October, 1835, put 47·4 grains = 1 atom (supposing the atom of soda to be 28, and that of the carbonic acid to be 19·4) prepared by calcining the bicarbonate into a watch-glass of known weight, and left it exposed to the atmosphere in a room in which no fire was kept. By frequently weighing the glass and its contents I found the gain of weight to be as follows:

		Weight gained.			Weight gained.
Oct. 21	....	3·3 grains.	Oct. 27	....	18·6 grains.
22	....	6·1	28	....	21·1
23	....	9·1	29	....	23·6
24	....	11·8	30	....	26·6
25	....	13·8	31	....	28·6
26	....	16·1			

The salt having concreted into a rather hard mass was now broken up, so that it might the more readily acquire more moisture.

		Weight gained.			Weight gained.
Nov. 1	....	31·6 grains.	Nov. 3	....	38·6 grains.
2	....	34·1	4	....	40·8

The salt was now removed from the watch-glass, crushed and spread over the surface of a dinner plate, so that the process might be sooner finished.

		Weight gained.			Weight gained.
Nov. 6	....	56·1 grains.	Nov. 16	....	78·6 grains.
11	....	77·6	21	....	78·6
14	....	78·6			

I left it exposed to the atmosphere till the 4th December, when I found it to be of the same weight as on the 21st November. It was therefore evident that no more water could be absorbed.

Now the 78·6 grains of water absorbed are only 1·4 grain short of being equivalent to ten atoms*, and it is probable that a waste of that quantity of salt may have been made in the numerous weighings. If this be granted, the author says he is right in concluding that the anhydrous carbonate acquires water until it is of the same constitution as the crystallized carbonate, if exposed to such an atmosphere as he made use of, one in which the temperature ranged from 53° to 43°, and whose vapour point is not more than 7° below the temperature nor less than 5°. The vapour point was ascertained by the aid of an hygrometer on the principle of Leslie's, and described by the author at the meeting of the British Association at Edinburgh.

On the 20th of October, 100 grains of crystals of carbonate of soda were exposed in the same room as that above alluded to; by one day's exposure 2¼ grains were lost, and no further loss occurred although the exposure was continued till the 4th of December. Mr. Watson therefore concludes that the water was merely adherent, and not combined.

In a room in which a moderate fire was regularly kept Mr. Watson exposed 20 grains each of crystals of sulphate of soda, of crystals of

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[* Only 0·4 gr. short of 10 atoms, if the usual equivalents are adopted, 47·4 : 78·6 : : 54 : 89·6.—Edit.]

carbonate of soda, and of anhydrous sulphate of soda; the first from the 30th of October to the 4th of December, and the two last from the 31st of October to the 4th of December. The crystals of sulphate of soda began in one day to effloresce, and on the 9th of December they had lost 9¾ grains; and Mr. Watson concludes that if the exposure had been continued they would have become anhydrous. The anhydrous sulphate gained no weight. On the 1st of November, the crystals of carbonate of soda were found neither to have lost weight nor to have effloresced in the slightest degree; on the 5th, some of the outside crystals were slightly effloresced, which appeared to be in consequence of the lowness of the vapour point on the 2nd (11° below the temperature), and also on the 4th (10° below the temperature); on the 11th, these crystals were no more effloresced than they were on the 5th; by the 10th December, however, efflorescence was considerably more apparent, though even then it had not made a great advance; in the interval between the 11th of November and the 4th of December, the vapour point had several times been 10° and 11° below the temperature, which was undoubtedly the cause of this further efflorescence.

The conclusion drawn by Mr. Watson from the above and other similar experiments is, that the crystals of carbonate of soda begin to effloresce at the temperature of 58°, when the vapour point is at 48°, and that the crystals of sulphate of soda begin to effloresce at the temperature of 58°, when the vapour point is at 49°; and therefore, that the carbonate may be left exposed to the atmosphere at the temperature of 58° when the vapour point is not lower than 49°, and the sulphate when the vapour point is not lower than 50°, without any of their water of crystallization being lost; and since the atmosphere in those states of dryness is capable of evaporating uncombined water, we have beautiful means afforded of providing ourselves with the salts in question free from any water not belonging to the constitution of the crystals, but at the same time, with all that does belong to it. As the crystals of sulphate of soda only begin to effloresce at the temperature of 58° when the vapour point is 49°, Mr. Watson considers that when the vapour point is only at 50° then the affinity of space for vapour is just equal to the affinity of this salt for its water of crystallization. The ordinary phosphate of soda appears to possess the same efflorescing property as the carbonate.

DETECTION OF COMMON SALT IN CHLORIDE OF POTASSIUM.

Mr. Watson in the paper from which the above extract is made, observes that it is sometimes a desideratum to ascertain if and to what amount muriate of potash [chloride of potassium] (an article much used in the manufacture of alum) is adulterated with common salt. The means, he observes, which would hitherto be used to accomplish that object are tedious. If, however, a solution of a sample of the kind in question be treated with sulphate of ammonia till all the chloride is converted into sulphate, and the resulting mixture be evaporated to dryness, and calcined till all the ammoniacal salt is dissipated, the residue will be the anhydrous sulphate of the alkali or alkalis of the sample; by placing this under an exhausted receiver along with a vessel of water, we shall be able to ascertain whether it is pure sulphate of potash or mixed with sulphate of soda; if it be the former it will gain no permanent weight however long the experiment may be continued; but if there be any of the latter present, it will gain weight till that portion becomes of the same constitution as it is of when it exists in the crystallized state.

NEW ACETATE OF LEAD.

M. Payen produced to the Academy of Sciences a regularly crystallized specimen of a new acetate of lead. It is composed of three atoms of neutral acetate and one atom of trisacetate; its formula is represented by

$$3\ \mathrm{PbO},\ \mathrm{C}^{8}\,\mathrm{H}^{6}\,\mathrm{O}^{3}+3(\mathrm{PbO},\ \mathrm{C}^{8}\,\mathrm{H}^{6}\,\mathrm{O}^{3}).$$

It is distinguished from the neutral and trisacetate by its crystallization in hexagonal plates, which form slowly, by exposing the solution, in radiating mamellated crystals; it crystallizes very abundantly so as to become almost a mass on cooling, whilst the trisacetate crystallizes with difficulty. Its solubility in water and of alcohol of various strengths, both cold and hot, is much greater than that of the two other acetates; thus at 65° Fahr. water dissolves about four times as much of it as of the neutral acetate and six times as much as of the trisacetate. It has an alkaline reaction; it is more stable than the first acetate and less so than the second. A saturated aqueous solution dissolves either of the other acetates, and acquires by so doing the consistence of a syrup, and thus retards or prevents all crystallization. An equal volume of anhydrous alcohol does not decompose this solution, whereas it causes the other acetates to appear in their respective solutions. When heated it suffers igneous fusion only, while the neutral acetate undergoes two successive fusions and the trisacetate does not fuse at all. It does not lose any of its acid in a dry vacuum, whereas the neutral acetate loses a portion of it on cooling. Carbonic acid decomposes an atom of the trisacetate of lead, and converts it entirely into neutral acetate. This new acetate can, on the contrary, dissolve hydrated or anhydrous oxide of lead and become complete trisacetate. By the addition of ammonia, according to the proportion and temperature, the new acetate yields either trisacetate, or anhydrous protoxide of lead, or crystallized hydrate.

This acetate is prepared by dissolving three atoms of neutral acetate in a solution of one atom of trisacetate; after due evaporation the solution is to be allowed to crystallize, and this occurs in three or four days after the solution has become cool; the crystals are to be drained, pressed in blotting paper, and dried in vacuo; a syrupy solution remains. M. Payen observes that the existence of this new acetate explains why several authors have stated that the subacetate of lead is more soluble than the acetate, while others have shown the contrary: this has happened because the former have observed the solubility of the new acetate or of its mixtures, while others have operated on the trisacetate.

It also appears why the neutral acetate effloresces, and by losing a portion of its acid in the air acquires an alkaline reaction, then combining gradually with carbonic acid is gradually converted into this new subsalt, and then into carbonate. This alteration, the first step of which occurs even in air deprived of its carbonic acid and in vacuo, explains the difficulty which all chemists have experienced in obtaining a solution of neutral acetate which is not precipitated by carbonic acid. Lastly, it is owing to the formation of the subsalt by the spontaneous disengagement of the acid, which occurs in the manufactories of acetate of lead, that the mother waters are rendered uncrystallizable, and this would occasion great loss, if an excess of acetic acid was not employed. It is evident that the disappearance of only one part of acid renders a solution of about 20 parts of acetate syrupy.—*L'Institut*, Nov. 1837.

[As the symbols merely and not the analysis of this new subacetate appear in the above extract, the exact composition of the salt is not very easily arrived at. I suspect that it is a compound of 4 equivalents of acid +5 equivalents of oxide, which are equal to 3 equivalents of acetate and 1 eq. of diacetate of lead.—R.P.]

SULPHURET OF AZOTE.

M. Soubeiran obtains sulphuret of azote by the action of ammonia upon chloride of sulphur; the dried ammoniacal gas is passed into a large receiver, in which there is placed a capsule containing a small quantity of chloride of sulphur, which is renewed when the action is over; a dirty green flocky matter is formed, which is exposed for 24 hours to an atmosphere of ammonia; the product of this operation is a mixture of hydrochlorate of ammonia and sulphuret of azote; it is treated with water, which dissolves the ammoniacal salt only.

The success of this operation requires several precautions; the chloride of sulphur must be saturated with chlorine; the temperature must be prevented from rising by the action of the ammonia upon the chloride of sulphur; on this account a large receiver should be used, and small quantities of chloride of sulphur must be employed at a time; the ammonia must always be in great excess; the mixture of sulphuret of azote and hydrochlorate of ammonia should be quickly washed; the sulphuret of azote must be dried, first by pressure between folds of blotting-paper and afterwards in vacuo.

The following are the principal properties of sulphuret of azote; it has a lemon yellow colour; is inodorous; at first it is tasteless, but it soon imparts a very distinct acrid taste. It detonates violently by percussion, or by the sudden application of heat. If it be mixed with some inert body it decomposes quietly, at about 284° Fahr., into sulphur and azote. It is slightly soluble in water; but it is gradually converted by heat into hyposulphate of ammonia; it is more soluble in alcohol and æther than in water. When the æther is very pure and dry, after its evaporation, it leaves crystallized sulphuret of azote. The alkalis convert it quickly into ammonia and hyposulphate; with the acids it yields ammonia, sulphur, and sulphurous acid. Sulphuret of azote is formed of two atoms of azote (two volumes) and three atoms of sulphur. It corresponds, in the series of the sulphurets, to the acid of the nitrates in the series of oxygenated bodies; it is nitrous acid, in which oxygen is replaced by sulphur. Sulphuret of azote possesses the general character of the amides; by combining with water it changes into ammonia and an acid.—*L'Institut*, Nov. 1837.

XANTHOPHYLLE,—THE COLOURING MATTER OF LEAVES IN AUTUMN.

Berzelius immersed the lemon-yellow autumnal leaves of the *Pyrus communis*, immediately after they were gathered, in alcohol of sp. gr. 0·833, and they remained in it for 48 hours. The alcohol became of a yellow colour, but the leaves were still yellow, though paler than at first. The alcohol was decanted, and the bottle was kept for some time inverted: the leaves, whenever they were acted upon by the air, became brown, whereas the parts of the leaves in contact with the bottle retained their yellow colour. Alcohol was repeatedly poured upon the leaves, and was each time rendered yellow; and lastly, the leaves were boiled in alcohol: it still acquired a slight yellow tint, but became gelatinous on cooling. The various alcoholic solutions were mixed and distilled to 1–8th; there was then deposited a granular and somewhat crystalline substance; after this was separated, the distillation was continued, until there remained only the water of the vegetation of the leaves. On this yellowish brown liquor there floated a soft, yellow, greasy substance, which appeared identical with the grains containing the yellow colouring matter of the leaves. These grains do not appear when examined by the microscope to possess any crystalline structure; when rubbed by the finger they become a yellow unctuous greasy matter; this is mixed with a small quantity of fat oil, which could not be separated with any certainty, with another greasy substance also; the greater portion of the former may be separated by digestion with potash, which saponifies the oil and dissolves but little of the yellow grease; the yellow fat acids are precipitated from the alkaline solution by hydrochloric acid; and by redissolving them in water containing in each ounce about five or six drops of ammonia, and again precipitating, they may be obtained colourless. In order to deprive it of the solid fatty matter, it must be treated with cold alcohol, in which it is not soluble. It was not possible to separate these two fatty matters. As it was procured it was a yellow unctuous fat, readily fusible and liquefying at 108° Fahr.; it then concretes, becomes transparent and yellowish brown; it cannot be volatilized without decomposition, and when distilled it yields a brownish fat, which is slightly soluble in alcohol, and leaves a residue of carbon. It is insoluble in water, but if when melted, hot water be poured upon it, it becomes transparent, swells slightly, assumes a paler yellow colour, as if water had chemically combined with it. When sprinkled with water and exposed for a long time to the air and light it becomes colourless, and a fatty matter is formed which is with difficulty soluble in alcohol, and when dissolved in it by heat it precipitates light white flocks as the solution cools. The yellow fat is also but slightly soluble in alcohol. It does not sensibly become colourless in the solution, in the same time that it bleaches with water. The alcoholic solution is decomposed by water; it then has a pale yellow milky appearance, which it is difficult to render clear, and it retains this appearance after the evaporation of the alcohol. By spontaneous evaporation it separates from the alcohol in the state of a granular crystalline mass. Æther dissolves it in large quantity, and it remains, after the evaporation, transparent and of a yellow colour. When mixed with sulphuric acid it becomes brown; a small quantity, altered in properties, is dissolved; the solution is yellowish brown, and is precipitated by water of greyish white. By caustic potash it is but very slightly dissolved, and the solution when exposed to air and light becomes colourless; acids throw down pale yellow flocks from the potash, which when properly washed do not redden litmus. It is but little, if at all soluble in carbonate of potash, and is insoluble in caustic ammonia, to which, however, it imparts a yellow colour. This yellow colouring matter is then a peculiar fatty matter, intermediate as to the fat oils and the resins, which may be bleached and retain its property of dissolving in alcohol with difficulty. It may be called *Xanthophylle* (from ξανθος, yellow, and φυλλον, foliage). Berzelius thinks that there is every reason to suppose, that during the disappearance of the green colour of leaves, and its conversion to a yellow colour, this is derived from the green, by means of some change in the organization of the leaf, effected by cold, which modifies the organic action; he tried in vain to reproduce the green colour from the yellow, nor could he convert the green to yellow. There is nothing in common between the brown and the yellow colour of the foliage; the former is produced by an extractive principle which is at first colourless, and which after the disorganization of the epidermis of the foliage, becomes brown by the action of oxygen; it then communicates to the fibre of the skeleton of the foliage a brown colour, which cannot be removed even by digestion in caustic potash, nor destroyed by long treatment with sulphuretted hydrogen.—*Journal de Pharmacie*, July 1837.

DETECTION OF METALLIC CHLORIDES IN BROMIDES AND IODIDES. BY M. HENRY ROSE.

It is very easy to discover small quantities of soluble bromides and iodides in great quantities of metallic chlorides; but on the contrary, it is very difficult to discover minute quantities of metallic chlorides in large quantities of metallic bromides and iodides.

After many experiments, M. Rose succeeded in discovering very small quantities of chloride in bromide of sodium; when bromide of sodium is mixed with excess of chromate of potash, and the mixture is distilled with concentrated sulphuric acid, pure bromine only is given out, which dissolves in excess of ammonia, forming a solution which is perfectly colourless. If chloride of sodium be treated in the same manner, there is obtained chromate of chloride of chromium, the colour of which very much resembles that of pure bromine, but which, when dissolved in ammonia, forms a solution of a deep yellow colour, in which the usual tests easily discover the presence of chromic acid.

In order to detect a chloride in a metallic bromide, it must be mixed and powdered with bichromate of potash; this mixture is to be put into a small tubulated retort, to which a receiver containing a sufficient quantity of solution of ammonia is adapted by a cork; there is then poured upon the mixture an excess of fuming [concentrated?] sulphuric acid, and the retort is to be heated.

In employing only 0·012 of a gramme of chloride of sodium mixed with 0·640 of a gramme of the bromide, indications of chromic acid were obtained in the ammonia, but in this case it was not by the colour that it was recognised; it was detected by evaporating the ammoniacal solution to dryness and exposing the residue with a phosphate to the flame of the blow-pipe upon charcoal; heated to redness. But if greater quantities of a mixture containing a smaller proportion of the chloride be employed, a yellowish ammoniacal solution may be obtained; this was done with a mixture of 0·053 gramme of the chloride with 0·580 gr. of the bromide. M. Rose endeavoured, but without success, to determine the quantity of the chlorine by this process. If iodide of potassium be mixed with excess of chromate of potash, and heated with sulphuric acid, iodine only is disengaged; if a mixture of iodide of potassium with chloride of potassium or sodium be treated in the same manner, no chromate of chloride of chromium is obtained, but chlorine is first evolved, and afterwards vapour of iodine, so that no chloride of iodine is formed. It is only when the proportion of metallic chloride is greatly in excess, that chromate of chloride of chromium is procured; 12 parts of iodide of potassium with 60 parts of the chloride impart a slight yellow colour to the ammoniacal solution; but 12 parts of iodide with 20 or 30 of chloride, did not yield a trace of chromium to the ammoniacal solution. On account of this singular property it is not possible to discover the presence of the chloride in the iodide of potassium, by the same process as that by which it is detected in the bromide. The best method of detecting small quantities of metallic chloride in the iodide of potassium is that proposed by M. Gay Lussac, founded upon the very slight solubility of iodide of silver in ammonia. Add a solution of nitrate of silver to a solution of the salt, until no further precipitation takes place, then add ammonia in excess. If after agitation and filtering, only opalescence occurs in the liquor on supersaturating it with nitric acid, it is a proof that the iodide of potassium contains no chloride or but a trace; this last salt would be discovered by the precipitation of chloride of silver when supersaturated by nitric acid. —*Journal de Pharmacie*, October 1837.

ON THE EMPLOYMENT OF METALLIC SULPHURETS IN ANALYSIS.

BY MONS. E. F. ANTHON.

It is well known that certain oxides possess the property of precipitating others from their solutions, by combining with the acid of the dissolved oxide; and this process has been adopted for the separation of certain metallic oxides.

Metallic sulphurets, prepared in the usual way, may be employed in the same way as the oxides, for precipitating oxides from their solutions; the latter are then converted into sulphurets, whilst the metal of the sulphuret continues in the state of oxide with the acid, previously united with the metal precipitated; this action of the sulphurets frequently possesses advantages in chemical analysis.

The results obtained by employing eight metallic sulphurets will be stated; they were prepared either by precipitation with sulphuretted hydrogen or an alkaline hydrosulphate. In operating on the solution of a salt by a sulphuret, the sulphuret was always used in excess, and the mixture was exposed to a boiling heat for about a quarter of an hour.

Sulphuret of Lead precipitates nitrate of silver, sesquichloride of iron, nitrate of copper; it does *not* precipitate nitrate of cobalt, nitrate of cadmium, nitrate of manganese, sulphate of nickel.

Sulphuret of Cobalt precipitates acetate of lead, sesquichloride of iron, sulphate of cadmium, sulphate of copper, nitrate of nickel, nitrate of silver; it does *not* precipitate sulphate of manganese.

Sulphuret of Iron precipitates nitrate of lead, sulphate of cadmium, sulphate of copper, nitrate of silver; it does *not* precipitate nitrate of cobalt, sulphate of manganese, nitrate of nickel.

Sulphuret of Cadmium precipitates nitrate of lead, sulphate of copper, nitrate of silver; it does *not* precipitate nitrate of cobalt, sesquichloride of iron, sulphate of manganese, nitrate of nickel.

Sulphuret of Manganese precipitates acetate of lead, nitrate of cobalt, sesquichloride of iron, sulphate of cadmium, sulphate of copper, nitrate of nickel, nitrate of silver.

Sulphuret of Copper precipitates nitrate of silver; it does *not* precipitate acetate of lead, nitrate of cobalt, sesquichloride of iron, sulphate of cadmium, sulphate of manganese, nitrate of nickel.

Sulphuret of Nickel precipitates acetate of lead, sesquichloride of iron, sulphate of cadmium, sulphate of copper, sulphate of silver; it does *not* precipitate nitrate of cobalt, sulphate of manganese.

Sulphuret of Silver does not precipitate acetate of lead, nitrate of cobalt, sesquichloride of iron, sulphate of cadmium, sulphate of copper, sulphate of manganese, nitrate of nickel.

It will be observed on examination that sulphuret of manganese decomposes all the solutions of metallic oxides tried, while the sulphuret of silver did not decompose any one whatever; it results from these facts that if silver has the strongest and manganese the weakest affinity for sulphur, all the other metals are intermediate as to these, and arranged according to their degrees of affinity for sulphur; they stand thus: silver, copper, lead, cadmium, iron, nickel, cobalt, manganese.

The metals are here so arranged that any one of them in state of sulphuret does not act upon a solution of the metals following: thus for example, the sulphuret of nickel precipitates the salts of silver, copper, lead, cadmium, and iron, but effects no change in those of cobalt and manganese.

There is only one exception, it is that the sulphuret of iron precipitates the nitrate of lead, whilst the sesquichloride and pernitrate of iron are only partially precipitated by the sulphuret of lead.—*Journal de Pharmacie*, October 1837.

ON THE FERMENTATION OF SUGAR OF MILK. BY M. HESS.

It has generally been admitted by chemists as an indisputable fact, that sugar of milk is incapable of fermentation. Pallas endeavoured, but in vain, in his collection of historical facts respecting the people of Mogol, (St. Petersburgh, 1776, vol. i. p. 33), to controvert this generally received opinion, by objecting that the people of Asia prepared an intoxicating liquor from milk; although this fact was known to many persons, and has often been quoted respecting milk, the idea has nevertheless remained, that sugar of milk is not susceptible of the alcoholic fermentation, and it has been proposed to expunge it from the list of sugars, and to give the name of *lactin*.

In order to elucidate this subject by some experiments, M. Hess fermented cow's milk in wooden vessels; the fermentation occurred spontaneously and without any addition; it is necessary only that the vessels should be sufficiently deep and exposed to a sufficiently high temperature; it is of no consequence whether the milk be previously skimmed or not. The fermentation continues for a long period; it is accompanied with a disengagement of gas, perceptible even by the ear. The gas collected was examined by solution of potash, which absorbed it within $\frac{1}{100}$ part, which could be nothing but atmospheric air; the fermented liquor was passed through a flannel, to separate the ferment, and afterwards distilled. The product of the distillation was acid; it was saturated with carbonate of lime, and several times redistilled, taking care to receive each time only one-fourth of the liquid. The liquor thus obtained was mixed with excess of dry carbonate of potash, which combined with the water, and an alcoholic liquor floated on the saline one; it was separated by repeated distillations from the salts which it contained; then, in order to have it pure, it was rectified from lime; it always possessed a peculiar odour. By analysing 0·48 of this liquid there were obtained 0·827 of carbonic acid, and 0·561 of water, which gives for 100 parts:

Carbon	47·64	
Hydrogen	12·96	
Oxygen	39·40	
		100·

and as 47·64 of carbon indicate 90·46 of alcohol, which are equal to

Carbon	47·64	
Hydrogen	11·66	
Oxygen	31·16	
		90·46

there remain 1·3 of hydrogen, which indicate 11·81 of water, from which there results an excess of 2·27 in 100. As M. Hess performed this analysis with care, and believed he was sufficiently guarded from the usual source of error, that is, of hygrometric moisture, he presumed that there was present in the liquor some combination containing more hydrogen than alcohol does; and it is proved by the experiments of Dœbereiner that ammonia is formed during fermentation. A solution of chloride of platina produced so abundant a precipitate in the liquor of ammonio-chloride of platina, that the author almost suspected an accidental error. The experiment was then repeated with a certain quantity of alcohol recently prepared from milk; the precipitate was collected on a filter, dried and then heated to redness in a glass tube. The large quantity of muriate of ammonia obtained, left no doubt on the subject in question. M. Hess also determined that the peculiar odour was derived from an admixture of ammonia. Then in order to obtain pure alcohol, he first separated the water by lime and then distilled it with a salt water bath, at a very low temperature, with a few drops of sulphuric acid. The liquor obtained had however a weak æthereal odour; 0·513 gave 0·993 of carbonic acid and 0·596 of water, which give for

100 parts: Carbon ..	53·48	but alcohol contains	52·66
Hydrogen	12·90	.. ..	12·90
Oxygen..	33·67	.. ..	34·44
	100·		100·

The æthereal odour sufficiently explains the cause of the slight excess of carbon, and it appears, in fact, that the alcohol obtained is identical with common alcohol, but in order to be quite certain of this, M. Hess mixed it with an equal weight of sulphuric acid, and he obtained by distilling the mixture common sulphuric æther. As all kinds of milk are susceptible of fermentation, and as no other kind of sugar but sugar of milk has been discovered in them, these facts prove that it must be fermentable. The author is of opinion that two facts have especially contributed to lead observers into error; first, it is quite possible that the usual ferment (yest) is not sufficiently powerful to decompose sugar of milk, which requires for its decomposition the action of its natural ferment (caseum); secondly, the extreme slowness of the fermentation.—*Journal de Pharmacie*, October 1837.

FORMATION OF NITRE IN EXTRACT OF QUASSIA. BY M. PLANCHE.

The ashes of quassia are slightly alkaline and cold water dissolves about 25 per cent. of them, composed of potash, lime, carbonate of potash, carbonate of lime, chloride of sodium, nitrate of potash, and traces of sulphate. The insoluble residue yielded a little sulphate of potash and of lime to boiling water; the remainder consisted principally of carbonate and sulphate of lime. There are few vegetable ashes which contain so small a quantity of alkali, and the existence of nitre in a product which has been subjected to a red heat is also remarkable.

Not only is nitre contained in the ashes of quassia, but it exists in the extract, and the quantity increases by exposing it to the action of air and moisture. An ounce of extract of quassia (A) recently prepared, and of a consistence fit for pills was put into an earthen vessel, capable of holding four ounces; it was furnished with a cover; it was kept in a place which was perfectly dry at all times. An equal quantity of the same extract (B) was put into a similar vessel covered merely with linen; this vessel was placed on a stand, at about four feet above the ground in a place used occasionally for distillation, and in which large quantities of water were evaporated, so that the air contained more or less moisture; in the same place and by the side of the extract of quassia, and in a similar vessel also covered with linen, an ounce of extract of gentian was placed (C), which is well known to contain no nitre. All the vessels thus placed were kept so for a whole year, at the expiration of which the three extracts were examined.

The extract (A) was rather dried and had lost 27 grains, Treated repeatedly for half an hour, with boiling alcohol of sp. gr. 837, it yielded 8 grains of nitrate of potash.

The extract (B) was much softened and had increased 90 grains in weight, it was heated in a salt water bath, in order to restore it as nearly as possible to its original consistence, it was then treated like the foregoing. It yielded 10½ grains of nitrate of potash. Lastly, the extract (C), that of gentian, had increased 38 grains in weight. Submitted to the same treatment as the preceding, it did not yield an atom of nitre.

Thus in the extract (B,) which was exposed to moist air, the increase of nitrate of potash was 2½ grains, in the space of a year, whilst the extract (C), which had been placed in the same circumstances, and in which there exists no azotized matter like that of quassia, no portion of nitre was formed. M. Planche concludes that after these results it is difficult not to attribute the newly-formed nitre in the extract of quassia to the azotized matter which it contains.—*Journal de Pharmacie*, November 1837.

METHOD OF DISSOLVING IRIDIUM. BY M. FELLENBERG.

M. Fellenberg remarks that by Wœhler's method of separating iridium there is obtained a double alkaline chloride of iridium, whereas by his process, which is as follows, a simple chloride is obtained which is very soluble in water. This process is founded on the fact that chlorine converts the greater number of metallic sulphurets into corresponding chlorides.

Iridium, separated from the ore of platina, containing osmium or not, is to be reduced to the finest powder in an agate mortar; upon this, the success of the process mainly depends. This powder is then to be mixed with three times its weight of flowers of sulphur, six times its weight of carbonate of potash or dry carbonate of soda, then heated in a well closed porcelain crucible, and kept in a strong red heat till no vapour of sulphur or of sulphurous acid is perceptible. When the crucible is cold, the mass, which is of a black-brown colour, is reduced to powder, and washed with boiling water till it ceases to produce any effect upon a solution of lead. The sulphuret of iridium is easily separated from the liquor by simple decantation; it is then to be thoroughly dried and reduced to a fine powder.

This is to be placed in a tube with a bulb, which is to be connected with an apparatus, evolving dry chlorine gas: the apparatus is first filled with this gas, the sulphuret of iridium is heated in the bulb by a double-wicked spirit lamp. By the gradual action of the heat, sulphur and its chloride are volatilized; the mass which has hitherto been black, crystalline and brilliant, becomes brown and afterwards yellowish red. When no more chloride of sulphur appears, the heat is to be raised so as to render the bulb strongly red hot, chlorine is again to be passed in until no further change appears.

The experiment is then finished; the lamp is to be removed, and the apparatus is to be allowed to cool, full of chlorine. The chloride of iridium then forms an orange-coloured mass, which immediately dissolves, without residue, in cold distilled water; the solution is transparent and of a deep orange-red colour. This is the chloride of iridium, it has sometimes a purplish tint, and it then possibly contains some sesquichloride. If any residue remain it is either metallic iridium or sand, or white brilliant spangles of osmo-iridium, which has escaped all the previous reactions; they may easily be separated by washing.

If the iridium contains osmium the same process is employed, but the chlorine is used moist; when the chloride of sulphur has distilled, white thick vapours appear, which when sublimed into a cool tube, give a crystalline mass of fine white osmic acid, while all the iridium remains in the bulb. If the chlorine were dry, chloride of osmium would sublime, which condenses with greater difficulty than oxide of osmium, and is easily carried away by excess of chlorine. But by the aid of moist chlorine, the osmic acid is obtained isolated, and it may be conveniently collected in a long and large tube, drawn out and kept cool at the end, while all the chloride of iridium remains in the bulb; by this process the two metals may be separated.

M. Fellenberg learnt by chance that moist chlorine favours the separation of osmium and iridium; he observed, that in a parallel experiment as soon as the tube which contained the chloride of calcium for drying the gas, was moist, and that all the chloride of sulphur had distilled, white vapours of oxide of osmium were formed, which were received in a long cold glass tube; these were recognized by their crystalline form, their action upon sulphurous acid, and on tincture of galls.

With perfectly dry chlorine, no trace of oxide of osmium is obtained, but only chloride which condenses in the state of a reddish brown crystalline powder, the greater part of which is carried off and may be collected in a bottle containing distilled water.—*Journal de Pharmacie*, November 1837.

ON LACTIC ACID IN SOUR-CROUT. BY M. LIEBIG.

Sour-crout contains an acid which is not volatile and which is not destructible by digestion, for it continues to act in a peculiar manner on the intestines. It appeared probable that it was lactic acid; it is formed, in fact, by a peculiar fermentation, to which the name of the *viscous fermentation* has been properly applied.

M. Liebig heated several pounds of sour-crout with water to the boiling point, and he added carbonate of zinc until no further effervescence nor any acid reaction was perceptible. The liquor, decanted and filtered, deposited after evaporation to the consistence of a syrup, a great quantity of crystals, which after decoloration by charcoal were of a brilliant whiteness, and possessed all the properties of the purest lactate of zinc. On precipitating the mother-waters by alcohol, a large quantity was also obtained; no other organic acid but the lactic was obtained, without even any notable quantity of acetic acid. Sour-crout contains acetic acid in so great quantity, that it may be recommended as a very good substance for its preparation. The lactate of zinc thus obtained has been analysed by M. Thomson with results which show its constitution to be

Carbon	24·72
Hydrogen	5·41
Oxygen	42·98
Oxide of zinc	26·89

The formula is

$$C^6 H^{16} O^8 + ZnO = C^6 H^{10} O^5 + Zn O + 3Aq.$$

It is extremely probable that the acid of sour turnips, cucumbers, &c., is merely lactic acid.

There are many other plants in which peculiar acids have been discovered, the composition of which is yet unknown; it may be maintained, with great probability, that if all these acids were examined according to their best characterized chemical relations, they would be reduced to a very small number. M. Liebig invites pharmacians to the investigation of the subject, calling to their recollection the fact that the acid of fruits changes according to the period of maturation, for example, the fruit of the mountain-ash contains in the first months tartaric acids, then tartaric and citric acid, and lastly, malic acid alone. The preparation and examination of these acids in many fruits, would lead to the most interesting conclusions respecting the connection of the organic acids.—*Journal de Pharmacie*, Nov. 1837.

METEOROLOGICAL OBSERVATIONS FOR NOVEMBER 1837.

Chiswick.—Nov. 1. Stormy and wet. 2. Boisterous, with showers. 3. Overcast: clear with lightning at night. 4. Frosty: fine: clear and cold. 5. Overcast: fine. 6. Very clear: fine: slight fog. 7—9. Frosty and foggy. 10. Hazy. 11. Cloudy and fine. 12. Fine. 13. Overcast: rain. 14. Fine: rain. 15. Clear and cold. 16. Fine. 17. Frosty: fine. 18. Frosty: hazy: drizzly. 19. Overcast: rain. 20. Clear: slight rain. 21. Cloudy. 22. Slight rain. 23. Densely overcast. 24. Cloudy. 25. Clear: frosty. 26. Frosty: rain. 27. Very fine. 28. Cloudy. 29. Clear and frosty. 30. Rain.

Boston.—Nov. 1. Rain. 2. Cloudy. 3, 4. Fine. 5. Cloudy. 6, 7. Fine. 8. Foggy. 9—11. Cloudy. 12, 13. Fine. 14. Rain. 15. Fine. 16. Cloudy: rain early A.M. 17. Fine. 18, 19. Cloudy. 20. Fine: rain P.M. 21. Fine. 22. Stormy: rain early A.M. 23. Stormy: 24. Cloudy. 25. Fine. 26. Cloudy: rain P.M. 27. Fine. 28. Cloudy. 29. Fine. 30. Cloudy.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; and by Mr.* Veall *at Boston.*

Days of Month. 1837. Nov.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8¾ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8¾ A.M.	London: Roy Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering. Max.	Min.	Max.	Min.								
W. 1.	28·978	29·060	28·801	28·58	52·4	54·3	41·4	55	38	43	ssw.	sw.	calm	·311	·12	·18	45
Th. 2.	29·130	29·186	29·142	28·57	42·8	56·4	41·0	50	31	41·5	w.	w.	w.	·150	·05	·34	42
F. 3.	29·258	29·441	29·263	28·83	39·7	48·8	36·4	48	28	34	sw.	w.	calm	·036	·02	...	37
S. 4	29·694	30·007	29·705	29·27	40·2	48·3	36·3	50	27	36·5	sw.	nw.	calm	...	...	...	38
☽ ⊙ 5.	30·072	30·185	30·094	29·64	39·8	48·3	36·8	49	28	40	wsw.	nw.	calm	...	...	...	36
M. 6.	30·314	30·429	30·336	29·84	38·7	48·3	36·6	48	24	33	nw.	n.	n.	...	...	...	33
T. 7.	30·456	30·477	30·425	30·03	34·7	46·8	32·8	44	23	30	e.	s.	nw.	...	...	...	33
W. 8.	30·320	30·355	30·247	29·93	32·0	45·3	30·4	33	22	30	e.	n.	calm	...	...	...	32
Th. 9.	30·142	30·196	30·114	29·71	30·7	40·3	28·0	46	31	34	e.	nw.	w.	...	·03	...	30
F. 10.	29·984	30·011	29·931	29·53	46·2	47·8	31·0	53	45	46	wsw.	sw.	calm	·019	...	...	38
S. 11.	29·940	29·986	29·961	29·40	48·4	54·3	45·8	50	39	48·5	wsw.	nw.	nw.	·050	...	...	42
○ ⊙ 12.	30·018	30·162	30·020	29·55	43·7	54·3	42·8	46	31	40	wsw.	w.	calm	...	...	...	40
M. 13.	30·118	30·162	29·796	29·62	40·3	46·0	36·7	48	41	34	wsw.	w.	calm	...	·30	...	37
T. 14.	29·552	29·670	29·533	29·10	46·3	49·3	40·0	47	34	42	w.	w.	calm	·286	·07	·27	43
W. 15.	29·948	30·074	29·959	29·63	38·6	48·7	36·8	45	29	38·5	n.	ne.	calm	...	...	·27	37
Th. 16.	30·008	30·041	29·957	29·63	37·0	44·5	35·4	41	22	37	n.	ne.	calm	...	...	·03	35
F. 17.	29·980	30·093	30·013	29·68	35·6	41·3	32·7	42	23	31	n.	n.	calm	...	...	...	34
S. 18.	30·128	30·180	30·084	29·72	34·6	42·3	31·4	51	36	31	s.	s.	calm	...	·01	...	32
⊙ 19.	30·010	30·010	29·869	29·50	44·8	45·5	33·2	52	40	41	sw.	sw.	calm	...	·22	...	37
☾ M. 20.	29·872	29·883	29·734	29·42	44·7	52·6	42·7	52	42	41	ssw.	w.	calm	·200	·01	...	42
T. 21.	29·850	30·029	29·867	29·32	39·7	49·0	36·5	52	36	38	ssw.	w.	w.	...	·04	·14	38
W. 22.	29·930	29·935	29·894	29·34	50·6	51·3	39·8	54	51	50	sw.	sw.	w.	·033	·01	·19	42
Th. 23.	29·806	29·824	29·654	29·11	52·4	55·2	50·3	54	42	53	s. var.	w.	w.	...	·13	...	48
F. 24.	29·954	30·006	29·959	29·37	46·7	55·0	46·4	47	34	43	sw.	ne.	calm	·088	...	...	46
S. 25.	30·092	30·190	29·959	29·70	39·3	49·0	39·2	43	24	32	n.	ne.	calm	...	...	...	40
⊙ 26.	30·070	30·056	29·578	29·60	37·8	43·3	33·4	47	33	37	sw.	sw.	calm	...	·25	...	36
M. 27.	29·524	29·542	29·438	29·06	37·8	48·7	37·2	47	39	38	sw.	w.	nw.	·233	...	·13	37
● T. 28.	29·282	29·323	29·215	28·90	42·2	45·0	37·9	44	30	39·5	wsw.	nw.	calm	...	...	...	39
W. 29.	29·494	29·767	29·513	29·10	34·7	45·7	33·4	46	24	30·5	w.	n.	calm	·013	...	...	34
Th. 30.	29·700	29·738	29·609	29·32	42·2	45·7	33·3	52	39	37	s.	sw.	calm	...	·06	...	36
	29·854	29·933	29·789	29·40	41·2	49·0	37·2	47·8	32·8	38·3				Sum 1·419	1·32	1·55	38·0

Lond. & Edin. Phil. Mag. Vol. XII. Pl. 3.

Fig. 1.

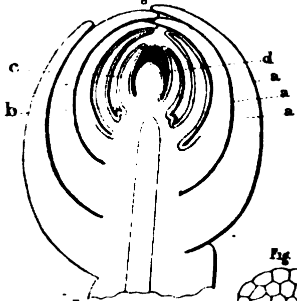
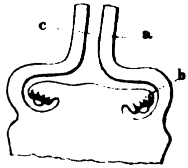


Fig. 2.



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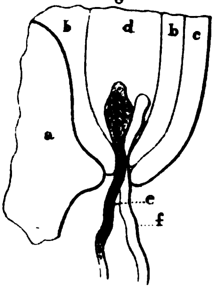

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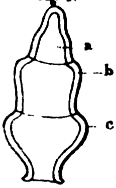


Fig. 5.

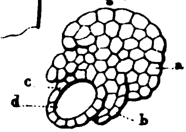


Fig. 7.

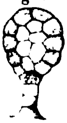


Fig. 6.

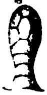


Fig. 8.

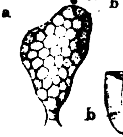
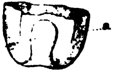


Fig. 9.



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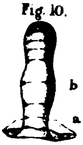

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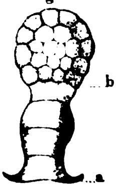

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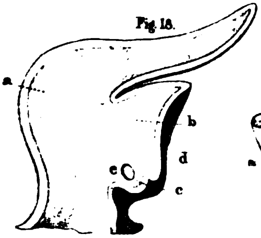

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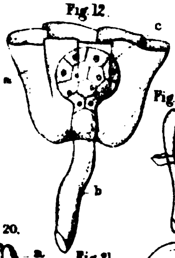

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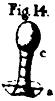

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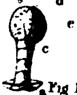

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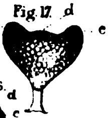

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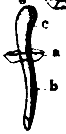

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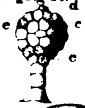

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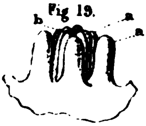

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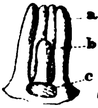

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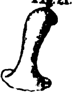

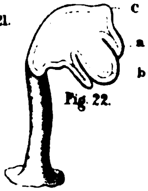

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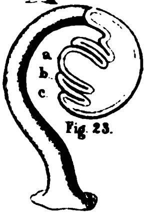

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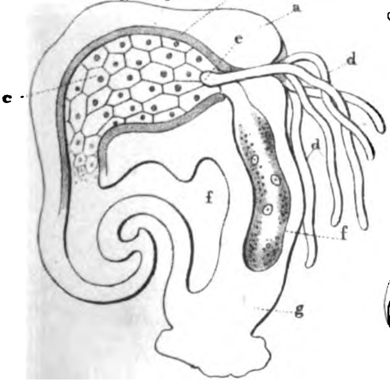

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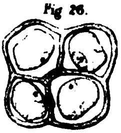

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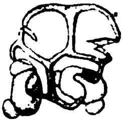

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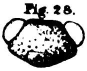

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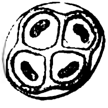

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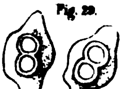

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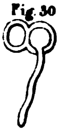

J. Basire, lith.

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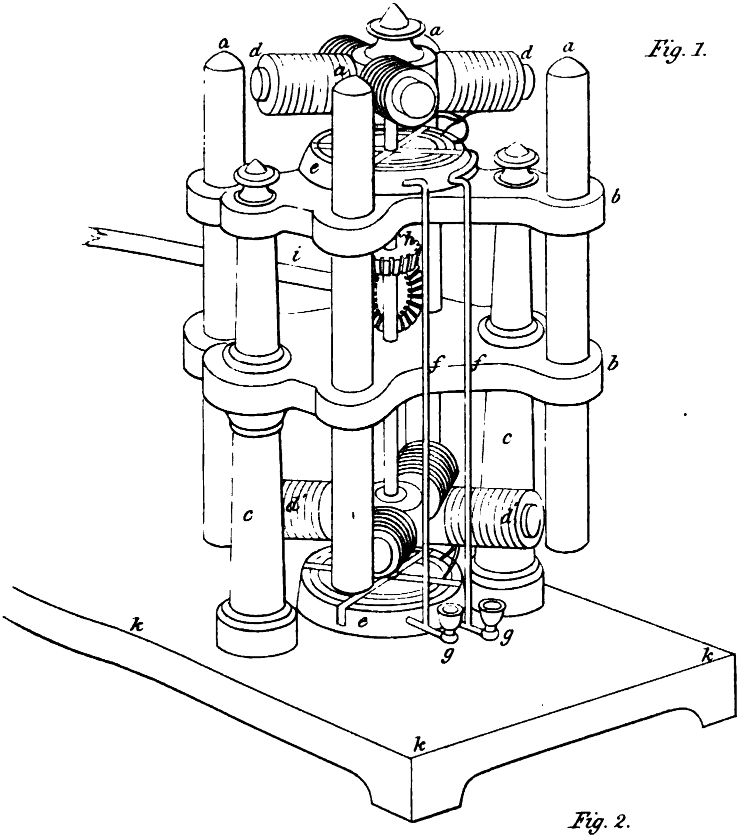
Fig. 1.



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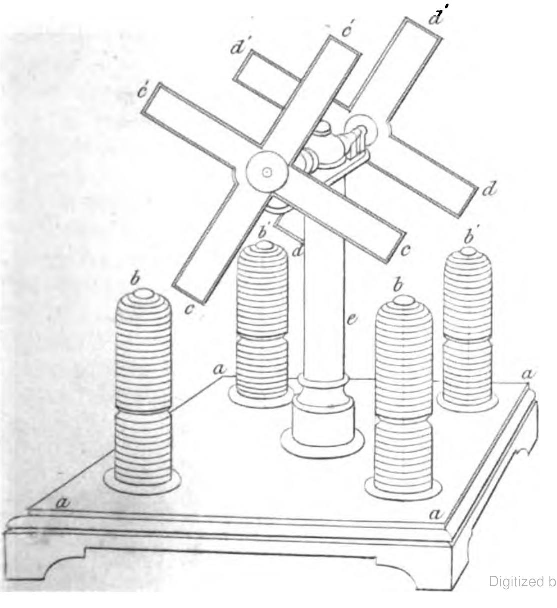

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[THIRD SERIES.]

FEBRUARY 1838.

XXII. *On a new Property of Nitre.* *By* H. F. Talbot, *Esq., F.R.S.*[*]

§. 1. THE property of nitre, of which I purpose to communicate a short account, is one which appears to me to have an immediate bearing upon the fundamental doctrines both of double refraction and of crystalline structure in general; and I think it requires that some modifications should be made in the received theory on those subjects.

In general, crystals are formed by successive depositions of layers of particles on a small primitive nucleus. For instance, when a hot solution of some salt grows cold, we observe the crystals grow and extend themselves in a manner analogous to vegetation. Or if the crystals have geometrical figures, these appear at first of the smallest size and gradually increase in all their dimensions.

The crystalline structure may be destroyed by igneous fusion. For instance, it is stated that quartz which has been fused is destitute of the double refraction which characterizes it in its natural state. I have not tried this experiment myself, but I believe that it is the received opinion, that the structure not only of this, but of all crystals, is destroyed by fusion, so as not to be recovered in growing cold.

Nevertheless the following experiment decisively shows that this opinion is erroneous.

Put a drop of a solution of nitre on a small plate of glass, and evaporate it to dryness over a spirit-lamp; then invert the glass, and hold it with the salt downwards and in contact with the flame. By this means the nitre may be brought into a state of fusion, and it will spread itself in a thin transparent film over the surface of the glass.

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[*] Communicated by the Author.

Removed from the lamp it immediately solidifies, and the film in cooling cracks irregularly. As soon as the glass is cool enough, let it be placed beneath the microscope (the polarizers being crossed, and the field of view consequently dark). We shall observe the following phænomena. In the first place the nitre appears very luminous; a proof that it is in a crystalline state, and not amorphous, as (after fusion) we might have expected to find it.

For if it had lost its crystalline structure it would produce no more effect when laid beneath the microscope than a thin sheet of glass would do, that is to say, it would be absolutely invisible, and the field of view would remain dark.

If its crystalline structure were *imperfectly* restored on cooling (so as to present a confused assemblage of minute crystals) the field of view would appear also imperfectly and irregularly luminous, according to the accidental positions of the small crystals.

Neither of these results however takes place, but an entirely different one, which would hardly have been anticipated. For the crystalline structure of the fused nitre is *perfect*; that is to say, that the film appears very luminous, and *uniformly* so, as if it were a thin slice which had been taken from a large crystal of the substance.

In order to see how perfectly it is crystallized, select any portion of the film for observation, and turn it slowly round in its own plane upon the stage of the microscope. Its brightness will be seen gradually to fade, and finally, in a certain position, to be altogether extinguished. Now when this happens it will be seen to be *uniformly* dark over its whole surface, and the smallest irregularity in this respect would of course immediately manifest itself by contrast with the rest. Consequently every part of it acts together, as if it were a portion of one and the same crystal.

But this does not continue indefinitely; for if we carry the eye over the whole of the dark surface which I have described, and which frequently extends in breadth over a space double or triple of the field of view, we shall come *abruptly* to its boundary, and shall see it succeeded by another portion of the crystalline film which is luminous. If now we darken this second portion (by again turning round the glass plate to a certain extent), in proportion as we do so we shall see the first portion recover its light.

Proceeding in this manner, we shall find that the whole crystalline film which is spread on the glass consists of ten, twenty, or a greater number of these separate portions, each of which is a perfect crystal in itself, but altogether unconformable to the remainder. Moreover we shall easily perceive that the cracks which took place in the film during its cooling are the boundary lines which separate these different portions; and that after each crack commences a new crystal, which has no connection whatever *with respect to the position of its axis*, with the preceding one. These successive crystalline spaces are however, of course, in the most perfect juxtaposition; and their general appearance and effect may be compared to a sort of mosaic pavement formed of slabs of black and white, or coloured marbles, very irregular in form and outline, but accurately fitting each other.

It appears then to follow as a consequence from this experiment, that the crystallization of melted nitre does not take place in the usual way by the formation of a primitive nucleus and the deposition of layers of molecules upon it; but that we have here a new sort of molecular action brought into evidence, by means of which considerable portions of crystal are *formed at once*, and pass from the fluid state into that of a crystalline solid with an axis in a determinate direction. We also see that whatever be the nature of the force which determines this direction, it often extends to a considerable distance. But since other portions of the fluid are disposed in solidifying to assume another direction of axis, this different tendency causes the film to crack at the point where the opposite forces counterbalance each other, and to separate into several independent crystals.

§. 2. I now come to relate another property of nitre equally remarkable with the foregoing; and as these experiments are generally successful, and easily exhibited, I trust they may be considered as some accession to our knowledge of molecular action.

Let a film of fused nitre be obtained in the manner already mentioned, and then let it be allowed to cool during three or four minutes. The plate of glass should be turned round upon the stage of the microscope until the crystalline film is darkened as accurately as possible. Things being thus adjusted, let the observer touch the film with the point of a needle, while he is observing it in the microscope.

He will perceive that the touch immediately produces a luminous spot on the dark surface, and this spot will slowly expand itself in all directions like a luminous wave. This is a very curious object, but difficult to describe. Its motion is extremelyirregular, its outline continually shifting and changing, and assuming different colours, until finally in four or five minutes it overspreads every part of the field of view, which by this singular process has been metamorphosed from a space almost entirely dark, into a luminous one, mottled with all manner of colours. Should the observer happen to have quitted his instrument in the mean while, and during his absence this change have taken place spontaneously, he would hardly be able to persuade himself that his adjustments had not been deranged, and some new object placed before the microscope.

This very beautiful phænomenon no doubt arises from the following cause, viz. that the crystalline state or arrangement of particles which nitre assumes at the temperature at which it first solidifies after fusion, is no longer suitable to it when grown perfectly cold; so that its condition is then one of unstable equilibrium which the slightest force is capable of subverting. By touching it with a needle a disturbance is produced, which propagates itself from the disturbed point throughout the entire mass.

But even if it is not touched the same change will take place *spontaneously* a few minutes later.

If however we touch it prematurely, as, for instance, during the first minute after it has become solid, this change does not take place.

We may *then* trace lines or letters upon the darkened film with the point of a needle, and these lines will appear luminous, in consequence of the crystalline particles which the needle displaces being thrown into such positions as to depolarize the light. But this does not disturb the rest of the field of view, which remains quiescent for several minutes after, and then changes spontaneously, as I have endeavoured to describe.

XXIII. *Meteorological Observations made during a residence in Colombia between the Years* 1820 *and*. 1830. *By the late Colonel* Francis Hall.*

IF the materials of science could be gathered only by the scientific, the following collection of observations would be a useless labour; but it frequently happens that in distant countries the opportunity of observing natural phænomena falls to the lot of those very ill-fitted in most respects to profit by it. The genius of a Humboldt, like an incantation of science, descends upon the New World but once in a series of ages. The most that can be done by an ordinary observer is to offer his mite, a single stone towards the pyramid of knowledge, in the hope that he may casually prove useful, and with such humble pretensions can scarcely be deemed importunate. Should even this apology barely extenuate the sterility of a ten years' residence in a country so admirably varied and rich in natural phænomena as Colombia, something further may be urged in excuse of the *military* traveller, obliged frequently to hurry through the most interesting parts, and to vegetate whole years in others of minor importance; without books, without instruments, without resources; fettered too often by the chain of his own daily wants and sufferings, and fallen on a time when every species of local or traditional information, every glimmering of philosophic research, had been buried and obliterated amid the storms and struggles of the revolution.

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* Communicated by Prof. William Jameson, of Quito, to Sir W. J. Hooker, F.R.S., and by him to Phil. Mag.

The geographical features of Colombia have been portrayed by Humboldt with an accuracy which renders further description superfluous. It is however impossible to traverse this extensive territory without being struck by the physical phænomena of a country where *height* produces the effects of *latitude*, and where the changes of climate, with all the consequent revolutions of animal and vegetable life, are brought about by localities, to which we find little analogy in Europe. The equatorial seasons, as is well known, are merely the wet and dry; and though the Spaniards, influenced by European recollections, have given the former the name of winter "*invierno*," it is during this period that nature revives from the vegetable torpor which the scorching tropical heats produce in the low-lands, in almost an equal degree with the frosts of northern climates. In the vast plains which extend to the south and east of the great chain of the Andes, the rainy season observes an invariable order. The Orinoco begins to rise in April, and attains its *maximum* of increase in July and August, when the immense savanas which extend to the base of the Andes are converted into the appearance of an inland ocean. It decreases from this period. and the summer is reckoned from October to April. In the mountains, on the contrary, the rains commence about the former month, and predominate, with intervals of fair weather, till May or June. The winter of the low-lands to the west and north of the Cordillera, both on the Pacific and Atlantic coast, is governed by that of the mountains, but with several curious local varieties. Thus, the rainy season of Guayaquil is nearly as regular as that of the plains, being reckoned from the middle of December to the middle of May; while the thick forests which, further to the north, cover the provinces of Esmeraldas, Barbacoas, and Choco, produce by their constant evaporation an almost perpetual deluge. Wherever, on the contrary, the Cordillera recedes to some distance from the coast, as is the case with parts of the Venezuelan chain, the intermediate country is parched by a drought often of several years. Maracaybo, and a considerable part of the province of Coro, are instances, where sandy plains, scantily shaded by *Mimosas* and thick plants, afford shelter and subsistence only to flocks of goats and asses. The coast of Rio Hacha is equally dry and sterile till it approaches the foot of the isolated ridge of Santa Marta; while the Goagira territory, interposed between Rio Hacha and Maracaybo, is regularly inundated every year, and consequently, though destitute of streams, maintains considerable herds of cattle and horses, a circumstance to be ascribed to the vicinity of the Ocaña branch of the Andes, which extends, with its clouds and thick forests, almost to the confines of this province. The whole Peruvian coast from Payta to Lima is an additional instance of the same fact, where the recession of the Andes from the coast is marked by sandy deserts, which the industry of the Incas had rendered productive by artificial irrigation. In the valleys, and on the table lands of the mountains themselves, the culminating summits produce great variations in the distribution of moisture. The city of Caraccas, situated at the foot of the Silla, has the benefit of a regular, though mild rainy season; while within a league there are spots which suffer several years of drought. Popayan, placed at the head of the sultry valley of the Cauca, and surrounded by lofty *paramos*, has nine months of continued rains and tempests, attributable to the clouds which are driven in opposite directions from the mountains, till they encounter the hot ascending air of the valley. In that part of the ancient kingdom of Quito now called the Department of the Equator, the mass of Chimborazo interrupts the passage of the clouds from south to north, so that while the western slopes are deluged with rain, the elevated plains of Riobamba to the east recall to the imagination of the traveller the deserts of Arabia Petræa. Following the same mountain-chain towards the city of Quito, we observe the storms arrested between Cotopaxi and Pichincha over the valley of Chillo, while two leagues further to the north the climate of the village of Pomasqui is so dry as to have given it the name of Little Piura.

The manner in which rain is formed and precipitated at various elevations seems to illustrate and confirm the theory of Leslie. In the regions of *paramos*, i. e. from 12,000 feet upwards, the encountering aërial currents, unless in the case of some strong agitation of the mass of the surrounding atmosphere, are of a low and nearly equal temperature. The rains, in consequence, assume the form of thick drizzling mists, known by the name of *Paramitos.* On the elevated plains we find the showers more or less sudden and violent, according to localities which give rise to a mixture of currents more or less variably heated. Quito, for example, is situated on what may be called a *ledge* of the lofty mountain of Pichincha, and overlooks the valley of Chillo or Guaillapamba, furrowing the adjacent table-land, in which the thermometer often rises to 80° in the shade. The encounter of portions of the atmosphere thus variously heated produces showers as sudden and heavy as those which generally distinguish tropical climates. On the slopes of the Cordillera the rains are generally violent, for the same reason.

Looking to the hygrometrical state of the atmosphere, as it results from observations made on the table-lands of the Equator and the coast of the Pacific, we find it to vary from 0° in the damp forests of Esmeraldas to 97°·1 on the elevated plain of Cayambe: the experiments in both places being made during June and July, the summer months both of the coast and mountains. The average medium for the low-lands is 23°·85; for the Cordillera 44°·36 of the hygrometer constructed upon Leslie's principle; but we are in want of sufficient data for those elevations which approach the limit of perpetual snow. To judge however from a small number of observations made on the mountain of Cayambe, at 12,705 and 14,217 feet of elevation; and at the hut of Antisana at 14,520 feet, where the hygrometer was found to give 16°·5, 13°·9, and 30°·3, it would not seem that the dryness of the atmosphere increases in ratio of the elevation, at least in the neighbourhood of snowy mountains where a continual moisture is exhaled, and heavy mists sweep over the soil towards the evenings even of the fairest days.

To estimate the general distribution of temperatures through the vast territory of Colombia, we may conveniently consider it as divided into five zones. 1. That of the level, or nearly so, of the ocean. 2. That of small elevations, from 500 to 1500 feet. 3. That of the slopes of the Cordillera, from 2000 to 7000 feet. 4. That of the elevated plains, or table-lands, from 8000 to 10,000 feet. 5. That of the *Paramos*, from 11,000 to the limit of perpetual snow.

1. The degree of heat at or near the level of the ocean is modified by a variety of local circumstances, which may be ranged under the following heads: Proximity of the sea; of great rivers or lakes; of lofty ridges of mountains; of extensive forests; of contiguous elevations, which impede the circulation of air and produce reflected heat. The various combinations of these circumstances may be considered as affording a rule of the increase or diminution of temperature.

Thus, La Guayra, situated on a sandy beach backed by a precipitous wall of rocks, has no counterpoise to the excess of heat but the sea breeze, and the remote influence of the ridge of the Silla, which nowhere reaches the limit of perpetual snow. Humboldt considers it in consequence as the hottest place on the shores of the New World, (Personal Narrative, vol. iii. p. 386) the mean annual temperature being 82°·6; yet the observations I made during some months' residence in Maracaybo, give an annual mean of 84°·63; nor is this surprising, when we consider the localities of both places.

In Maracaybo the sun's rays are reflected from a barren sandy soil, scantily sprinkled with *Mimosas*, and prickly plants; the mountain chains are too remote to have any influence on the atmosphere, so that several years frequently pass without any regular fall of rain. The vicinity of the lake, no doubt, acts slightly as a refrigerant, but the city is built on the border of its outlet to the sea, where it is both narrow and shallowest, and is consequently heated nearly to the temperature of the incumbent atmosphere. Add to this the small sandy elevations to the north, which intercept the partial effect of the sea breezes, so that they are scarcely felt, except in the months of December and January, when the thermometer sometimes sinks to 73°·0; yet the medium even of these two months is not less than 81°·0, while that of La Guayra from November to December at noon is, according to Humboldt, 75°·8, and at night 70°·9 (Pers. Nar., vol. ii. p. 387). Rio Hacha is situated on a sandy beach; the sea breeze blows with such violence that boats can scarcely land between ten in the morning and four in the afternoon: these winds however, sweeping over the hot plains of Coro and Maracaybo, have but a partial effect in lowering the temperature, the annual mean of which is 1°·98 less than that of Maracaybo. I never saw the thermometer lower than 75°·0, nor above 89°·0. In Santa Marta the average of the coolest months is 82°·25. The thermometer however never rose during my residence there above 87°·0. The soil is sandy, and the city is surrounded by bare rocky heights to the north and south, which counterpoise the cooling influence of the *Sierra nevada* (snowy mountains), from which it is but a few leagues distant. The temperature of Barranquilla, a village situated on the river Magdalena, about 18 miles from its mouth, is nearly the same with that of Santa Marta; for if on the one hand the air is refreshed by the evaporation from a damp soil covered with luxuriant forests, and the vicinity of a large river; on the other it is beyond the reach of the sea breeze, and the influence of the mountains which operate in Santa Marta. The annual mean is 82°·20; that of Cumanà is, according to Humboldt, 81°·0. The breezes which sweep from the gulf of Paria over the wooded Birgantine chain probably contribute to lower the temperature.

We have thus, on a calculation of six points on the Atlantic coast of Colombia, a mean annual temperature of 82°·56 *. The shores of the Pacific, as far as the latitude of Payta, are subjected to other influences, being almost entirely covered by damp luxuriant forests; while the ocean itself is cooled, as Humboldt observes, by the winds which blow constantly from the south. This, however, is more perceptibly the case from latitude 8° to 13°, where the air is cooled to an average of 71°·8. Betwixt 9° north latitude and 3° south latitude, if we may trust to observations made at the five points of Panamà, Esmeraldas, el Morro, the island of Punà and Guayaquil, the annual mean is 80°·11, being 2°·45 less than the mean of the Atlantic coast. A notable difference also arises from the superior elevation of the Pacific chain of the Andes, and its more immediate vicinity to the coast; while the Venezuelan branch, with the exception of the Santa Marta ridge, is both lower and more inland.

2. On penetrating into the interior of the country and examining the temperature of small elevations, we may take as forming an aggregate specimen of the whole country, 1st, the damp wooded valleys of the Orinoco and Magdalena; 2nd, the forests which border on the Pacific; and, 3rd, the immense plains of Venezuela, alternately flooded and parched with excessive heat. Humboldt assigns to the valley of the Orinoco a mean temperature of 78°·2 The small number of observations I have made on that of the Magdalena, would give a mean of nearly 83°, which I should scarcely think too high, considering the localities of the river, which flowing from south to north, affords no channel to the sea breezes. Its mass of water is also much less considerable than that of the Orinoco, while its numerous sinuosities, and the low ridges which border it in the upper part of its course, contribute to render the air stagnant and suffocating. The temperature of Honda, at 1200 feet of elevation, is as high as that of any part of the coast, except Maracaybo. The unbroken forests which extend from the roots of the Quitenian Andes to the shores of the Pacific have a much lower temperature, caused by the proximity of the snow-capped Cordillera, and the humidity which prevails throughout the year. Accurate observa-

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* I have not included Cartagena, because the number of observations is, perhaps, too limited to draw a conclusion as to the yearly temperature. If we take them into the calculation the annual mean would be 82°·86, which is probably too high.

tions give an annual mean of 76°·78, or 1°·42 lower than the valley of the Orinoco, and 6°·22 lower than that of the Magdalena. The mean temperature of the plains of Venezuela is reckoned by Humboldt at 88°·4 (*De Distribut. Geog. Pl.*, p. 92.) yet several reasons may induce the belief that this calculation is excessive. This illustrious traveller performed his journey during the summer season, when the atmosphere is heated by the reverberations from a parched and naked soil. Persons who have resided near the Apure state the climate in rainy weather to be cool, and refreshed by a constant breeze. It is only on the coast of the Pacific that the rainy season is the period of the greatest heat, when the air is still and undisturbed by those electric explosions so common on the mountains and in the interior. The observations I made at Varinas and San Carlos, towards the beginning of the winter season, give a mean of 81°·0; and averaging the dry season at 88°·4, we have a yearly mean of 84°·7, which is probably the extreme or something beyond it. There is no doubt it is in the plains of the interior that we find the greatest heat during the dry season. In the level country called the valley of Upar, between the mountain ridges of Santa Marta and Ocaña, I found the thermometer in the shade several times above 100°, and once as high as 108°·0. The average of nineteen observations, made at different points of this district, is 89°·09; but we must allow a considerable decrease during the months when the soil is covered with thick vegetation, and drenched by continual rains. As a general mean of the interior at small elevations, we may take 80°·67, or nearly that of Cumanà.

3. The temperate mountain region lies nearly between the elevations of 3000 and 7000 feet. Below this may be considered as a hot climate: such for instance is Valencia and the valleys of Aragua in Venezuela, the height of which is from 1500 to 2000 feet, and its mean temperature 78°, or 0°·14 above that of Guayaquil on the Pacific; but the soil, stripped by cultivation of its ancient forests, imbibes freely the solar rays, which are besides reflected from the rocky elevations which everywhere surround the cultivated districts. The temperature of Caraccas (elevation 2904 feet) was fixed by Humboldt in his essay, *De Distribut. Geograph. Pl.*, p. 98. at 69°·6; but in his Personal Narrative, b. iv. c. xii. (vol. iii. p. 460) he considers 17°·2 of Reaumur = 70°·40 of Fahrenheit, nearly as the true yearly mean. My own observations during a residence of some months give 71°·40. The preference would be certainly due to Humboldt's calculation, but for some collateral circumstances deserving of attention. I heard it generally remarked in the city, that the seasons had grown *hotter*

since the earthquake of 1812. It would be difficult to explain how the temporary evolution of volcanic gases, supposing such to have taken place, could operate any permanent change on the surrounding atmosphere; yet other causes may have produced an effect falsely ascribed to the phænomena most impressed on the imagination of the inhabitants. On looking over Humboldt's collection of observations for December and January 1799, we find the thermometer seldom rise to 75°, and often sink to 59°, so that the mean of these months is about 68°. During the same months in 1821 the daily range was from 65° to 76°. I never observed it lower than 61½°, and on one occasion at 5 a.m. it stood at 71°. The mean of these two months is 70°·21, or 2°·21 higher than the estimate of Humboldt. The clearness and beauty of the sky during almost the whole period of my residence is also a circumstance opposed to Humboldt's "Cœlum sæpe nubibus grave quæ post solis occasum terræ appropinquant." (*De Distrib. Geog. Pl.*, p. 98.) I remember but *once* to have seen a fog in the streets of the city. Future observations will show whether any change of climate has really taken place, or whether the differences observed be only such variations as may be frequently remarked in the same places between one year and another. The mean of the whole temperate mountain region may be reckoned at 67°·80, that is if we limit ourselves to the districts partially cultivated and inhabited. The declivities of the Andes, still covered with vast and humid forests, have probably their temperature proportionally lower. Thus the village of Mindo, on the western declivity of Pichincha, embosomed in humid forests, at 3·932 feet of elevation, has a medium temperature of 65°·5, the same with that of Popayan.

4. The elevated plains of the Andes between 8000 and 11,000 feet, on which were anciently united the most powerful and civilized indigenous nations beneath the dominion of the Lipas of Tunja and Bogotà, and the Incas of Quito, and where the great mass of Indian population is still to be found, have a general medium temperature of 59°·37, modified however by local circumstances, and particularly by the proximity of the *nevados*. Thus the village of Guaranda, placed at the base of Chimborazo, though nearly 500 feet less elevated, is at least 1°·0 colder than the city of Quito, sheltered on all sides by the ramifications of Pichincha. The city, again, is above 1°·0 warmer than its suburbs on the plains of Añaquito and Turupamba to the north and south. Riobamba is about 200 feet below Quito; yet its situation in an open plain bordered by the snowy mountains of Chimborazo, Tunguragua, and La Candelaria renders the climate colder and more

variable; while the town of Ambato, only 300 feet lower than Quito, but built in a nook of the river which runs near it, and shut in by dry sandy elevations, has a climate about 2°·0 milder, so that sugar-cane is cultivated in its immediate vicinity. The general uniformity of temperature, which spreads a certain monotony over tropical regions, is joined, at great elevations, to a daily variability, which must exercise a considerable influence both on vegetable and animal life. The thermometer, which often sinks at night to 44°·0, rises in the sun, wherever there is reflected heat, frequently to 120°·0, being equal to the heat of Jamaica; while in the shade it seldom exceeds 65°·0: so that, on passing from shade to sunshine, one is immediately exposed to a difference of above 50°·0, and in the course of twenty-four hours to nearly 80°·0. The shade, in consequence, even on the hottest days, imparts a feeling of chilliness, while the solar rays seem to scorch like the vapour of a heated oven. The same difference is perceptible on the *Paramos*. At the foot of the Nevado of Santa Marta, I observed the thermometer at 5 a.m. sink to 22°·0, and at 9 a.m. it rose to 73·°0 in the sun. On the height of Pichan, between Quito and Esmeraldas, elevation 12,986 feet, the thermometer stood at 53°·0 in the shade, and 83°·0 in the sun. On Antisana the difference was 22°·0 *at the same time*, but 34°·0 between 6 a.m. and 3 p.m.: when the atmosphere is calm it is much more considerable.

5. Although at great elevations, *i. e.* from 12,000 to 16,000 feet, it is difficult to form a series of meteorological observations, such is the yearly equality of the temperature that a single day may be safely taken as a sample of the whole year. Nay more, a collection of observations made at similar heights, though in different places, will give a similar result to a series taken on the same spot. Thus, in the following table, there is little difference between the result of seven observations, made on seven different mountains, and the six made on that of Antisana.

1.	Paramo of Santa Marta	15,000 ft.	22°	5½	a.m.
2.	Paramo of Cayambe	12,705	37	6	„
3.	Paramo of El Altar	12,986	42	8	„
4.	Mine of Conderasto	14,496	45	12	„
5.	Volcano of Pichincha	15,705	46	1	p.m.
6	Mountain of Atacaso	14,820	41		„
7.	Nevado of Cayambe	14,217	43	1½	„
	Mean	....	39°·42		
	Paramo of Antisana........	14,520	38 ·58: six observ.		
	General mean..	....	39 ·87.		

General Table of Temperatures and Elevations.

Elevations.	Places.	Temperature.	Ditto of Humboldt.	Hygrom.	Lat.	Observations.
0	Cumana	....	81°			The Elevations here indicated by Zero are such as are too small to influence the temperature.
0	La Guayra	....	82 ·6			
0	Maracaybo	84°·63				
0	Rio Hacha	82 ·65				
0	Santa Marta	82 ·28				
0	Baranquilla	82 ·21				
	Mean of the Atlantic Coast	82 ·56	81 ·8			
0	Panama	81 ·14	....			
0	Esmeraldas	79 ·65	....	21°·78		
0	Guayaquil	77 ·26	....	27 ·01		
	Mean of the Pacific Coast	79 ·35				
500ft.	Valley of the Orinoco..............	78 ·2	78 ·2			
650	Valley of the Magdalena	83 ·				
522	Plains of Venezuela	84 ·7	88 ·4			
543	San Carlos in ditto	81 ·15				
600	Canigue Forests of the Pacific	76 ·78	....	22 ·77		
	Mean of the Interior	80 ·61	83 ·3			
1527	Valencia	78 ·25				
2903	Caracas	71 ·68	70 ·40			
5823	Popayan	65 ·40	65 ·6			
6782	Loxa	66 ·6	66 ·6			
	Temperate Mountain Regions ...	67 ·71	67 ·17			
8694	Bogotà	60 ·49	60 ·8			
9514	Quito	60 ·47	59	40 ·98		
	Suburbs of ditto ...	58 ·75	....	42 ·03		
9724	Cayambe	57 ·25				
	Elevated Plains ...	59 ·24	59 ·9*			
14,520	Antisana Farm ...	38 ·58	....	30 ·3		
12,457 to 15,727	Paramos of Cayambe. El Altar, Pichincha, and Santa Marta	39 ·16				
	Mean Temperature of the Paramos	38 ·87				

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* Humboldt ascribes to this region a yearly mean temperature of 54°, while the result of the mean temperatures of the various places he has instanced is 60·64°.

XXIV. *Sequel to an Essay on the Constitution of the Atmosphere, published in the Philosophical Transactions for* 1826; *with some Account of the Sulphurets of Lime.* *By* JOHN DALTON, *D.C.L., F.R.S. &c.**

IN an essay of mine on the constitution of the atmosphere, which was printed in the Transactions for 1826, I signified my intention of following it with a sequel of experiments to ascertain if possible which of the two views therein developed was most countenanced by facts. I now proceed to give an account of such investigations relating to this subject as have engaged my attention during a long period of years.

It may be needful to premise certain facts which are, I believe, universally admitted as indisputable; namely, that the atmosphere consists principally of two elastic fluids, azote and oxygen, either mixed by some mechanical law, or otherwise combined by a chemical principle in proportion nearly as four parts of the former to one of the latter in volume; that the two elastic fluids may be obtained separately in a state of purity; that when thus obtained they may be mixed in all possible proportions; and that the aggregate volumes in such cases are just equal to the sum of the two volumes of the ingredients; also, that any body which has a chemical affinity for either of them, so as to combine with it in a separate state, will also combine with it in the mixed state.

It is also pretty generally admitted that oxygen and azote are capable of chemical combinations in five or more definite proportions, namely,

2 vol. of azote with 1 vol. of oxygen—forming 2 vol. of nitrous oxide.
1 vol. of azote with 1 vol. of oxygen—forming 2 vol. of nitrous gas.
1 vol. of azote with 1½ vol. of oxygen—forming 1½ vol. of hyponitrous acid.
1 vol. of azote with 2 vol. of oxygen—forming 2 vol. of nitrous acid vapour.
1 vol. of azote with 2½ vol of oxygen—forming 2½ vol. of nitric acid.

There does not appear to be a doubt of the reality of five combinations, but all chemists are not agreed as to the proportions of the volumes being precisely as above specified, chiefly because no general law has been found to obtain in such gaseous compounds.

These compounds are never formed nor decomposed without manifest chemical agency; they all contain oxygen, but no portion of it can be abstracted from any one of them without some chemical operation; whereas nitrous gas will immediately seize the oxygen from any of the afore-mentioned mixtures, the same as if it was alone, whatever may be the proportions. Atmospheric air itself, or any artificial mixture of the two gases in the same proportion as common air, is equally affected by nitrous gas and by every other agent.

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* From the Philosophical Transactions, 1837, Part ii.: an abstract of Dr. Dalton's former paper will be found in Phil. Mag., First Series, vol. lxvii., p. 310.

Waving at present any consideration as to the nature and properties of the above chemical compounds, I shall now proceed to state the means by which the proportions of oxygen and azote in mixtures of these two gases may best be determined. Having been engaged in this investigation occasionally for more than forty years, I may be entitled to give my opinion on this important subject in practical chemistry.

Various methods of analysing common air have been discovered in the last fifty years. I have principally directed my attention to three, namely, (1.) by the use of Volta's eudiometer and hydrogen, or (2.) by nitrous gas, or (3.) by quadrisulphuret of lime, to abstract the oxygen from the azote.

First Method, by Volta's Eudiometer.

Mr. Cavendish was one of the first to investigate the changes produced by firing mixtures of hydrogen and common airs in various proportions. (Vid. Philos. Trans. 1784.) The following table will exhibit a lasting monument of his skill in effecting such an investigation. Many have attempted since to improve the methods of analysis, and have brought out results widely differing from those to be derived from his table; but it is now universally allowed that his results are nearer approximations to the truth than most of those we have seen since.

His method was to take 100 measures of common air and mix them with various proportions of hydrogen, beginning with upwards of 100, and gradually descending till about 20; then, firing each mixture by an electric spark, he marked the diminution of the mixture each time as under.

The following results are extracted from Mr. Cavendish's Table, except the last column, "Amendment," which I have attached, for reasons assigned below.

Exp.	Common Air.		Inflammable Air.		Diminution on firing.	Amendment.
1.	100	measures mixed with	124·1	gave	68·6	66·3
2.	100	———————	105·5	——	64·2	65·8
3.	100	———————	70·6	——	64·7	64·9
4.	100	———————	42·3	——	61·2	60·6
5.	100	———————	33·1	——	47·6	47·4
6.	100	———————	20·6	——	29·4	29·5

In the first three experiments no oxygen was found in the residuary gas; in the fourth a trace of oxygen was found; and in the fifth and sixth, considerable quantities of oxygen were found in the residues.

It is obvious that Mr. Cavendish began intentionally with an overdose of hydrogen, probably expecting the diminution to be a constant quantity till the hydrogen became deficient, and then of course the diminution must be lessened; this was not the case exactly; but the reason is easily discovered, and it proves the accuracy of the observations.

Hydrogen gas is rarely obtained quite pure: it frequently holds two or three per cent. of common air, detached from the water through which it bubbles and by other means; this air increases as more water enters the hydrogen bottle, till sometimes it amounts to ten per cent. at the last, as every one knows who has had a due share of experience. Now as Mr. Cavendish does not mention the purity of his hydrogen, we must try it by the means now generally known, as the reported results will guide us in the investigation.

On looking at the column headed "diminution on firing" it is easy to see there is a discrepancy in the first three experiments in that column; if the hydrogen used contained any oxygen the diminution on firing ought to have continually decreased, whereas it was greater in the third than in the second experiment. This it must be allowed is a proof of inaccuracy in one or both of the experiments; but it is no greater error than usually occurs if we trust to a single experiment with any gaseous mixture. The average of two or three experiments on mixtures of the same proportions should be taken. The fourth experiment clearly shows that the hydrogen contained oxygen as well as azote; for a diminution of 61·2 would denote the union of 20·4 oxygen with 40·8 hydrogen; hence there must have been 1·5 common air in the hydrogen. I have formed the column "amendment" by assuming the hydrogen in all the experiments to contain 4½ per cent. common air. If we combine the results of the third and fourth experiments, either by assuming Mr. Cavendish's diminution or that of the amendment, we shall obtain a very good approximation to the quantity of oxygen in atmospheric air, the former experiment giving too great diminution by reason of the excess of hydrogen and that containing some oxygen, and the latter giving too little diminution for want of the requisite quantity of hydrogen; the former will give 20·98 per cent. oxygen, and the latter 20·92 per cent. oxygen in atmospheric air. If any doubt should remain as to Mr. Cavendish's hydrogen containing oxygen, it is removed by the consideration that his first experiment would indicate 22·9 oxygen per cent. in air, which cannot be allowed; and his last experiment that 8·8 oxygen must have combined with 20·6 hydrogen instead of 17·6, which is equally inadmissible.

Since the period 1784 it has been found by various chemists that in mixtures of oxygen and hydrogen, as well as in other similar ones, the electric spark does not always cause an explosion, and when it does a complete combination does not always take place, but that in the residue sometimes portions of both the ingredients may be found. The limitations and restrictions are now pretty generally known; and with regard to the mixtures of common air and hydrogen, I published a letter in the 10th volume of the Annals of Philosophy, (New Series) page 304, in which I showed the limitations found by my own experience to be as under:

Common air and hydrogen in which the oxygen is only $\frac{1}{13}$th, or from six to seven per cent. of the whole mixture, do not explode.

Common air and hydrogen in which the oxygen is only $\frac{1}{14}$th, or seven per cent. explode imperfectly, leaving both oxygen and hydrogen.

Common air and hydrogen in which the oxygen is from $\frac{1}{13}$th to $\frac{1}{6\cdot86}$th, or from eight to fourteen or fifteen per cent., fire leaving hydrogen and azote only.

Common air and hydrogen in which the hydrogen is $\frac{1}{3\cdot38}$th to $\frac{1}{7}$th, or from fourteen to thirty per cent., fire and leave oxygen and azote only.

Common air and hydrogen in which the hydrogen is $\frac{1}{8}$th to $\frac{1}{14}$th, or from eight to twelve per cent., fire imperfectly, and leave oxygen, hydrogen, and azote.

Common air and hydrogen in which the hydrogen is $\frac{1}{13}$th or less than seven per cent., do not explode.

It should be observed that when one of the gases is so far deficient as not to allow of an explosion by a single spark, the effect may be obtained by a current of sparks for a longer or shorter period, accompanied by the requisite diminution of volume. In such instances where the effect is produced only by a current of sparks it may be proper here to suggest the reason. When mixtures explode perfectly but feebly, we see the flame, lighted by the spark, to run down the eudiometer till it reaches the water; when they explode still more feebly, the flame runs perhaps half-way down the tube and is extinguished before it reaches the water. There scarcely can be a doubt that the extinction must be occasioned by the cooling effect of the eudiometer and of the intermixture of the mass of air which has to be heated by the feeble flame. Another spark in its passage will re-alight the flame, to suffer a quicker extinction, and so on till at length the combustion is complete. This reason will also explain the excessively slow combustion of azote by the electric spark, as ascertained by Mr. Cavendish, and as I have found by repeated experience. Query, might not this experiment succeed better by heating the eudiometer?

From what we have stated it must be obvious that in order to secure the complete abstraction of either oxygen or hydrogen from mixtures by Volta's eudiometer, we should avoid too near an approach to the limitations we have pointed out; or if that cannot be, we should carefully examine the residue for both gases. The best test for very small portions of oxygen is undoubtedly nitrous gas; for somewhat larger portions of oxygen or hydrogen, additions of those gases might be made so as to bring the mixtures into proportions capable of being exploded.

Second Method, by Nitrous Gas.

The nitrous gas eudiometer is of singular utility on many occasions. No other can exceed it in accuracy when mixtures contain very little, as one or two per cent. of oxygen; or on the other hand when nearly the whole of the gas is oxygen. But when the mixture of gases contains from twenty to eighty per cent. of oxygen, as in the case of common air, it is not the best when great exactness is required. The reason is well known; when oxygen and nitrous gas combine, the combination is not like that of oxygen and hydrogen, in uniform proportion. We may take one third of the diminution for oxygen, when mixed over water; but this can be considered only as a first approximation. One hundred parts of oxygen may combine with 130 or 360 parts, or any intermediate quantity of nitrous gas, according to circumstances. When only 1 or 2 per cent. of oxygen are expected I put in 5 or 10 per cent. of nitrous gas, and take one third of the diminution for oxygen. When the oxygen (freed from carbonic acid) is judged to be 90 or more per cent. pure, I put 100 parts of nitrous gas of known purity (say 98±) to 100 of the oxygen, and mark the diminution; I next put in 40 nitrous and mark the diminution, and so on, till there is manifestly a slight portion of nitrous left; then this is to be removed by a small portion of oxygen; finally, knowing the quantity of azote which was in the nitrous gas, the rest must have been introduced by the oxygen.

In this way I find a perfect agreement, whether the nitrous test or the hydrogen is used; but with common air the residue is so enlarged with azote as to render the measuring of it not so accurate.

Third Method, by Quadrisulphuret of Lime.

Quadrisulphuret of lime is an excellent test for oxygen, and may be applied to common air or to other mixtures of which oxygen is a part, up to the purest oxygen. As this and other similar compounds seem to me destined to act an important part in chemical operations, it may not be improper here to give some account of their origin and their constitution, as far as actual experiments have demonstrated.

The alkalies and the alkaline earths that are soluble in water have been long known to combine with sulphur, both in the dry and humid way. In the last century they went by the name of *hepar sulphuris*, or liver of sulphur, from their colour.

Scheele was the first to use the quadrisulphuret of lime to abstract oxygen from atmospheric air. Lavoisier also made use of the same article; but it was to De Marti of Spain we owe the most successful attempt with the quadrisulphuret of lime to abstract the oxygen from atmospheric air. His memoir, printed in 1795, and reprinted in the *Journal de Physique*, vol. lii. 1801, may still be read with interest*. All the *hepars*, when dissolved in water, have usually gone by the harsh name of *hydroguretted sulphurets* in our English works of chemistry since the commencement of the present century.

In 1798 Berthollet published an essay on the nature and combinations of sulphuretted hydrogen, with reference to the part it acts in the sulphurets. Proust afterwards controverted some of Berthollet's opinions in the 59th volume of the *Journal de Physique*, 1804. Gay-Lussac, in the 78th volume of the *Annales de Chimie*, 1811, gives some important results on the mutual action of metallic oxides and alkaline hydrosulphurets; he finds amongst other results that no *sulphates* are formed, that water is formed, that sulphites or sulphuretted sulphites, and often metallic sulphurets are formed; and that consequently it is not possible to obtain the simple metallic bases of hydrosulphurets by means of hydrosulphurets of their oxides; and that when a sulphuret is dissolved in water, no sulphate is ever formed, as is commonly imagined, but sulphites and sulphuretted sulphites. Some proofs are afterwards given†. Vauquelin, in the 6th volume of the *Annales de Chimie et de Physique*, 1817, presents us with a laboured

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[* A translation of De Marti's Memoir appeared in Phil. Mag., first series, vol. ix. p. 250.—Edit.]

† See also vol. lxxxv. p. 199.

series of experiments on the alkaline sulphurets, the chief object of which is to ascertain the state of the alkali in the sulphuret, whether it is that of a metal or of an oxide. After many experiments on the sulphurets of potash, soda, and lime in the dry way, and one on sulphuret of lime in the humid way, the author sums up, and notwithstanding his leaning to the opinion that the alkalies exist in sulphurets in the state of *metals*, he is obliged at last to acknowledge "that it is probable, *but not yet demonstrated*, that in all the sulphurets formed by means of the alkaline oxides by a red heat, these last lose their oxygen, and are united to sulphur in the metallic state as is the case with the other metals." Gay-Lussac, in the sequel of the same volume, page 322, in a memoir, animadverts on the before-cited paragraph; and allowing that sulphuric acid is formed when a sulphuret of potash made by a red heat is dissolved in water, he contends, according to a suggestion of Berthollet, that the acid is formed in the instant of solution from the reciprocal action of the sulphuret and the water, rather than from the oxygen of the potash and sulphur. This opinion is countenanced by several combinations of a similar nature, which he has adduced, and which are worth the attention of chemists.

Without adverting at present to my own experiments, I may observe that Sir John Herschel, in an essay in the first volume of the Edinburgh Philosophical Journal, 1819, was the first writer who published an atomic view of the class of salts called sulphuretted sulphites, or hyposulphites, that accorded with what I had long entertained and demonstrated by reiterated and decisive experiments*. In the above-mentioned essay he showed clearly that the hyposulphurous acid is composed of two atoms of sulphur and two of oxygen, which united to one atom of base, as potash or lime, compose an atom of hyposulphite. The formation of those of lime, potash, soda, barytes, and some metallic oxides is more particularly explained. A saturated solution of hyposulphite of lime at 50° he found to be 1·30 specific gravity†.

In the 14th volume of the *Annales de Chimie et de Physique*, Gay-Lussac has given the principal results of Herschel's essays on the hyposulphurous acid with some judicious remarks, but he leaves the subject as one requiring further investigation.

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* See New System of Chemical Philosophy, vol. ii. Preface, and p. 105.

† Dr. Thomson, in a paper on the compounds of chromium in the Transactions of the Royal Society for 1826, disputes the accuracy of this constitution of hyposulphurous acid. I have never had any doubt concerning it since 1815.

In 1822 Berzelius published a memoir on the alkaline sulphurets. The results of his experiments seemed to him confirmatory of the previous notion of Vauquelin. Those experiments were on the sulphurets of potash and lime made in the dry way; he made only one on lime, which agreed very well with the theory; but this very delicate experiment was not enough to establish so important a law of combination, and I do not find that any one besides has obtained the same result *.

Though I am not prepared to deny that sulphurets of potassium and calcium can be obtained by the process of Berzelius, I am quite satisfied that sulphurets of potash and lime, &c. may be easily procured in the dry way: of that of lime I have had numberless instances. As the compounds of sulphur and the alkaline earths have been very little subjected to investigation by chemists in general, we find great vacancy in the accounts given of them by the modern compilers of chemical books. For this reason I shall introduce here a few of the results I have obtained in a long series of experiments on this branch of chemical inquiry.

Sulphuret of Lime, in the dry way.

In 1806 I formed, for the first time, the protosulphuret of lime by heating 50 grains of fallen lime with 50 sulphur in a covered crucible not quite air-tight, so that the escape and combustion of the excess of sulphur might be allowed; when raised to a red heat an addition was made to the weight of the lime; by repeating the dose of the sulphur and heating, a further addition was made to the weight; but repeating the operation a third time seldom made any further addition. The weight of the compound was 65 grains; it was a white powder with a tinge of yellow, not caustic, but bitter to the taste.

In 1809 I examined this powder more minutely, and found it was best made by mixing equal weights of pure hydrate of lime and flowers of sulphur, putting the mixture into a covered crucible and heating it slowly to red; when the escape of the sulphur fumes ceases, cool the contents, and again mix them with the same weight of sulphur as in the first operation, and again heat it as above; at last it will be found that 32 parts of hydrate of lime = 24 lime have combined with 14 of sulphur, or one atom to one†. In the work referred to I have stated that pounded lime and sulphur scarcely form any union by this process, and carbonate of lime and sulphur still less.

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* Annals of Philosophy, 1822.

† See New System of Chemical Philosophy, vol. ii. pages 99 and 102.

An ingenious pupil of mine, Mr. William Barnett Watson of Bolton, has succeeded in uniting lime and sulphur by heat; instead of taking pounded lime, which has a harsh gritty feel, he takes hydrate of lime, and expels the water by a red heat continued till 32 parts of hydrate are reduced to 24; this is a fine soft powder; when 24 parts of this pure and finely divided lime freed from water are well mixed with 24 parts of sulphur and heated red in a covered crucible, a partial combination takes place, and an increase of weight to the lime; this operation is to be repeated till the additional weight becomes 14 grains, after which no further addition can be effected. Mr. Watson found it require several repetitions. I have since found it may be effected by two or three only. This sulphuret is not used in eudiometry.

Quadrisulphuret of Lime, in the humid way.

When sulphur and hydrate of lime in almost any proportions are boiled together in water, quadrisulphuret of lime is formed and dissolved in the water; the solution is of a deep yellow colour, and has a very bitter taste. I have not seen in any author the proportion that ought to be used, nor the quantity and specific gravity of the liquid solutions. These are subjects which have engaged my attention. If lime is in excess, the liquid consists of *lime-water* holding in solution quadrisulphuret of lime. If sulphur is in excess, the liquid consists of *water* holding in solution quadrisulphuret of lime. I have long known that the œconomical proportions to be used are 32 parts of dry hydrate of lime by weight with 56 of sulphur, that is, one atom of lime with four atoms of sulphur. If more lime than that above be used, it will be found prevalent in the residue; if more sulphur, then the redundant sulphur will be found in the residue. A few ounces of the mixed ingredients may be gently boiled in an iron pan for an hour or more, stirring the liquor occasionally, and covering the pan with a lid to prevent the too free admission of atmospheric air. Or, in order to prevent the action of oxygen on the liquid, a flask may be substituted for the pan; the materials may be put into the flask nearly filled with water, and the flask loosely corked, may be immersed in a pan of boiling water so as to be almost covered by the water. The liquor to be preserved should be kept in green glass bottles nearly full, and having ground stoppers. After the boiled liquor has cooled and the sediment subsided, the clear liquor may be decanted, if it be strong or deep coloured the sediment may be washed with a little water, and another quantity of the liquor obtained of inferior strength. The sediment may be dried if necessary, and subjected to analysis, as I have mostly done. The quantity and specific gravity of the clear liquors should then be ascertained.

The first quadrisulphuret of lime I made was in 1804; it was very weak, since it only absorbed one fourth of its bulk of oxygen gas; the next that was made took its bulk of oxygen. The next, made in 1806, took 2½ times its bulk of oxygen. In these no account was taken of quantities or residues of lime and sulphur. After this I saw the necessity of investigating, (1.) the quantities of lime and sulphur mixed; (2.) the quantity and specific gravity of the liquid obtained; and (3.) the quantity and proportion of the materials left in the residue, in order that the rationale of the changes effected might be explained. From 1806 to the present time (1837) I have made no quadrisulphuret of lime without attending to all those particulars. In this period I have made it 23 times, six of which were in flasks, and the rest in iron pans covered as mentioned above; the difference of the two methods I found to be very little: it consisted chiefly in traces of sulphuret of iron being found in the residues when pans were used.

A few trials of the various liquids obtained soon furnished me with a formula for ascertaining the quantities of sulphur and lime in a liquid of given specific gravity; namely, multiply the three leading decimals in the specific gravity of the liquid by 13, and the product will give the aggregate weight in grains of sulphur and lime in 1000 water grain measures of the liquid; of this aggregate $\frac{9}{13}$th will be sulphur, and $\frac{4}{13}$th lime.

With regard to the residue after boiling and its analysis, it is obvious the residue must consist chiefly of sulphur and lime, which for want of due continuance of the ebullition have escaped combination; and there may be some impurities in the sulphur, or the hydrate of lime may not be free from carbonate, &c.; but when the residue is comparatively small no material disturbance of proportions in the quadrisulphuret can take place. If the residue be chiefly sulphur, its quantity may be approximated by ignition; but if lime is in excess, it may be estimated by the quantity of muriatic acid required to saturate it.

The following table exhibits a selection of the principal varieties in the proportions of ingredients and products obtained so as to illustrate the foregoing statements.

Table of Proportions in Quadrisulphuret of Lime.

	Quantities of hydrate of lime and sulphur mixed.	Proportions of lime and sulphur.	Quantity of liquor obtained in water grain measures, and quantities of lime and sulphur in it.	Measures of oxygen required to saturate 100 liquid.	Quantity of residue when dried.
	Hydrate.	Lime. Sulph.			
1	120 = 90 lime + 210 sulphur.	4 : 9⅓	3100 of 1·056 containing 70 lime + 156 sulph.	900	56 = 16 lime + 40 sulph.
2	50 = 37½ lime + 50* sulphur.	4 : 5⅓	2200 of 1·0240 contains 21 lime + 47 sulph.	400	20 = 12 lime + 4 sulph. + loss
3	150 = 112½ lime + 200 sulphur.	4 : 7+	1450 of 1·146 containing 85 lime + 190 sulph.	2350	†20 = 7 lime + 13 sulph.
4	96 = 72 lime + 168 sulph.‡	4 : 9⅓	2800 of 1·056 containing 63 lime + 141 sulph.	900 §	34 = 9 lime + 25 sulph.
5	35 = 26 lime + 140 sulph.	4 : 21·6	1600 of 1·037 containing 23·7 lime + 53·3 sulph.	600 §	83 all sulph.

[To be continued.]

XXV. *On the Divergence of the numerical Coefficients of certain Inequalities of Longitude in the Lunar Theory.* By J. W. Lubbock, *Esq.*, *F.R.S.*||

THE divergence of the numerical coefficients in the lunar theory, made manifest by M. Plana's development of the expressions according to powers of m, presents a difficulty in a complete numerical solution of the problem, that is, a solution intended to embrace all quantities which are sensible in practically ascertaining the moon's place with the accuracy required for comparison with the best observations. But the following questions naturally occur: Is there any method of approximation which will serve to select the more considerable terms, rejecting others? Is the divergence due chiefly to the development and expansion, according to powers of m, of the divisors introduced by integration? In the latter case the difficulty might be easily avoided; but I fear that each of these questions must be answered in the negative.

In order to illustrate this point I have selected indifferently two terms in the longitude amongst those in which this divergence is met with, and I propose to examine their construction without introducing details which do not bear immediately upon the point referred to.

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* Boiled in a flask loosely corked.

† Lost some of the ingredients by boiling over; hence a deficiency.

‡ Boiled in a flask with great care.

§ The oxygen was determined by especial care in these two cases.

|| Communicated by the Author.

λ denotes the moon's longitude, ξ and $\xi_{,}$ the mean anomalies, e and $e_{,}$ the eccentricities of the moon and sun.

$$\lambda = \lambda_{14}\, e\, e_{,} \sin (\xi - \xi_{,}) + \lambda_{15}\, e\, e_{,} \sin (2\tau - \xi + \xi_{,}) + \&c.$$

$$\frac{\mathrm{a}}{r} = r_{14}\, e\, e_{,} \cos (\xi - \xi_{,}) + r_{15}\, e\, e_{,} \cos (2\tau - \xi + \xi_{,}) + \&c.$$

$$\xi - \xi_{,} = \mathrm{c\,n}\,t - m\,\mathrm{n}\,t \qquad 2\tau - \xi + \xi_{,} = 2\,\mathrm{n}\,t - m\,\mathrm{n}\,t - \mathrm{c\,n}\,t$$

$$\mathrm{c} = 1 - \frac{3}{4} m^2 - \frac{225}{32} m^3 - \frac{4071}{128} m^4 + \&c.$$

I find from the equation

$$\lambda = \int \frac{h}{r^2} \left\{ 1 - \int \frac{\mathrm{d}R}{\mathrm{d}\lambda} \mathrm{d}t \right\} \mathrm{d}t$$

$$\lambda_{14} = \left\{ \frac{21}{4} m + \frac{1065}{32} m^2 + \frac{12615}{64} m^3 + \&c. \right\} \left[1 + m + \frac{7}{4} m^2 + \&c. \right]$$

$$= \frac{21}{4} m + \frac{1233}{32} m^2 + \frac{15333}{64} m^3 + \&c.$$

$$\lambda_{15} = \left. \frac{15}{4} - \frac{15}{4} m - \frac{53}{32} m^2 + \frac{49321}{384} m^3 + \&c. \right\} \left[1 + m + \frac{m^2}{4} + \&c. \right]$$

$$= - \frac{15}{4} m - \frac{173}{32} m^2 + \frac{48325}{384} m^3 + \&c.$$

Lubbock on the Lunar Theory, p. 193.

It is evident that the divergence of the coefficients $\frac{15333}{64}$ and $\frac{48325}{384}$ is not due chiefly to the quantities $1 + m + \frac{7}{4} m^2$ and $1 + m^2 + \frac{m^2}{4}$ which arise from the expansion of divisors introduced by integration, nor is the expansion of these divisors according to powers of m a step attended with labour or difficulty, and on that account desirable to be avoided. The diverging quantities $\frac{12615}{64}$ and $\frac{49321}{384}$ arise from the summation of the following quantities in the value of $\frac{\mathrm{d}\lambda}{\mathrm{n\,d}t}$;

$$\frac{12615}{64} = - \frac{675}{32} + \frac{315}{64} - \frac{7}{4} - \frac{45}{32} + \frac{89}{16} + \frac{1687}{8}$$

$$\frac{49321}{384} = \frac{9}{16} - \frac{39}{16} + \frac{5}{4} + \frac{63}{32} - \frac{13}{16} + \frac{49277}{384} - \frac{91}{24}.$$

The principal terms are evidently $\frac{1687}{8}$ and $\frac{49277}{384}$: these arise from the coefficients r_{14} and r_{15} belonging to the same arguments in the reciprocal of the radius vector; so that with respect to these only, and to terms of the same nature in other arguments which I have examined, a tolerably approximate value would be obtained from the expression

$$\lambda = 2\int\left\{\frac{\mathrm{a\,n}}{r}\right\}\mathrm{d}t$$

r_{14} and r_{15} are thus deduced from the well-known equation

$$\frac{\mathrm{d}^2.\,r^2}{2\,\mathrm{d}t^2} - \frac{\mu}{r} + \frac{\mu}{a} + 2\int \mathrm{d}R + \frac{r\,\mathrm{d}R}{\mathrm{d}r} = 0$$

If $\qquad \dfrac{\mathrm{a}}{\mathrm{r}} = 1 + p \qquad\qquad \mathrm{a}^3\mathrm{n}^2 = \mu$

$$\frac{r^2}{2} = \frac{\mathrm{a}^2}{2} - \mathrm{a}^3 p + \frac{3}{2}\mathrm{a}^4 p^2 - 2\,\mathrm{a}^5 p^3 + \&\text{c.}$$

if $p = \Sigma\, r_n E_n \cos(i\,\mathrm{n}\,t + q) \qquad \mathfrak{r} = \dfrac{3}{2}\,\mathrm{a}^2 p^2 - 2\,\mathrm{a}^3 p^3 + \&\text{c.}$

$$2\int \mathrm{d}R + \frac{r\,\mathrm{d}R}{\mathrm{d}r} = \frac{\mu P}{\mathrm{a}}.$$

The differential equation gives for determining

$$r_n,\; r_n[i^2-1] = \mathfrak{r}_n\, i^2 - P_n,$$

n at foot being the index of the argument.

$$r_{14} = \frac{21}{8}m + \frac{1113}{64}m^2 + A_{14}\,m^3 + \&\text{c.}$$

$$r_{15} = -\frac{15}{8}m - \frac{17}{64}m^2 + A_{15}\,m^3 + \&\text{c.}$$

$$\left\{\frac{21}{8}m + \frac{1113}{64}m^2 + A_{14}m^3 + \&\text{c.}\right\}\left[-2m - \frac{1}{2}m^2 - \frac{201}{16}m^3 + \&\text{c.}\right]$$
$$= \left[1 - 2m - \frac{1}{2}m^2 + \&\text{c.}\right]\left\{-\frac{9}{4}m^2 + \frac{267}{32}m^3 + \frac{8049}{64}m^4 + \&\text{c.}\right\}$$
$$-2\left\{\left[2 + m + m^2 + \&\text{c.}\right]\left\{\frac{3}{4}m^2 + \frac{759}{64}m^3 + \frac{9265}{128}m^4 + \&\text{c.}\right\}\right.$$
$$\left. + 24\,m^4 + \&\text{c.}\right\}.$$

Lubbock on the Lunar Theory, p. 177.

$$\left\{-\frac{15}{8}m - \frac{97}{94}m^2 + A_{15}m^3 + \&\text{c.}\right\}\left[-2m + \frac{5}{2}m^2 + \frac{201}{16}m^3 + \&\text{c.}\right]$$
$$= \left[1 - 2m + \frac{5}{2}m^2 + \&\text{c.}\right]\left\{-\frac{3}{4}m^2 - \frac{221}{32}m^3 + \frac{6133}{384}m^4 + \&\text{c.}\right\}$$
$$-2\left\{\left[2 + m + m^2 + \&\text{c.}\right]\left\{-\frac{9}{8}m^2 - \frac{3}{8}m^3 + \frac{12289}{256}m^4 + \&\text{c.}\right\}\right.$$
$$\left. - \frac{87}{32}m^4 + \&\text{c.}\right\}.$$

Equating coefficients,

$$-\frac{4221}{128}-\frac{1113}{128}-2A_{14}=\frac{8049}{64}-\frac{267}{16}+\frac{9}{8}-\frac{9265}{32}-\frac{759}{32}-\frac{3}{2}-48$$

$$-\frac{3015}{128}-\frac{485}{128}-2A_{15}=\frac{6133}{384}+\frac{221}{16}-\frac{15}{8}-\frac{12289}{64}+\frac{3}{4}+\frac{9}{4}+\frac{87}{16}$$

$$A_{14}=\frac{1687}{16} \qquad A_{15}=\frac{49277}{768}.$$

In order to judge of the relative magnitude of these several fractions, I divide the numerators by the denominators, and retaining only whole numbers, I get

$$-32-8-2A_{14}=125-16+1-289-23-1-48$$

$$-23-3-2A_{15}=17+13-1-192+0+1+6$$

$$A_{14}=105 \qquad A_{15}=64.$$

The leading terms -289 and -192 arise from the development of R, and would be included in the following expression:

$$-\frac{d^2\,a^2\,\delta\frac{1}{r}}{dt^2}-\mu\,\delta\frac{1}{r}+4R=0.$$

This approximation would save the calculation of some quantities, but it evidently could not be safely adopted, and it would still leave the calculation of R, which is extremely troublesome. Very little trouble would be saved by not developing the divisors introduced by integration, or the quantities in square brackets. The quantities $\frac{9265}{128}$ and $\frac{12289}{256}$, which belong to the development of R, arise as follows, from a multitude of diverging fractions, andI think it would be impossible to give any safe rule for selecting the principal terms.

$$\frac{9265}{128}=\frac{1161}{128}-\frac{243}{256}+\frac{525}{128}+\frac{3}{4}-\frac{63}{8}-\frac{3}{8}+\frac{3}{8}+\frac{3591}{256}-\frac{75}{128}$$
$$-\frac{1}{4}+\frac{9}{8}+\frac{21}{8}-\frac{519}{128}+\frac{357}{64}-\frac{693}{64}-\frac{33}{64}+\frac{5523}{128}$$
$$-\frac{51}{64}+\frac{99}{64}+\frac{231}{64}+\frac{405}{32}$$

$$\frac{12289}{256}=-\frac{81}{128}+\frac{3483}{256}+\frac{1}{4}+\frac{27}{8}+\frac{513}{128}-\frac{3}{16}+\frac{3}{8}-\frac{3}{4}+\frac{3699}{128}-\frac{15}{16}$$

$$8R=-a\left(\frac{dR}{da}\right)r\,\delta\frac{1}{r}+\left(\frac{dR}{d\tau}\right)\delta\lambda.$$

The term $\frac{5523}{128}$ arises from the combination of the term $\frac{263}{16}m^2e\sin(2\tau-\xi)$ in the longitude $(\delta\lambda)$ with

$\frac{21}{4} m^2 e_{,} \sin(2\tau - \xi_{,})$ in $\frac{dR}{d\tau}$ and the term $\frac{3699}{128}$ arises from the combination of $\frac{1233}{32} m^2 e e_{,} \sin(\xi - \xi_{,})$ in the longitude $(\delta\lambda)$ with $\frac{3}{2} m^2 \sin 2\tau$ in $\frac{dR}{d\tau}$. These are the most considerable, but it would evidently be impossible to employ with safety any rule of approximation which did not embrace other terms. In this and in other cases in the Lunar Theory it will be found that coefficients, when formed correctly, are made up by the addition of numerous small terms, which come from various sources: hence the danger of attending only to the leading or principal terms which may occur upon an incomplete examination, hence also the extreme practical difficulty of the problem in whatever manner it be approached.

In the method which M. Plana has adopted, first, as is well known, the mean longitude of the moon is obtained in terms of the true longitude, and the true longitude is afterwards found in terms of the mean longitude by reversion. But the divergence of the numerical coefficients exists equally in the former expression, and does not arise in the operation of reversion.

XXVI. *Some Observations on the Development of the Organization in Phænogamous Plants.* *By* Dr. M. J. SCHLEIDEN.*

[With a Plate.]

> Nullo modo generationem explicasse judicare possum eos, qui ne ullam quidem partem, ne ullum attributum quidem corporis ex traditis suis principiis explicuerunt, sed sermones saltem de ea re fecisse, utcunque doctos, veros et elegantes.
>
> C. F. WOLFF,—*Theoria Generationis.*

ALTHOUGH it must be granted that Linnæus had a tolerably clear idea of the metamorphosis of plants, yet the introduction of this doctrine and its reception into the higher botany takes its date from Goethe. Long, however, before Goethe, the ingenious C. F. Wolff had shown how fruitful this idea could be rendered; but his work was little read by the botanists of the time, not at all understood, and soon forgotten. Thus the science, to its prejudice, did not gain possession of this doctrine through Wolff, in whose hands it would probably have become so fertile, but through Goethe, and owing to the manner in which it was introduced by him, it has hitherto been of comparatively but little service. If we understand by the term metamorphosis the principle that a plant has only a certain limited number of different fundamental organs, and that all other organs distinguish themselves from these potentially, only inasmuch as the tendency exists in them of allowing a certain peculiar degree of development and variety of form, which, however, is not so absolute but that it can be suppressed under certain circumstances, allowing the usual form of the organ to manifest itself,—I say, if we lay down this principle as a foundation, it is clear that this doctrine must furnish the most important results for the science, and give it an internal unity which no other branch of empirical natural science has hitherto obtained; that is, if this idea can be substantiated by fact; and it must only be received so far as it can be proved, since as much as does not actually exist in nature and cannot be perceived by the senses, is no longer an object of natural science and can never serve to extend our knowledge of the material world.

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* From Wiegmann's *Archiv für Zoologie*, Part IV., Berlin, 1837.

[The Editors are indebted to the kindness of Dr. Wood, of Bristol, for the translation of this paper.]

C. F. Wolff adopted the only correct plan, that of the study of the development, and proved the identity of the greater part of the foliaceous organs quite satisfactorily. He was, however, not at all appreciated, and Goethe was the first who introduced the doctrine of metamorphosis, but not as an induction arising out of practical consideration of the process of development, but as the speculative result of the comparison of the different forms of the developed organ. Now such a comparison may certainly lead us to conjecture the existence of such a law, but can never lead to its absolute establishment. Goethe says in another place:

> "Alle Gestalten sind ähnlich, doch Keine gleichet der Andern;
> Und so deutet der Chor auf ein geheimes Gesetz."
>
> Analogy pervades all forms,—yet all unlike;
> The whole thus indicate a hidden law.

Thus it was that the botanists received this important doctrine, one which was capable of yielding such valuable results, under a wrong light, since it was presented to them only as a philosophical idea, and indeed it seems as if a tolerably general conviction prevailed that the demonstration of what was true in the theory was not possible. At a later period Francis Bauer, who like Wolff was no botanist, again had recourse to the only correct method, inasmuch as he traced the individual organs to their original forms, in order to explain their proper nature; his investigations have, however, unfortunately become too little known and have been used to advantage by scarcely any one else than Robert Brown.

In this manner has the doctrine of metamorphosis or the morphological design of the organs of plants gradually formed itself into a peculiar department of scientific botany; and at the same time that it has offered a field for the exertions of the most celebrated men, upon which they have obtained lasting honour, it has become the appropriate theatre for all friends of enigmas, dreamers, and dealers in paradoxes, and has served for the display of the most wonderful things, which have been unhesitatingly dignified with the proud name of Philosophy or Speculation. Speculation, however, is only admissible where our means of observation fail us; and if it thrusts itself forward, desirous of taking the place of observation, we should do wisely to get rid of it as a tiresome guest. How much might we be in advance of our present position, even in the speculative sciences, were it not that speculation devoted itself to objects, which, consuming its best time and best powers, not only did not require it, but would even have been better without it! It is precisely in the history of development that examples of this sort are particularly frequent. If therefore the prosecution of this branch of science shall attain importance and become established in all its parts, it will not suffice that investigations be commenced on a bean or something of that sort which may conveniently be dissected with a penknife; a much earlier period must be chosen, that of the first origin of the embryo. A ripe seed presents the young plant already provided with such manifold organs, that a wide field is here opened to mere speculation sufficiently extensive to render subsequent investigations vague and unprofitable.

Upon its first appearance the embryo is recognised as a membranous cylinder (Pl. III., fig. 9 & 13) rounded and closed superiorly, but open inferiorly, since the membrane constituting the embryo is invariably continued into the sac containing it (appearing indeed to be merely a reduplication of that sac) and filled with an organizable, for the most part pellucid fluid mass, which becomes gradually converted into cells, beginning from above downwards, (fig. 6 & 10) during which process the cellular nuclei also become apparent (fig. 12 & 24), which appear at all times to perform a principal part in the formation of cells. At this point a leading phænomenon in vegetable life finds its explanation. The embryo originally consists of axis alone, and being closed superiorly, only allows a further development from within outwards, but is not limited inferiorly; and by the secretion of organizable matter becoming transformed into cells, admits of unlimited prolongation; whence not only the direction but the mode of growth of the stem and root, differing as they do, become intelligible. During the second stage of the development the upper end of the germ expands in a globular form, (fig. 6, 7, 11, 12, 14, & 15,) and from the sides of this globular extremity, in dicotyledonous plants, the two rudimentary cotyledons become developed as cellular projéctions, their points being more or less free* (fig. 16 & 17). In these, as also in the stem itself, the elongated cells and spiral vessels are not formed until a much later period: the mode in which this growth takes place was in its principal features described with perfect accuracy by C. F. Wolff. In the monocotyledonous plants, on the other hand, an asymmetrical elevation is formed at the summit of the cylindrical embryo (fig. 8), which ultimately constitutes the cotyledonous leaf surrounding the stalk, and which also subsequently incloses more or less the terminal bud (*plumula*) †. This process offers the second and greatest difference to which a plant can lay claim, namely, the antagonism between vertical longitudinal formation and horizontal superficial extension.

All subsequent development of the plant, and every later formed organ, are only modifications of these two portions of the axis, the *stem*; and of the lateral organs, the *leaves*. This antagonism therefore appears to be something original; indeed the axis is formed at an *earlier* period than the cotyledons, from which may be seen the great error of that opinion which considers the stem to consist of adherent leafstalks, and the terminal bud to be an axillary one, as for instance Agardh does. The most important points of difference in the cotyledons are again repeated in the leaves also, which are indeed only after-formations of those organs: thus for example we find in the *Stapeliæ*, in which the cotyledons are very small, that the leaves are also rudimentary, and in *Cuscuta* the absence of the cotyledons in the embryo even points to the subsequent habit of the plant. The close agreement between the cotyledon of grasses and the leaves has already been alluded to in the preceding note.

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* *Punctum vegetationis*, according to C. F. Wolff.

† It will be seen from this description of the process of development that every monocotyledonous embryo possessed originally a *plumula exserta*, and that wherever this is inclosed, there must invariably be a fissure present, however fine it may be. The grasses have been usually understood to belong to the families with a *plumula exserta*, but quite incorrectly, since the *plumula* in this family becomes *perfect* by means of an elevation of the cotyledon closed with the exception of a very fine slit (the outer closed leaf of botanists), and this portion of the cotyledon, like every other of its peculiarities, becomes repeated in the subsequent leaves in the analogous formation of the ligula, whilst the *scutellum*, constituting the principal portion of the cotyledon, corresponds to the leaf itself. Sometimes the cotyledon folds itself together once more, as in *Zea Mays*, which formation has been falsely compared to the fissure of the cotyledon in the *Aroideæ*; or it sometimes forms on its anterior surface small protuberances, which however cannot be considered as second cotyledons, since they are in connexion with the axis at a point lower down than the cotyledon itself; and a second leaf cannot possibly be formed beneath the earlier one.

The investigation of the laws of position of the leaves forms a very interesting section of this inquiry; the manner in which the varying relations of foliage become developed out of the originally opposed and perfectly cotemporaneous cotyledons, until nature often appears to return at the extremity of a plant to the original type of two opposite leaves. The consideration of this subject would however lead me too far beyond the limits of these brief remarks.

It will be unnecessary that I should make any remark as to the calyx and corolla being foliaceous organs, since that is universally received. I will merely remark that in all monopetalous calices and corollæ, those parts which at a later period are joined together, forming the single leaf, are in the earlier stages without exception so independent as to render all discussion as to the number of the individual parts superfluous, since it is a matter of investigation to be established by actual evidence. Every flower then has in its earliest development a regular construction, and the supposed abortions which sometimes appear in the arrangement of the leaves, and which have often been so completely misunderstood, especially whilst the diversity of the laws as to number was disregarded, are therefore entirely unfounded, wherever they cannot be actually proved.

The *Euphorbiæ* have with injustice been denied the benefit of their original structure. Their *involucrum* is not formed out of five leaves, but out of two quintuple verticils, of which the outer one develops the glands; these even exhibit earlier than the five inner leaves a middle nerve with evident spiral vessels, which cannot therefore be considered to be *vasa recurrentia* from the other. There can nowhere be found a better example of the original regularity of structure of the flower than in the grasses, in which the flower becomes at a later period so contorted by unequal development, adhesions, and suppressions of individual parts that every possible explanation has been offered excepting that which nature herself offers. For instance in *Secale cereale*, the *spicula* consists of a lateral *rachis*, on which about five alternating flowers are formed. The superior three of these, together with that portion of the axis which appertains to them, remain in a rudimentary state, whilst on the other hand the two inferior are at first perfectly regular in their development. In the axilla of every bractea (*gluma* auct.) we find a flower consisting of a calyx of three parts, each leaf of which is completely divided from the others, equally large, and standing at the same height; the two inner leaves become gradually united, and form with the external one, which has grown disproportionately large, the later developed *paleæ* auct. Of course under these circumstances the inner one possesses the two central nerves of the formerly separate leaves. With these parts belonging to the calyx there alternate three corollary leaves (*squamulæ* auct.), forming an inner circle, and in like manner standing at the same height; of these, that which is directed towards the axis becomes at a later period abortive through pressure. We find also further, three stamina *alternating regularly* with these corollary leaves, the two inner of which, although at a later period, are thrust by the lateral pressure towards the side of the ovarium; lastly, the basis of the entire flower, the very short *pedunculus*, cannot, on account of pressure, extend itself horizontally upon the secondary rachis, and is therefore forced to ascend upon the inner side, by which means that part of the flower which is directed towards the *rachis spiculæ* assumes an apparently greater elevation than the outer does. In this manner probably we may be able to explain in a simple manner the apparently complicated development of the flower in the *Gramineæ*.

We will now pass to the consideration of the stamens. These are more deserving of attention, since some (among others Agardh, following Wolff, whom, however, he does not quote, although he is otherwise well acquainted with him,) have appeared disposed to give them the character of buds; the opinions also concerning the formation of the anthers are not yet unanimous.

It is evident likewise from the study of their development that the stamens are modified leaves, for they constantly appear at a *later period* than the petals (although they afterwards develop themselves more rapidly); they stand at first higher up upon the axis than the preceding circle of corollary leaves, and alternate invariably with them; by this means, and from the smallness of the individual parts, the relative proportions can be much easier observed*, and for this reason they cannot be axillary buds of the calyx.

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* In some families the petals and sepals, (as is frequently the case with the stamens) or indeed other *perigonial parts*, consist of more than one circle of leaves, as, for instance, in the *Berberideæ* of 2-3 leaved, in the *Thymeleæ* of 2-leaved circles; we can here therefore speak of opposition with as little correctness as in the *Liliaceæ*. Whenever actual opposition of the outer circle of stamens towards the inner circle of petals occurs, it will always be found that an intermediate circle of stamens has become abortive.

The incorrectness of Agardh's view is also made evident by a consideration of those flowers, in which the internode between petals and stamens is perfectly developed, as in some *Capparideæ.*

The regularly formed leaf consists of a central rib, on each side of which there is a twofold cellular tissue, between which the nerves take their course. In this manner is the anther naturally formed, whose superior and inferior cellular tissue* is converted into pollen on both sides of the principal nerve; thus is formed the anther with four cells, which we find to be the general law.

I have found the anther before its bursting quadrilocular in more than one hundred families; amongst these I may name *Gramineæ*, *Cyperaceæ*, *Liliaceæ*, *Labiatæ*, *Borragineæ*, *Scrophularineæ*, *Synanthereæ*, *Umbelliferæ*, *Ranunculaceæ* with its allies, *Rosaceæ* (Juss.), and the *Leguminosæ*, which alone constitute almost one-half of the entire vegetation of the globe. It has been often asserted that the anther could not originally be quadrilocular, since it springs open with two fissures only; that is as much as to consider two chambers as one, because they have not folding doors, but simple doors placed close together. Properly speaking every anther really bursts open with four fissures; they appear however only as two because each pair lies at the sides of the common septum. The difference between quadrilocular and bilocular anthers of descriptive botany, (here however the *Antheræ dimidiatæ* and some few others must be excepted,) consists in this alone: whether the valves detach themselves from the septum earlier or later, in the close observation of which we may distinguish every state of transition.

Sometimes, though rarely, the original middle layer is not developed, and in this case of course the division into two lateral cells is not found. Still more rarely is the one lateral half of the leaf only developed into an anther, the other retaining its leafy character; this condition is the type of the *Marantaceæ*, and occurs frequently as monstrosity in the conversion of the floral leaves into stamens, or of stamens into petals. In both cases, however, the course of the epidermis proves incontrovertibly (what is likewise established by the study of development) that the pollen forms itself in the interior of the leaf, and that therefore the anther cannot be considered as a leaf rolled up either backwards or forwards, which produces the pollen upon its surface.

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* The normal leaf, as is well known, exhibits upon its upper surface cellular tissue, different in structure from that on the under; to this we find that the pollen of the anterior and posterior cells of those compartments corresponds. It may perhaps be possible, and certainly not uninteresting, to ascertain by experiment, whether or not the pollen of one of these compartments only possess the external characters of pollen, and likewise different functions in the process of impregnation, or whether in Diœcious plants one kind would produce male, the other female embryos.

If we carry back our investigations of the anther as far as its first appearance, we find that in every family it goes through just the same conditions, and that all the apparently deviating characteristics of this organ in the *Orchideæ*, *Asclepiadeæ*, *Cucurbitaceæ*, *Stylideæ*, &c., are merely later unfoldings of the same fundamental type, and are only physiologically unimportant modifications of the same plan, which nature, here as everywhere else where external differences of form only are concerned, has made the subject of so great and wonderful variety.

The formation of the pollen takes place in this manner: the four groups of cells intended for the pollen separate themselves from the remaining tissue of the leaf, their individual cells continually increasing, and in the interior of each probably for the most part four other cells are formed, in each of which a grain of pollen is produced, upon which the original cells become entirely reabsorbed. The four pollen grains often appear to be developed in one cell, if we decline the assuming that the delicate cells, closely surrounding them, have been overlooked. Sometimes, although seldom, there are only two grains of pollen found in the larger original cell, for instance, in *Podostemon ceratophyllum*, which in that case afterwards remain adherent one to the other, (figs. 29 and 30). Yet the quadruple number is undoubtedly the general rule, which explains the frequent occurrence of *pollen quaternarium*.

If, however, the reabsorption of the original cells does not take place, or is not perfect, a very peculiar arrest of development occurs, which being the constant type in the *Orchideæ* and *Asclepiadeæ*, has afforded botanists abundant occupation, whilst the entire peculiarity consists in this, that the pollen stops short at an earlier point in its development. This same condition may be seen as a temporary stage in the development of the flower of *Picea* and *Abies* in January and February, in *Pinus* in February and March, in which a loose waxy pollen-mass may be found imbedded in each division of the anther. At a somewhat later period we may see the four cells in *Picea* and *Abies*, in which the four grains of pollen lie closely united, and it offers a very pleasing spectacle when we observe under the microscope each grain expand itself by the absorption of water until it bursts its case in order to escape, leaving the four cells emptied of their contents (figs. 25 to 28).

In this way we are enabled to recognise in the formation of the anthers only a stage in the development of the lateral organs of plants.

If we proceed further we next meet with the ovarium, the object and aim of the entire vegetable organization. In this we find all the constituent parts so closely condensed that their distinction appears very difficult, and it is here that the most extended stage has offered itself for hypotheses of all descriptions; indeed many have advanced the most extravagant speculations, relying upon their powers of guessing more than upon their talent of observation, by which it cannot be denied some fortunate hits have been made; of such Agardh's *Organographie* offers a series of capital examples.

According to the general, and at present commonly received view, the ovarium consists of buds (*ovula*), which develop themselves on the borders of leaves (*carpella*).

If we examine this view upon the usual grounds, we detect unfortunately a logical incorrectness in the reasoning which alone can be advanced and asserted in its support. And this is not the only case in which an entirely unfounded assumption has crept into science long ago, and which supported by tradition has been esteemed sacred and impregnable, so that no one has ventured to deprive the assumed deity of its veil, and show that this adoration had been prostituted before a vain puppet. We observe continually a sort of dread for the high authorities which first introduced such a doctrine; whilst in natural science nature herself should be the only lawful authority, and it is only in cases where she cannot be made subservient to our inquiries that any other testimony should be tolerated.

If we contemplate the entire range of the vegetable world we shall recognise this universal law, that a bud never forms itself on a leaf but from the axis of the plant or its derivative organs alone. If therefore the ovula are considered as buds, we must, as matter of course, conclude that the placenta is an altered axis. But what are the grounds which have been adduced in order to controvert this simple and necessary conclusion?

1. The well-known phænomenon in *Bryophyllum*; and
2. A monstrous development of gemmæ on the leaf of a *Malaxis* and an *Ornithogalum* observed twice.

The latter case is an abnormal product, and therefore least of all suited to establish a general rule, which is in contradiction to all known phænomena, and which will, as well as the other case, be presently explained. The first case constitutes, however, a singular exception. But I cannot help expressing a doubt as to its being really an exception, and would ask if the said leaf may not perhaps be a foliaceous expanded stalk. How long have such grounds been deemed sufficient to overturn a general rule, naturally deducible out of the principle of unity? It is further a recognised axiom in logic, that an hypothesis is by so much the more allowable, the easier it explains *all* phænomena connected with it, and the less it stands in need of other hypotheses for its support. Now I ask, in order to take an extreme case, what abnormal assumptions are not rendered necessary in the explanation of the true *placenta centralis libera* according to the usual mode, as, for instance, in the *Plumbagineæ* (figs. 20 to 23)? Here the five carpellary leaves would have been bent inwards, have united by their edges, then have separated themselves once more from their edges, would have again expanded, and then have united to one another anew; and lastly of at least ten ovula, nine would be abortive, the remaining one having in addition taken a remarkable position upon the summit of the central pillar; and be it remarked, all this would occur without the possibility of discovering even one step of so complicated a process in the plant itself. It would indeed be universally necessary to have recourse to the supposition of an abortion in all uniovulate ovaries, a circumstance not in the least corroborated by an appeal to nature.

The second and opposite case is however almost more dangerous still as respects the usual view; for when the entire surface of the carpellary leaf bears ovules, as is the case in the *Gentianeæ*, *Nymphæaceæ*, *Butomeæ*, &c., I know of no tenable explanation of this phænomenon deducible through the common hypothesis. This has made it necessary to have recourse to many explanations; sometimes the ovula are represented as formed on the edge of the carpellary leaf, sometimes on the central nerve*, and sometimes on both.

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* Thus in a work by a M. Eisengrein, entitled "*Die Familie der Schmetterlingsblüthigen mit besonderer Hinsicht auf Pflanzen-Physiologie*," it is advanced as a law that in the *Leguminosæ* the ovules are formed on the middle nerve. Independently of the circumstance that the position of the different parts of the flower shows that in this family the convoluted borders of the leaf are the seat of the ovules, M. E. might easily have convinced himself of the uselessness of his lengthy observations, had he taken the trouble to examine a bean-bud with a tolerably strong magnifying glass. I feel indeed disposed to consider the book altogether as a pathological symptom of the spirit of the age. It combines the most barren trifling with empty comparisons, in the style of a modern, but already expiring school, and this is put forth as philosophy! The book discloses as little investi-

In this manner an extravagant view has been thrust upon the science, founded upon the weakest possible grounds, the circumstance itself been loaded with difficulties, and the natural condition most completely neglected. We shall see further on how easy is the explanation of the only apparently contradictory fact of the *placenta parietalis*, by the assumption that the placenta is a formation of the axis (*Axengebilde*), which indeed may be proved, without the assistance of hypothesis, from the well-known modifications of the stalk. But if we pass over to the investigation of nature, we find—to commence with the most simple conditions—that each individual carpellum is at first quite isolated, constructed similarly to every young leaf or lateral organ of the plant. It is not until a much later period of their development that it begins to direct its edges inwards when the carpellum is closed, or to adhere to the neighbouring edges when the pistil is *unilocular* and *many-leaved.*

Amongst those families which in this respect again deviate from the ordinary plan, must be included the *Gramineæ* and *Cyperaceæ*. In both families their development shows that the ovarium consists of *one* carpel only. In both families the two anterior* stigmata for the carpel are merely a further development of the *ligula*; the posterior, however, which is so often abortive in the grasses, is analogous to the surface of the leaf, and the ovarium itself to the sheath of the leaf.

We can now take a review stage by stage of the entire development of the pistil, from its first appearance as a flat foliaceous organ, until it becomes divided into ovary, style, and stigma. This will enable us to obtain a correct idea of these parts, for which little has hitherto been done, as organs whose use and function, completely different, have received the same name.

The ovarium then is that portion of the leaf which incloses the *ovula*; the style, that portion which is rolled up and does not develop *ovula*, whose object is to conduct the prolongation of the pollen tubes; and lastly, the stigma is the free termination of the superior part, whose object is to receive and hold the pollen.

This result is attended again with manifold consequences. We find in the nomenclature of organs, for instance, that entire families to which styles had been ascribed, as the grasses,

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gation of living nature as of the "physiological principles" paraded in the title; and the author shows himself to be at least thirty years behind the most common-place botanical works of the present day, and even not *au niveau* with such men as Grew and Malpighi.

* If the ovarium be viewed in a direction from the axis.

possess *stigmata sessilia* only. Some few species belonging to these families, as *Lygeum* and *Zea*, possess an actual style. It has always appeared singular to me that the same botanists who, on the one hand, have advanced the position that the styles offer the surest means of determining the number of the carpels, because every carpel has its corresponding style, should, on the other hand, have ascribed only one carpel to the grasses, although they at the same time speak of several styles. A true style occurs equally seldom in the majority of the family *Euphorbiaceæ*; indeed in *Euphorbia*, *Ricinus*, *Andrachne*, *Crozophora*, &c., in which more than one style has been described, there is either none at all, but merely *stigmata sessilia bifida*, or only one style present, as for instance in *Euphorbia*, in which three carpellary leaves are united superiorly so as to form a tube, although a short one. We find the style also to be deficient in most of the *Alismaceæ*, *Malvaceæ*, *Phytolaceæ*; they possess only stigmata: in some of these plants, for instance *Ricinus* and *Phytolacca*, the so-called surface of the stigma sinks down with its papillæ as far as the basis of the carpel-leaves. It is equally incorrect to speak of *rami styli* in the *Compositæ*, which are in fact only forms of the double-lobed stigma. Hitherto little more than a traditional meaning has been applied to the words style and stigma, and this has been still more corrupted by means of pretended logical distinctions. It will, however, be easily seen that if botany is to be treated in a really scientific manner, the terms must be based upon ideas, which being derived from the nature of the vegetable structure, imply certain actual organic differences, and can be adopted in a sense so strict as to avoid the possibility of including the most various things under the same term; or on the other hand of separating identical organs by giving them different terms. The prosecution of the inquiry into the process of development also very simply settles the old dispute as to whether the style possesses a canal or not. Since each style is formed either by the rolling together of a single leaf (apocarpous fruit, Lindl.) or through the union of the edges of many leaves (syncarpous fruit, Lindl.), it must always possess a canal, which certainly cannot be always recognised as a cavity upon making a section of the style in the open flower, since the internal layer of cellular tissue (*Tissu conducteur,* Brongniart, properly speaking the epidermis of the upper surface of the leaf,) becomes so expanded through alteration in the form of the cells and the exudation of mucus in the intercellular spaces, that the individual cells become completely detached from their connection, and lie loosely imbedded in the mucus, as for instance in the *Orchideæ*, many *Liliaceæ*, &c.

These are then the important points from which nature does not deviate in the vegetable organization, whilst she manifests the greatest possible variety in regard to the external differences of form. The form of the stigma exhibits the most wonderful variations of shape, and upon this account has been of all parts that most frequently misunderstood. The style also, and even the carpel-leaf, offer many varieties, especially the latter, from the formation of spurious septa by cellular excrescences, as in the *Aroideæ*. We find further that the carpel leaf in the *Coniferæ* is not closed; in the *Resedaceæ* three are united to form one open basin, strictly closed in most families, frequently bent inwards towards the axis, and then turned backwards again, so that the placental portion forms a belly, and the style *appears* to spring from the base, where the transitions of development may be studied, beginning with the *Euphorbiaceæ*, through the *Phytolaceæ*, *Alismaceæ* as far as the *Borragineæ*, and *Labiatæ*, and lastly in the entire family of the *Dryadeæ*. The young ovarium in the *Labiatæ* and *Borragineæ*, for instance, is usually a two-leaved carpel (fig. 2), whose edges however are very soon joined to form the style; and by the development of the ovulum the part inclosing it becomes expanded both above and below, whilst the style, the upper end of the leaf, is incapable of this elevation and distention. The fruit of the palm presents a very similar appearance, in which the embryo stands at first erect soon after impregnation has taken place; as the seed however advances to maturity, the inner side of the ovarium does not enlarge with it: thus the point of the embryo becomes fixed and serves as a central point, about which the *radicula* describes a quadrant, to which figure it is limited by its partial development: in this manner is formed the *embryo horizontalis lateralis*. There has been a great deal of time lost in the discussion of such apparent abnormalities, which would not have been the case had surmise not taken the precedence of investigation.

If we now turn to the *placenta* and *ovulum*, and, which will be the most advantageous course to pursue, commence with the simplest form, we shall select that where no carpellary leaf is present, which is without doubt the one of all others that presents the most insurmountable difficulties if attempted to be explained according to the usual theory. This state is found in *Taxus*, for instance, where the entire female flower is nothing else than the terminal *leaf-bud* of the axis to which it belongs. The leaves are arranged in the customary spiral direction, even to the extreme summit, and no one leaf implies in the slightest degree an adaptation to the female part more than another (fig. 1). The axis, as is customary, terminates here also in a small protuberance (the *punctum vegetationis* of Wolff), and this is the *nucleus* of the ovule. Thus the axis forms the second antagonistic power (*differenz*) of the plant, forming as it does the female portion, and we are now in a condition to perceive that impregnation and fructification consist in the conjunction and balancing of the two most important antagonistic forces the plant possesses, viz., those of the horizontal and vertical structure.

We will however calmly prosecute the course of our investigation further. The terminal portion of the axis constitutes therefore the *nucleus* of the ovule, and is the *only actual* and never-failing portion of the entire female organ, whilst all other parts are or may be partially deficient; some in this plant, some in that. Now this end of the axis is frequently found bent, so that its point becomes reflected upon itself (*ovulum anatropum*), and adheres to that portion which retains its proper direction (*raphe*), a process which may be easily recognised on inspection. In this condition (*ovulum ex nucleo nudo constans*) we find the ovule in many families, as for instance in the *Santalaceæ*, *Rubiaceæ*, *Dipsaceæ*, *Cuscuteæ*, *Asclepiadeæ*, &c.*

There is indeed no reason why the nucleus may not be developed without suffering this reflexion of its axis, (as *ovulum atropum ex nucleo nudo constans*,) although I have never yet met with an instance of the kind.

The formative power concentrates itself in such a manner around this extreme point of vegetation that what subsequently appears as separate lateral organs is here consolidated in the shape of a sheath-like envelope. These leaves inclosing the stalk of the last bud are termed *ovular membranes*, and are distinguished by the total absence of spiroidal vessels, which are proper to the *raphe*, or that portion of the ovulum which is not divided into nucleus and integument; the presence of these vessels therefore would show that the membrane under examination was only an apparent ovular membrane. Still it sometimes happens that at a much later period—after fructification has taken place—vascular bundles may be detected in the actual integuments; this is, however, very rare indeed.

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* R. Brown includes the *Apocyneæ* also under this head; but they have a simple integument. In these, as well as in the *Asclepiadeæ*, it is not the nucleus which becomes developed in the interior after impregnation, but the sac of the embryo, which becomes at an early period filled with opaque albumen, which is visible after impregnation as a dark kernel perceptible through the integument.

Now such a simple envelope (*integumentum simplex* mihi)* is found under these circumstances:

1. Without the axis being bent (*ovulum atropum cum integumento simplici*) in *Taxus* at the flowering time, in the *Cupressineæ*, *Juglandeæ*, and *Ceratophylleæ*.

2. Or else the axis suffers the reflexion above described, whereby the envelope becomes adherent to the prolonged axis (*raphe*); (*ovulum anatropum cum integumento simplici*). To this class belong the *Abietineæ*, *Synanthereæ*, *Lobeliaceæ*, *Campanulaceæ*, *Goodenovieæ*, *Lentibulariæ*, *Scrophularineæ*, *Orobancheæ*, *Gesnerieæ*, *Sesameæ*, *Labiatæ*, *Bignoniaceæ*, *Polemoniaceæ*, *Convolvulaceæ*, *Solaneæ*, *Borragineæ*, *Gentianeæ*, (including the *Menyantheæ*, which have likewise only one integument; for the external hard covering, separable from the ripe seed, is nothing more than the epidermis of the integument, whose cells have become much lignified); further the *Apocyneæ*, *Umbelliferæ*, *Ranunculaceæ*, *Loaseæ*, &c.

Lastly, there is a second covering formed, which incloses the point of the axis (*integumentum externum et internum* mihi), and here also both modifications may occur.

1. The axis remains straight, as for instance in the *Polygoneæ* (fig. 4), *Cistineæ*, *Urticeæ*, and a portion of the *Aroideæ*.

2. Or else the axis becomes bent upon itself, adhering to the external integument (figs. 20 to 23). In the remainder of the family of the *Aroideæ* may be observed all possible states of transition, from an elongated axis, the reflected portion of which, with its integuments, hangs free (as is also the case in *Rafflesia* according to R. Brown), to the complete adherence, in which case the unreflected portion of the axis appears as *raphe*. Further, we must include amongst these *all* remaining monocotyledonous plants. R. Brown has not indeed expressed himself in positive terms on this point with respect to the *Orchideæ*; they possess, however, decidedly both integuments, which are only to be observed in their earliest stages (fig. 5), since the embryo sac having been very early developed has at the time of impregnation almost entirely compressed the *nucleus*, so that one would be induced to consider the very thin integument as the *membrana nuclei*. I will content myself with adducing the following among the dicotyledons as examples, to avoid occupying too much room with the mere enu-

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* I feel myself obliged to abandon the usual terms *testa* and *membrana interna* and others which are taken from the ripened seed, and are nowhere applicable, and which would only tend to confuse ideas of things on account of the many errors historically attached to them.

meration of names, *Nymphæaceæ* and *Cabombeæ*, the *Plumbagineæ*, *Resedaceæ*, *Passifloræ*, *Caryophylleæ*, and *Cruciferæ*.

Mirbel was the first who published any detailed account of the general formation of these integuments of the *nucleus*; but although he has partially observed the phænomena accompanying their formation, he has evidently been far from understanding them, and could not therefore clearly explain the process; and it is indeed scarcely possible to collect from his own words what was his real opinion of the matter. We are indebted to R. Brown, who has struck out so many new paths in this and every other department of botany, for the first correct account of their mode of formation in the *Orchideæ* in 1831, and at a later date (in 1834) in his dissertation on the female flowers of the *Rafflesia**, in which he extended his observations to many other families. Fritsche has however furnished the most detailed account of this subject in Wiegmann's *Archiv*, but he has confined his observations entirely to one species, and that the least favourable to such an investigation on account of its compressed growth and anatropous ovule. He has also neglected to take accurate micrometrical measurements, a point of the utmost importance here, by which alone he would have been able to avoid some errors. Thus, for instance, the expansion of a cylinder underneath a given line, and its contraction above it, are circumstances which can only be ascertained in objects so minute by means of comparative measurements, since, of course, every stage of the process cannot undergo examination at the same time, and these differences are of the greatest importance to the true appreciation of the subject. In this manner Fritsche has fallen into the error, on the one hand, of supposing that both integuments are a simultaneous formation produced by the inflexion of the first fold into the body of the ovule, and on the other hand he has viewed the formation of the inner integument in too confined a manner, as a mere fold of the *epidermis nuclei*.

The plan which nature adopts is simply this:—The example I shall select is that of the atropous ovule, for instance of the *Polygoneæ* (fig. 4), as being the most simple. At a certain distance below the apex of the original protuberance an ideal line may be recognised, intended as the basis of the *nucleus* (fig. 4 *b.*), which does not afterwards increase in thickness. Above this line the apex forms itself into the *nucleus*, and below it the substance of the axis expands and forms a protuberance (fig. 4 *b.*), which extending itself as a kind of mem-

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[* Account of the results of Mr. Brown's researches on these subjects will be found in Phil. Mag. and Annals, N. S. vol. x. p. 437, and Lond. and Edinb. Phil. Mag., vol. v. p. 70.—EDIT.]

branous fold gradually covers in the *nucleus*. (*Integumentum primum aut internum* mihi; *Secondine* Mirb.; *membrana interna* auct.) Sometimes soon after, and indeed almost contemporaneously with this, sometimes later*; sometimes immediately below the first protuberance, at other times at some distance from it (as for instance in many *Polygoneæ* and *Cistineæ*), we may next observe a second protuberance, which, as the second integument†, covers in the first. (*Integumentum secundum sive externum* mihi; *Primine* Mirb.; *Testa* auct.) The first-formed integument certainly does frequently consist only of a fold of the *epidermis* of the *nucleus*; nevertheless we do find a tolerably thick parenchyma taking part in its formation in almost all those families which form no second integument, and also in some which possess both coverings, as, for instance, in the *Euphorbiaceæ*, *Cistineæ*, and *Thymeleæ*. In the case of these three families, a peculiar process takes place, namely, upon the seed becoming ripe the external integument is gradually absorbed, until nothing but a thin membrane is left, usually described as *epidermis testæ*, or in the *Euphorbiaceæ* it has been given as *arillus*; and on the other hand, the actual modified *epidermis testæ* has also been described as the *arillus*, for instance, in the *Oxalideæ*. The apex of the original papilla, which develops itself as *nucleus*, varies exceedingly in its size in proportion to the entire ovule, if examined in the different families. It often forms a long and nearly cylindrical body, as in *Loasa* and *Pedicularis*; in many cases it is shorter, so that that portion of the ovule in which no distinction has taken place between nucleus and integument (the whole being like a fleshy distended stalk), is by far the more predominant, as in all the *Synanthereæ*, *Canna*, *Phlox*, *Polemonium*: it consists again, in some instances, merely of the extreme point of the papilla itself, as in *Convolvulus*; or nothing more than an ideal point remains, which can no longer be distinguished as an independent body, above which, however, a protuberance develops itself, and thus forms a micropyle, as in the *Dipsaceæ*.

Of course the process I have been describing becomes considerably modified in individual points, either through the unilateral development of the ovule(*ovulum campylotropum* Mirb.), or through the reflexion above described (*ovulum anatropum*). I should however be far exceeding the limits assigned me were I to insert here a detailed account of the numberless individual peculiarities which I have met with in the course of my observations. I will content myself with remarking that the *Quartine* of Mirbel does not exist: what he describes is nothing else than a temporary *endosperm* in those families, in which the embryo sac displaces the entire *nucleus* at an early period, although it is not destined to form *albumen* at a later period by means of a permanent endosperm.

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* This is most conspicuous in *Taxus*, in which the second integument (fig. 1 *b*.) does not exist until after impregnation has taken place (*cupula* auct).

† I observed this to occur very distinctly in *Hydrocharis* and *Vallisneria*; and, as Richard's analysis shows, all other true *Hydrocharideæ* have atropous ovules. Endlicher's attribution of an anatropous ovule to this family (*Genera Plantarum*, p. 160) is probably derived from a partial investigation of *Stratiotes* (which perhaps does not belong here), and which has been extended to the remainder of this family.

These integuments experience manifold changes during the ripening of the seed, so that the original number can seldom or never be recognised in the ripe seed. Sometimes all the integuments become consolidated so as to form but one; at other times, and this is more frequently the case, the integuments become separated into different layers of cellular tissue, of various degrees of development, in which case the homogeneous tissue can easily be separated from the heterogeneous. In this manner the integument of the ripe seed may sometimes be divided into as many as five layers, although only one or two membranes, or, as in *Canna*, no complete integuments were originally present. But since it frequently happens that the greatest variety may occur in the ripe seed in this particular in one and the same family*, as has been already related of the group *Menyantheæ*; whilst, on the other hand, the entire absence, or the presence of one or two integuments in the ovule appears to be very constant in the different families and groups, it may possibly be more advantageous to return entirely to the old terminology of Richard, and only speak of an *episperm* in the ripe seed, the different positions of which may then be more minutely characterized, whilst at the same time greater accuracy may be observed in the description of the ovule. Many interesting results may probably be ascertained when these investigations shall have been extended over all the families; already the small circle of my own observations has afforded many hints. It is remarkable, for instance, that *not a single* monocotyledonous family possesses fewer than two integuments, and the first impression caused by a review of the different families given above is, that amongst the dicotyledonous the majority of the monopetalous families is furnished with but one integument, whilst the polypetalous generally possess two.

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* Indeed in the same genus. Thus one portion of the *Salviæ* possesses spiral cells in the epidermis of the integument of the seed, and in the remainder they are absent.

[To be continued.]

XXVII. *On Electro-magnetic Motive Machines. By Mr.* Francis Watkins.

[With a Plate.]

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

PERMIT me in the pages of your next number to describe two or three modifications I have made in electro-magnetic motive machines, for I am inclined to think that any new arrangement of the working parts will interest those of your readers who hope to see realised the expectations which have been so confidently held out of successfully employing electro-magnetic power for propelling machinery.

We are indebted to Professor Joseph Henry, New Jersey College, Princeton, for the first hint, and for the first contrivance wherein electro-magnetic power is made to produce continuous motion.

In one of Silliman's American Journals for 1831 will be found Professor Henry's original description of his electro-magnetic motive machine, and as I believe it is not very generally known, I venture upon a brief description of it in this place. It consists of an electro-magnetic beam, supported horizontally on an axis passing through its centre of gravity, with two permanent steel bar magnets arranged vertically, one under each pole, of the horizontal electro-magnet, with their north poles uppermost. Without entering into the details of the method of changing the polarity of the mobile horizontal electro-magnet, the working of the machine will, I conceive, be sufficiently understood when it is stated, that by a timely alteration of the magnetic polarity (which is effected by changing the direction of the electric current inducing the polarity) of the horizontal electro-magnetic beam, an alternate series of attractions takes place between its poles and the poles of the vertical permanent steel magnets, and thus a reciprocating rectilinear motion is obtained by the vibration of the horizontal electro-magnetic beam.

Since Professor Henry's publication several modified forms of apparatus have been produced for exhibiting the electro-magnetic power. In England, as far as my knowledge extends, little as yet has been achieved beyond the construction of some very ingenious trifling machines or toys for exhibiting continued rotatory motion by this agent.

Several philosophers on the Continent have published accounts of experiments, and of machines made by them, their object being the practical application of electro-magnetic attractive force, and one of them supposed he obtained a power equal to that of half a man. I refer your readers for an account of their labours to Part IV. of Mr. Taylor's new and highly useful quarterly publication, entitled "Scientific Memoirs." The public have also been recently favoured with notices of a small electro-magnetic machine, said to perform wonders, on the other side of the Atlantic; but in spite of the talent already brought into play, it must be acknowledged that it is yet reserved for some happy genius to hit upon the right arrangement which shall *economically* employ this electro-magnetic power *on a sufficiently large scale* to be used with the desired success.

In the August number of your valuable Journal for 1835 (Lond. and Edinb. Phil. Mag. vol. vii., p. 107,) you favoured me by noticing an arrangement of the electro-magnetic motive machine which I had then contrived. This machine, like all others that I am acquainted with, was founded upon the fundamental principle of Professor Henry's, namely, that of varying at a particular period the polarity of the transient electro-magnet, thus obtaining a succession of magnetic attractions. It is true that Henry only obtained an alternate rectilinear motion, while his followers succeeded in producing continual rotatory motion; still the principle remained the same.

Henry's principle being universally adopted, it is clear that all those who attempted to carry out the idea of obtaining a motive power by this principle are only entitled to the credit of those modifications of the arrangement which may emanate from their mechanical ingenuity, and to such credit only do I aspire in the present communication on my machines, which I will now describe.

Fig. 1. (Plate IV.) is a representation of a working model of an electro-magnetic motive machine; *a*, *a*, *a*, *a*, are four vertical cylindrical permanent steel magnets suspended by two mahogany cross stages *b*, *b*, which are themselves upheld by two mahogany columns *c*, *c*. The electro-magnets *d*, *d*, and *d′*, *d′*, are arranged horizontally, and are attached to and supported by a metal vertical shaft very free to move. Hollow magnets would be lighter, and perhaps answer as well. *e*, *e*, are two wooden cisterns, each with two concentric troughs, and each trough divided into four parts. In the bottom of each of the partitions in the top trough, a short metal wire enters; all these short wires are properly connected with *f*, *f*, which are two main wires descending and connecting the battery current with the mercury in the partitions. A similar disposition of short wires is made with the partitions of the troughs in the lower cistern, and then connected with the mercury cups *g*, *g*, which answer for top and bottom cisterns. The ends of the copper wire coils of each pole are judiciously joined by solder to two pendent platinum wires, and form the ends of each system of the electro-magnets, as shown in the plate. Now when the points of the pendent platinum wires impinge upon the surfaces of the mercury in two partitions placed side by side, a current passes along the wire coils, and gives a polarity of a certain kind to all the four poles of the soft iron magnets, and those of an opposite kind to the adjacent poles of the fixed permanent magnets are attracted by the latter; but after they have arrived opposite the fixed magnets, they pass a barrier, and a change takes place in the direction of the current by the pendent wires again impinging on two surfaces of mercury: that in the inner surface is now in connexion with a different electrode to what it was before, it being in the same condition as the outer one was in the first instance; and, *vice versâ*, the outer partition is in connection with the electrode which in the first instance was in the same condition as the inner. The order of the polarity in the soft iron is influenced by the direction of the electric current pervading the system of coils. It is therefore easy to conceive how the polarity of the soft iron may be reversed when the direction of the current is changed, provided, as elsewhere has been observed, this reversal is within certain limits of time.

This alternate attraction produces the revolution of the electro-magnets. The lower system of electro-magnets is at an angle as regards the upper system; so that when one system is in a position in which it operates most powerfully, the other system is at the dead point or nearly so; by this arrangement the systems assist each other over that difficulty. On the metal vertical shaft is a bevel toothed wheel *h*, which gears into another bevel wheel on a horizontal shaft *i*, which is carried away and made to produce motion, and work at a distance small pieces of machinery, such as models of tilt hammers, pumps, dredging machines, &c. A plain pulley may be affixed to the vertical shaft, and with a long band made to operate; by this means the friction of the bevel gear is saved.

k, *k*, *k*, a mahogany base for the whole instrument.

Fig. 2. This is another form of model of an electro-magnetic motive machine: *a*, *a*, *a*, *a*, a mahogany board; *b*, *b*, *b′*, *b′*, two soft iron electro-magnets with their bent parts underneath the board; *c*, *c*, *c′*, *c′*, and *d*, *d*, *d′*, *d′*, are four flat bent permanent steel magnets, arranged like the arms of a windmill, and attached to a horizontal moveable axis, which is supported on a hollow wooden column *e*.

The arrangement of the axis could not be shown in the figure, but it has a contrivance of points dipping successively into the mercury in a divided wooden cup. One partition of the cup is connected with one element of the battery, while the other partition is in communication with the other element of the battery. The communicating wires from the division in the cup proceed down the hollow wooden column to the coil of wires surrounding the soft iron, while the battery wires proceed up the column, but not in contact, and finish by being connected to two separate cups with mercury, in which revolve two small circular discs of platinum affixed to the axis, one on each side of the divided cup, and supported by two wood arms from the top of the column. The dipping points are so insulated and arranged on the axis that they reverse the direction of the current in the wire coils, and so effect the order of polarity in the soft iron magnet: this being accomplished at the right period the motion of the permanent steel magnets is obtained.

Two pullies are attached to the axis, bands from which will urge trifling pieces of machinery.

I have constructed a more simple form of motive machine than that of fig. 2. Plate IV. It has only one electro-magnet and two permanent steel magnets. The axis or shaft is of the same description as that with the four magnets: it works remarkably well, and the axis having only to carry one system of magnetic arms revolves with great velocity, and raises nearly an equal *weight* in the same time and distance as that with the four magnets.

The batteries I employ for obtaining the electrical currents are small, and constructed upon the plan proposed by Professor Daniells, King's College, London, and called by him the constant battery. This plan of the battery I consider to be original and the best devised for constant action and convenience of manipulation.

When we reflect that magnetic attractive force is the fundamental principle upon which the motive machines act, the limited space through which this force operates to a working amount, and our imperfect means of developing its powers, it may be excusable if we pause before giving in the present state of our knowledge an unreserved assent to the ultimate success of employing its agency as a prime mover on an extensive scale.

I have mentioned that many trifling machines or philosophical toys, in addition to those I have just now described, have been constructed, and plenty more, I have no doubt, will be brought forward and work very successfully; and when they operate by continued rotatory motion of the shaft, carrying delicately suspended mobile magnets, the shaft and mobile magnets may be made to revolve with considerable velocity.

Many hundred revolutions in a minute have been assumed as the rate of speed of some of the already constructed revolving shafts and mobile magnets; but is it imagined that magnetic attractive force alone actuates the machine so many times within that period, and causes the great velocity? for such a condition of things, I respectfully submit, cannot exist. We certainly have mechanical arrangements which enable us to alter many hundred times in a minute the direction of the inducing electric current about the soft iron; but the polarity of the soft iron cannot be changed so rapidly if Herschel and Babbage's law be just, for they say "*time is an essential element of induction**," and it is by the induction of the electricity in the wire coils embracing the soft iron that the magnetism in the soft iron is induced, and thereby an attractive force gained. I am not aware that it has yet been determined what is the exact time necessary for the full development of the inductive process, yet experiments tend to prove that it is within the limit of many hundred times in a minute; and as it is necessary for the constant employment of the magnetic attractive force in its full effects that the polarity of each of the opposing poles of the fixed magnets should be in opposite states to the advancing poles of the mobile magnets, it is clear that if the *time* has not transpired necessary for the transient magnet to acquire its full and proper polarity, there will not be the whole effective magnetic attractive force gained.

What is it then that aids the rapid revolution of the mobile magnets and carrying shaft when they are light and very free to move, unless it be, their own inertia, when they have acquired a certain velocity, kept up, and contributed to at intervals in the revolution by the original prime mover, viz. magnetic attractive force? Now this inertia is a power that is soon overcome by additional friction, as may be observed when a very small portion of weight is added to a shaft which without the additional weight revolves rapidly. The diminution of velocity is immediately perceptible; and supposing that the slight extra weight thus counteracts the advantage gained by the inertia, then under such circumstances we have only the primitive magnetic attractive force of the machine left for mechanical purposes.

I find these conditions maintained in my small models, and also in one on a much larger scale which I have made; for on augmenting the size of the machine you augment the size of the revolving shaft and its magnets; friction increases conse-

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[* See Phil. Mag., First Series, vol. lxvi. p. 98.—Edit.]

quently in a very rapid proportion, while the space gained through which the magnetic attractive force operates is increased comparatively in a very small degree.

It is truly surprising how limited is the ratio of improvement in the power of a machine by enlarging its magnets, as we then unfortunately increase the weight of the moving parts. I have noticed before, that the model with one pair of steel magnets, with four arms, and one electro-magnet, raised a weight through a certain space in a given time. Now when the model with two pair of steel magnets with four arms and two electro-magnets was experimented with, it was found that the latter was not so much more powerful than the former as we might have been led to expect.

It should be remembered that the means employed to change the direction of the current and the weight of the axis or shaft in both cases were exactly alike; therefore I conceive it was the extra friction of the axis caused by the extra weight of the additional pair of steel magnets that decreased the inertia of motion, and thus prevented the available power increasing to the amount anticipated.

I have not remarked upon the resistance of the air to the revolving arms, for that must be a retarding action in all cases of revolution. Besides the arms cut the air edgewise, therefore they are under the most favourable circumstances as regards that point.

It has been suggested as the means of gaining more power o multiply the number of fixed and moveable magnets, and so contrive that the forces should conspire to produce their sum at the working point, and we may infer that an advantage to a certain degree may be gained by a skilful arrangement; but if my views are correct, the power gained could be obtained on a large scale from other sources more œconomically.

I am well aware it frequently occurs in the application of a philosophical principle or a mechanical arrangement that there is a considerable difference between a model and that of a large working machine; it therefore behoves all persons experimentally engaged in the application of a principle or a power to bear this in mind, and not to decide too hastily because they fail several times with models. And I am also aware that my arrangement is faulty, and not the most judicious that could be contrived, although one of them is very simple; yet it does appear from the nature of the force we employ, and its small distance of working action, that we must look forward and hope for a better knowledge of the nature of the mysterious and invisible agent which is to actuate our machines before complete success crown our endeavours.

Human perseverance has achieved wonders; and as the subject engrosses considerable attention just now, and we rejoice to find by the periodicals that the Emperor of Russia has placed at the disposal of M. Jacobi and a scientific committee 500*l.* for the purpose of making experiments, we may indulge in the hope that before long some successful results will be the fruits of their labours, and that a new method of employing magnetism will be discovered; for from the present mode, if my notions be correct, we have little to hope for on a large scale, and playthings are not worth the mechanician's notice. I remain, Gentlemen, yours, &c.,

FRANCIS WATKINS.

XXVIII. *Physical, Chemical, and Physiological Researches relative to the Torpedo; and some Remarks on the Contractions of the Frog.* *By* C. H. MATTEUCCI (*read before the French Academy by* M. Becquerel)*.

M. MATTEUCCI has presented to the Academy a Memoir on the electrical phænomena of the torpedo, as also several notes relative to the contractions produced in the frog by the contact of the muscles with the nerves. These having been submitted to the examination of a committee composed of MM. Breschet and Pouillet and of myself, we have the honour of laying before you an account of these various researches.

The sensation which the torpedo causes when it is touched has long ago attracted the attention of physicists and physiologists, on account of its analogy with that produced by an electrical battery, but it is only a few years since that it has been decidedly proved that both were owing to the same cause. Although all the principal circumstances of this phænomenon had previously been carefully studied, yet no one had succeeded in demonstrating its electrical origin from the want of suitable apparatus.

John Davy made known in a paper published in 1832 a great number of important data, such as the action of the discharge upon the magnet needle, and the chemical compounds; but the direction of the electrical current produced on this occasion was not well known until after the experiments made at Venice, 1835, by two of your members, and from which it resulted that the superior part of the electrical organ gives positive electricity, and the inferior part negative electricity. Matteucci has confirmed with the galvanometer and frogs prepared after the method of Galvani the observations which we had made respecting this point, as well as others also relating to the torpedo, for which we are indebted to various philosophers; at the same time he has demonstrated some new facts, of which the following is a short account.

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* Translated by Mr. Francis from the *Comptes Rendus*, No. 23, Dec. 1837.

He commences by showing that when the torpedo lances its discharge no change of volume is observed in its body. When the animal is possessed of great liveliness the sensation is felt at whatever point of the body it may be touched, but when its vitality is considerably diminished the discharge is no longer felt, except by touching the electrical organs at two different points.

Matteucci establishes the general laws of the distribution of electricity in this manner:—1. All the points of the dorsal part of the organ are positive relatively to the points of the ventral part; a fact already known. 2. The points of the organ on the dorsal surface placed above the nerves which enter it are positive in respect to the other points of the same dorsal surface. 3. The points of the organ situated on the ventral surface corresponding to the points which are positive on the dorsal surface are negative in respect to the other points of the ventral surface. 4. The intensity of the current varies with the extent of the platina plates which terminate the galvanometer, and with which the two surfaces of the organ are touched.

When the torpedo is very excitable the current may be compared to that of a pile consisting of a great number of pairs charged with a good conducting active liquid; whilst, on the other hand, when its liveliness is weak, the electric current resembles that of a pile composed of a small number of elements.

The spark which accompanies the discharge in the electrical fishes was remarked for the first time by Walsch in the *Gymnotus*; many vain efforts have been made since to reproduce it; MM. Matteucci and Linari have succeeded in obtaining it in every case from the torpedo; both these philosophers claim the priority of the observation. It appears from the notices which we have gathered that Matteucci was the first who had the idea of employing for this purpose Faraday's apparatus of the extra current, which M. Linari did not make use of until after it had been pointed out to him by his countryman.

Matteucci has since succeeded in obtaining the spark by placing the torpedo upon an isolated plate of metal, and placing another plate of metal above it, then fixing to each of them a gold leaf separated the one from the other by the distance of half a millimetre. By slightly moving the upper metallic plate the animal became irritated, and at the same moment the two leaves approached one another and the report of the spark was instantly heard.

Matteucci has carefully studied the internal and external causes which influence the discharge of the torpedo; among the external causes we may distinguish, besides the mechanical excitement, heat, in water at 18° Reau. The torpedo seldom lives more than five or six hours, preserving all its electrical power; on diminishing the temperature this power instantly ceases. On heating the water the discharges begin afresh, but if the temperature is increased to + 30° Reau., as we ourselves also observed, the animal after several discharges suffers violent contractions and dies in a sort of tetanic state.

Matteucci having analysed the air contained in sea-water, has determined the variations which result from this with respect to the respiration of the torpedo. According to the observations which he has made relative to this point, when the torpedo is tormented it respires more than that which is not; and what is most singular, if the fact be true, is that the former produces under the same circumstances less carbonic acid than the other: it would seem in general that the intensity of the electric function is in proportion to the force of the circulation and of the respiration.

The action of the most energetic poisons produce the following effects:—Hydro-chlorate of strychnia introduced into the mouth and stomach of a torpedo produced almost immediately violent contractions in the vertebral column accompanied with powerful discharges, afterwards of weaker discharges, and the animal expires in violent convulsions. Hydro-chlorate of morphine produces in eight or ten minutes after its introduction into the animal very powerful discharges; it sometimes gives more than sixty discharges in ten minutes.

The current of an electrical apparatus composed of eight pairs directed from the mouth to the branchiæ, and to the epidermis of the interior of the organ, produces strong discharges. Electricity acts in this case probably only as a violent excitative.

Matteucci having cut the half of the organ either in a horizontal or in a vertical direction, and having placed between the divided parts a plate of glass, the discharge still took place; this was the case also even when the organ held to the animal only by a nervous fibre: the effects did not cease before the substance of the organ became coagulated by the action of the acids or of the boiling water. We may remark with respect to this, that several philosophers, and especially Galvani, have made similar experiments; they found, for instance, that if the four nerves of one of the organs are severed, the discharge immediately ceases in this organ, while it manifests itself continually in the other; and that if only two or three nerves are cut, the sensation is limited to points corresponding to the nerves which have remained intact: they concluded from their observations that the brain and the nervous trunks exercise an influence which determines the electrical faculty of the torpedo. Matteucci has arrived at the same conclusions, but he has determined better than any one had done before him the extent of this influence, as will be seen. If we tie the nerves, the same effects are produced as on cutting them. When the nerves have been severed, if one of the nervous trunks which branches in the organ is drawn forth with pincers, we still obtain some discharges. If the brain is laid open, and certain parts be irritated with any body whatsoever, the discharge is instantly evident. The first lobes (the cerebral) may be irritated, severed, and even destroyed without the discharge disappearing; the same is the case with the third lobe. As to the fourth, it may be touched without producing powerful discharges; on destroying it, even when the others are left whole, the electrical power of the animal is entirely destroyed. This observation, which is very remarkable, will certainly be of great interest to physiologists on account of its singularity.

When the animal is in such a state of torpidity as not to give any more discharges, when the ordinary excitatives are employed, if we then lay open the brain and touch the electrical lobe, the discharges appear with force going indifferently from the back to the belly, and from the belly to the back, whilst no effect is produced on irritating the other parts of the brain; if we employ electricity as the excitative we obtain a similar result.

We think it our duty to mention in this place that M. Flourens had already proved by direct experiments, published in 1825, that the last lobe of the brain is in fish in general the special encephalic organ of respiration. If one side of this lobe is cut away, the movement of the operculum of this side is immediately destroyed; the movement of the operculum of the opposite side continues. If the lobe be entirely taken away, the play of the two opercula ceases suddenly. Flourens moreover proved that the action of the last lobe (of the lobe situated behind the cerebellum of the brain) upon the opercula continues complete after the taking away of all the other parts of the encephalus, as after the taking away of the spinal marrow, whether these two removals (that of all the other parts of the encephalus, and that of the spinal marrow) be made separately or simultaneously.

M. Matteucci having entirely separated from a great torpedo one of the electrical organs, without detaching the epidermis, one of the plates of the galvanometer was inserted in the organ near the outward edge, the other plate was put in communication with one of the four nerves: the needle deviated four degrees in the common direction of the discharge of the torpedo; on tying the nerves there was no longer any deviation; this result appears to us very remarkable.

The above observations, which we have not been able to confirm from the want of torpedos, go to prove, 1, that the electricity which produces the discharge proceeds from the last lobe of the brain, and is transmitted by the nerves to the organ; 2, that the discharge ceasing under the influence of the electric current, when the nerves are tied, must, in order to be transmitted, find in the nerve a particular molecular disposition; a conclusion to which the electro-physiological phænomena of the frog equally lead, as one of us (M. Becquerel) has indicated in various places in his treatise on electricity.

Since the ever-memorable epoch when Galvani demonstrated that the contact of two different metals in communication with the muscles and nerves of a frog sufficed to make it contract, the experiments have been varied infinitely in the hope to discover in this phænomenon the cause which constitutes life in animated bodies. The most remarkable fact, for which we are also indebted to Galvani, is that which relates to the contractions produced by the simple contact of the muscles and nerves without the intermediary of metallic armatures. It is now nearly demonstrated that this action does not proceed from a chemical action, but from the inherent current of the frog, which has been indicated with so much sagacity by M. Nobili.

On the other hand, Ritter and several other philosophers* have remarked that the irritability in those parts which are separated from the body of the frog does not at the same time cease in the entire course of the nerve; it begins by leaving off at those parts nearest to the brain and ends by those which are most distant. Müller moreover maintains that a nerve tied or compressed ceases to be a conductor of the agent which circulates in the nerves, whichever it be; it remains nevertheless a good conductor of electricity. Matteucci noticed a similar fact in the torpedo, as we have remarked;

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* Müller's Manual of Physiology, p. 603.

but it is chiefly on the frog that these observations afford with respect to this great interest. When the nerve is tied, a simple electro-chemical current passes through the ligature and ceases to cause the frog to contract much sooner than its own current ceases to act; the ligature does not in the least alter the conductibility of the current, however feeble it may be. In the living animal when the muscles are brought into contact with the nerves, the contractions are more feeble than those produced by the inherent current of the frog after death; the contractions grow weaker and discontinue when the parts have been well wiped; and if the animal remains still, the current generally ceases to take place.

If we place the frog between two pieces of glass for ten to twelve seconds, and then withdraw it, the inherent current no longer exists. On introducing oxygen into the mouth the animal instantly becomes agitated, jumps, and the peculiar current re-appears, and then vanishes, as Matteucci has observed in the torpedo.

When the thighs and crural nerves brought into contact no longer produce any contraction, if the nerves near to the spinal marrow are then cut, and if touched immediately with the thighs, contractions immediately take place. When all sign of the inherent current has disappeared, if we draw forth the sciatical nerve of the thigh and bend it on the muscles of the leg or of the other thigh, the thigh corresponding to the nerve touched will become contracted; this latter fact belongs to the law indicated by Ritter, which this physicist had remarked with the help of an electric current.

These observations go to prove, as has been admitted by several philosophers, that there exists an electric current continually circulating in the nerves and in the muscles of the living frog, by means of a complete arc, which can only be rendered perceptible by our apparatus when the animal is in a state of excessive excitation; whilst by preparing the frog after the manner of Galvani the integrity of the arc is destroyed and we easily recognise the inherent current.

The facts we have thus laid before the Academy, and of which many have been verified by us, throw some light on the electro-physiological phænomena of the torpedo and frog, and will not fail to interest the Academy. We therefore propose that the several communications of M. Matteucci be inserted in the *Recueil des Savans Etrangers**.

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* This was agreed to after a long discussion respecting the question of priority between Matteucci and Linari as to the production of the spark. See p. 197. Our readers are referred to an interesting notice connected with the subject of this paper in page 223.—W. F.

XXIX. *Notices respecting New Books.*

A Practical Treatise on Warming Buildings by Hot Water, with some remarks on Ventilation. By Charles Hood, Esq., F.R.A.S., illustrated by numerous Wood-cuts, 8vo. London, 1837.

This is a practical and scientific treatise on an important subject, which is every day attracting more and more public attention. The inquiry is pursued in a popular manner, in order to render it intelligible to all readers; all abstruse calculations and scientific technicalities have been avoided as much as possible. The merits of the various kinds of apparatus employed are carefully considered, their philosophical principles pointed out, and their utility displayed in a practical manner, and illustrated by well-executed diagrams. One of the most important chapters is that on ventilation, showing the deleterious effects upon the human frame of atmospheric air which has been changed by the process of respiration, or by subjection to the action of heat, or in any other way; and the necessity of sufficient and good ventilation. "It has been proved by experiments," says the author, p. 173, "that air which has been once inhaled loses about 10 per cent. of its oxygen, or nearly one half that it contains, and acquires from 8 to 8½ per cent. of carbonic acid gas. This gas, it is well known, is as destructive to animal life as the oxygen is necessary for its preservation, and, therefore, air cannot be breathed a second time without serious inconvenience. For, as it is found impossible to make atmospheric air contain more than 10 per cent. of carbonic acid gas, it follows, that, if breathing a quantity of air once impregnates it with 8½ per cent. of this gas, if it be breathed a second time, it can only receive 1½ per cent.; and therefore the remainder must be left in the lungs, where it exerts a most deleterious effect. The noxious qualities of this gas are well known; the foul air of wells, which causes death in so many instances, consists of this deleterious matter; and it is extraordinary that the heart and muscles of any animal that has been deprived of life by breathing it, entirely lose their irritability, and become insensible even to the powerful stimulus of galvanism." We have great pleasure in recommending this work to the attention of the public in general.

A Guide to an Arrangement of British Insects. By J. Curtis, Esq., F.L.S., Author of British Entomology, 2nd edit., 8vo. London, 1837.

In order to remedy the great inconvenience which students in Entomology must have experienced of not having a compact printed Catalogue of British Insects, Mr. Curtis has published this useful work, with a view to enable them to arrange their cabinets systematically; to mark off their own insects so as to know instantly whether they have a species or not, by which means their desiderata will be shown, and they will be enabled to enrich their cabinets by mutual exchanges; to form labels for cabinets, and save much time that would be required for writing them. It is also a systematic index to Mr. Curtis's British Entomology, a reference being given to every genus illustrated in that valuable and splendid work, which now contains more than 700 beautiful and accurate coloured representations of insects, and nearly as many of indigenous plants. The numerous additions that have been made to our British collections of late having been embodied in the present edition, it is far the most complete catalogue of the present day, comprising nearly 15,000 British species, and cannot fail to be highly serviceable to those for whose use it is designed.

Bibliographical Bulletin.

Poggendorff's Annalen, 1837, Nos. 8 and 9.

Contents.—On the nature of uric acid; by Liebig and Wöhler.—Jervin, a new vegetable basis; by E. Simon. (See for a translation of this paper pres. vol. p. 29.)—On œnanthic acid and œnanthic acid æther; by Liebig and Pelouze.—On the oil in liquors distilled from grain; by G. J. Mulder.—New preparation of chrome alum; by F. Marchand.—On the æther sulphates; by the same.—Detection of sulphuric acid in cases of medical jurisprudence; by J. Simon.—Occurrence of arsenical copper in Chili; by Zinkem.—Observations on the influence of crystalline surfaces on reflected light; by F. E. Neumann.—On Becquerel's simple series, whose current is said to originate from the combination of acids and alkalies; by F. Mohr. (We hope to be able to give a translation of this paper in our next number.)—On the production of electricity in chemical combinations; by P. Dulk.—Method of separating the oxides of cobalt and nickel, as also the protoxide of manganese, from the oxide of iron and from arsenic and arsenious acid; by Th. Scheerer.—On the simple and double Ajan metals; by Rammelsberg.—On a series of organic combinations which contain arsenic as a constituent; by Bunsen.—Paton, Marsh, and Simon's methods of detecting arsenic, with remarks by Berzelius.—Reduction of sulphuretted arsenic by means of silvered carbon; by F. Runge.—On the combination of azote with metals, for instance with copper when in a state of red heat; by Pfaff.

Neue Notizen von Froriep, vol. iii.

Contents.—No. 2. Magnets without cohesion.—Experiments on the process of respiration; by Th. Bischof.—No. 3. Further microscopical observations on the primitive fibres of the nervous system of the Vertebrata; by R. Remak.—No. 4. Natural classification of polypi.—No. 5. On the blood and lymph globules; by Prof. Mayer in Bonn.—No. 8. On some parasitical animals and organic products of the common rain worm.

Journal für praktische Chemie. By O. L. Erdman, Part 7—10.

Contents.—Description of two new minerals from Siberia; by Aug. Breithaupt.—On the alkaline reaction of various carbonate salts.—Analysis of Agalmatolite.—On the fabrication of straw paper; by Piette.—On the application of the pith of the mangel wurzel to the fabrication of paper.—Description of some new minerals; by Breithaupt.—Method of preparing Atropia and Atropic acid; by W. Richter.—Action of animal charcoal on salts of iron.

Flora. No. 25, etc., 1837.

Contents.—On the symmetry of plants; by H. Mohl.—On the generic characters of trees; by G. Liegel.—Plantæ quædam novæ vel minus cognitæ in Ægypto a cl. Acerbi, in Nubia a cel. Brocchi detectæ.—Martius, Herbarium Floræ Brasiliensis.

XXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xi. p. 196.]

Nov. 16, 1837. "DESCRIPTION of a new Barometer, recently fixed up in the Apartments of the Royal Society; with remarks on the mode hitherto pursued at various periods, and an account of that which is now adopted, for correcting the observed height of the mercury in the Society's Barometers." By Francis Baily, Esq., Vice-President and Treasurer, R.S.

The barometer, here alluded to, may in some measure be considered as two separate and independent barometers, inasmuch as it is formed of two distinct tubes dipping into one and the same cistern of mercury. One of these tubes is made of *flint* glass, and the other of *crown* glass, with a view to ascertain whether, at the end of any given period, the one may have had any greater chemical effect on the mercury than the other, and thus affected the results. A brass rod, to which the scale is attached, passes through the framework, between the two tubes, and is thus common to both: one end of which is furnished with a fine agate point, which, by means of a rack and pinion moving the whole rod, may be brought just to touch the surface of the mercury in the cistern, the slightest contact with which is immediately discernible; and the other end of which bears the usual scale of inches, tenths, &c.; and there is a separate vernier for each tube. A small thermometer, the bulb of which dips into the mercury in the cistern, is inserted at the bottom: and an eyepiece is also there fixed, so that the agate point can be viewed with more distinctness and accuracy. The whole instrument is made to turn round in azimuth, in order to verify the perpendicularity of the tubes and the scale.

It is evident that there are many advantages attending this mode of construction, which are not to be found in the barometers as usually formed for general use in this country. The absolute heights are more correctly and more satisfactorily determined; and the permanency of true action is more effectually noticed and secured. For, every part is under the inspection and control of the observer; and any derangement or imperfection in either of the tubes is immediately detected on comparison with the other. And, considering the care that has been taken in filling the tubes, and setting off the scale, it may justly be considered as a *standard barometer*. The present volume of the *Philosophical Transactions* will contain the first register of the observations that have been made with this instrument.

Mr. Baily then enters into a description of the several corrections that are required for the various kinds of barometers, in order to make them comparable with one another; and treats of each of these in their order. First as to the correction for temperature, both of the mercury and of the scale; next for capillarity; and afterwards for the height of the barometer above the level of the sea. A table is given for the first of these corrections; and a convenient formula for the latter: the correction for capillarity is constant, and of very small magnitude.

The author next describes the mode in which the observations of the barometer have, from time to time, been recorded in the Meteorological Journal of this Society; and points out several inaccuracies which have occasionally been committed in this department, for want of an uniform plan of reduction. Now this state of confusion and uncertainty he remarks ought not to exist in a meteorological journal emanating from this Society, more especially as the true values are as easily attainable as the approximate ones. And although, in a general point of view, the minute differences caused by such errors may be unimportant, yet as appeals are frequently made to the barometer of this Society, as a standard, by persons engaged in important researches, the most scrupulous accuracy ought to be adopted and pursued, and the fullest explanation placed on record. And Mr. Baily says that notwithstanding the details which he has given may create some doubt respecting the accuracy of the past, yet he is persuaded that the system now pursued will inspire more confidence for the future. It is on this account that he has entered thus at large on the subject; trusting that what he has stated will not only tend to preserve for the future a more correct and uniform system, but also justify the Council in directing that the register should henceforth contain the daily observations *uncorrected*, and thus prevent the possibility of any similar confusion and mistakes hereafter.

Mr. Baily then adverts to the height of the Society's barometer above the mean level of the sea; a subject of much interest to many persons engaged in various pursuits, but which appears, from the notes attached, at different periods, to the meteorological journal of this Society, to be involved in some confusion and uncertainty. Thus, prior to the year 1823, the cistern of the barometer is said to be 81 feet above the level of low-water spring tides at Somerset House; but without any information how this was connected with the sea. From 1823 to 1825, both inclusive, it is said to be 100 feet above the same level. And from 1826 to 1836, both inclusive, the above indication is omitted, and the height is said to be 83 feet 2½ inches above a *fixed mark* on Waterloo Bridge; or "above the mean level of the sea (presumed about) 95 feet." The discordance between the 81 feet and the 100 feet is easily accounted for by the fact that the old barometer, prior to 1823, was fixed up in the Council-room of the Society, or the contiguous ante-room: but when Mr. Daniell's barometer was finished, at the end of the year 1822, it was fixed up in the closet adjoining the library, on the floor which is immediately *over* the Council-room; the assumed difference in the elevation of the two floors (namely, 19 feet) having since been ascertained to be correct.

With respect to the new reference of altitude, namely, the fixed *mark* at Waterloo Bridge, much doubt has frequently been expressed about its existence, since no person had been able to discover it. The fact is that there is no *mark*, in the common acceptation of the term; but the intended reference is nevertheless more conspicuous, more durable, and more convenient than any mark that could have been inscribed by hands. This standard mark, or level, was fixed on by Mr. Bevan in the year 1827, at the request of the Council of this Society: and it is the surface of the granite pedestal at the base of the columns, at the north abutment of the bridge, and on the eastern side; which is about 5 feet above the lowest platform, or landing, at the stairs. Nothing therefore was wanting but the difference of level between this mark and the one made by Capt. Lloyd at London Bridge, the height of which above the mean level of the sea had been determined by him*. This has been recently done by Sir John Rennie, at the request also of the Council: and the result of the whole is, that the cistern of the barometer is 97 feet above the mean level of the sea.

The author concludes his paper with some remarks on the propriety of the position of the several meteorological instruments of the Society. With respect to the *barometer*, he says he is not aware that any objection can be offered; and as to the *hygrometer*, the observations have been found, by recent trials, not to differ materially from some expressly made in another position, at King's College, which was considered to be more favourable for such experiments. It therefore only remains to speak of the external *thermometer* and of the *rain-gauge*; of which all that can be said on the subject would be merely a repetition of what was justly said sixty years ago by Mr. Cavendish on a similar occasion (Philosophical Transactions, 1776), namely, "that, on the whole, the situation is not altogether such as could be wished, but is the *best* the house affords."

Nov. 23.—"Magnetical Observations made in the West Indies, on the Coasts of Brazil and North America, in the years 1834, 1835, 1836 and 1837." By Sir James Everard Home, Bart., Commander Royal Navy, F.R.S., the Observations reduced by the Rev. George Fisher, M.A., F.R.S.

The observations for the dip were made with an instrument of modern construction, by Dollond. Each observation consisted of an equal number of readings of the position of the needle, before and after the inversion of its poles, and a mean of all the readings taken for the true dip. Tables are subjoined, containing the dips observed at each place; the times of making a hundred vibrations of five horizontal needles, and the mean horizontal forces computed therefrom; and likewise the results estimated in the direction of the dipping needle, compared with direct experiments made with the dipping needle itself.

A paper was also read in part, entitled "On Low Fogs and Stationary Clouds." By William Kelly, M.D. Communicated by Capt. Beaufort, R.N., F.R.S.†

Jan. 11, 1838.—The reading of a paper, entitled "Experimental Researches in Electricity," Eleventh Series, by Michael Faraday

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* See Phil. Mag. and Annals, vol. ix. p. 357; and Lond. and Edinb. Phil. Mag., vol. i. p. 187.

† An account of the Anniversary Proceedings of the Royal Society, and of the papers read at the meetings prior to January 11, will appear in our next.

Esq., D.C.L., F.R.S., Fullerian Professor of Chemistry at the Royal Institution, &c., was resumed and concluded.

The object of this paper is to establish two general principles relating to the theory of electricity, which appear to be of great importance; first, that induction is in all cases the result of the actions of contiguous particles; and secondly, that different insulators have different inductive capacities.

The class of phænomena usually arranged under the head of *induction* are reducible to a general fact, the existence of which we may recognise in all electrical phænomena whatsoever; and they involve the operation of a principle having all the characters of a first, essential and fundamental law. The discovery which he had already made of the law by which electrolytes refuse to yield their elements to a current when in the solid state, though they give them forth freely when liquid, suggested to the author the extension of analogous explanations with regard to inductive action, and the possible reduction of many dissimilar phænomena to one single comprehensive law. As the whole effect upon the electrolyte appeared to be an action of the particles when thrown into a peculiar polarized state, he was led to suspect that common induction itself is in all cases an *action of contiguous particles*, and that electrical action at a distance, which is what is meant by the term induction, never occurs except through the intermediate agency of intervening matter. He considered that a test of the correctness of his views might be obtained by tracing the course of inductive action; for if it were found to be exerted in curved lines it would naturally indicate the action of contiguous particles, and would scarcely be compatible with action at a distance. Moreover, if induction be an action of contiguous particles, and likewise the first step in electrolyzation, there seemed reason to expect some particular relation of this action to the different kinds of matter through which it was exerted; that is, something equivalent to a specific electric induction for different bodies; and the existence of such specific powers would be an irrefragable proof of the dependence of induction on the intervening particles. The failure of all attempts to produce an absolute charge of electricity of one species alone, independent of the other, first impressed on the author the conviction that induction is the result of actions among the individual and contiguous particles of matter, having both forces developed to an extent exactly equal in each particle.

The author describes various experiments, with the view of showing that no case ever occurs in which an absolute charge of one species of electricity can be given. His first experiments were conducted on a very large scale: an insulated tube, twelve feet in the side, consisting of a wooden frame, with wire net-work, every part of which was brought into good metallic contact by bands of tin foil, had a glass tube, containing a wire in connexion with a large electrical machine, passed through its side, so that about four feet of the tube entered within the cube and two feet remained without; but it was found impossible in any way within this apparatus to charge the air with the least portion of either electricity.

For investigating the question whether induction is an action of contiguous particles, and for deciding that of specific inductive capacity, the author employed, in conjunction with the torsion balance of Coulomb with certain variations and additions (such as an electrometer), a new apparatus, constructed for the express purpose. This apparatus consisted of two hollow brass spheres, of very unequal diameters, the smaller placed within the larger and concentric with it; the interval between the two being the space through which the induction was to be effected. The apparatus had a tube in the lower part, furnished with a stop-cock, by means of which it might be connected with an air-pump or filled with any required gas. In place of the lower hemispherical shell of air, occupying the interval between the two spheres, any solid dielectric, of the same form, such as shell-lac, glass, or sulphur, might be substituted. Two of these instruments, precisely similar in every respect, were constructed, and the author ascertained that the inductive power was the same in both by alternately charging each and dividing the charge with the other, and finding that, in all cases, the charge remaining in the one, and also that received by the other, was very nearly half the original charge.

The experiments on which the author principally relies, in support of the correctness of his views relative to induction being exerted in curved lines, are the following: a brass ball being laid on the top of an excited cylinder of shell-lac placed perpendicularly, the charge which a carrier ball received when brought to different points near to the brass sphere was measured by means of the electrometer, and it was inferred, from the characters of the electricity, that the charge was one by induction, and from its measure, that it proceeded in curved lines. By substituting for the brass sphere a disc of metal, above the shell-lac cylinder, it was found that when the carrier ball was brought near to the middle of the disc no charge was communicated, although a sensible one was obtained at the edge of the disc, and also at a point above its centre, further removed from the excited cylinder. Corresponding and very striking results were obtained when a brass hemisphere was placed on the top of the cylinder of lac. The charge communicated at the centre of the hemisphere was only one third of that obtained at the edge of its periphery; but by taking it at a point at some height above the centre, and consequently much further removed from the inducing cause, the charge was nearly equal to that of the periphery. Here, the author remarks, the induction fairly turned a corner, exhibiting both the curved lines or courses of its action, when disturbed from their rectilineal form by the shape, position and condition of the metallic hemisphere; and also a lateral tension, so to speak, of these lines on one another; all depending on induction being an action of the contiguous particles of the dielectric thrown into a state of polarity and tension, and mutually related by their forces in all directions. In the foregoing experiments the dielectric was air, but they were afterwards varied by substituting a fluid, as oil of turpentine, and likewise a few solid dielectrics, namely, shell-lac, sulphur, carbonate and borate of lead, flint-glass, and spermaceti, and with these, corresponding results were obtained. These results, the author considers, cannot but be admitted as arguments against the received theory of induction, and in favour of that which he has put forth.

In the course of these experimental researches, some effects due to conduction, which had not been anticipated, and which were similar to the residual charge in the Leyden jar, had been obtained with such bodies as glass, lac, sulphur, &c. If the inductive apparatus, fitted with a hemispherical cup of shell-lac, after having remained charged for fifteen or twenty minutes, was suddenly and perfectly discharged, and then left to itself, it would gradually recover every sensible charge; the electricity which thus returned from an apparently latent to a sensible state being always of the same kind as that given by the charge. This return charge is attributed to an actual penetration by conduction of the charge to some distance within the dielectric at each of its two surfaces, and several experiments are adduced in support of this view. With shell-lac and spermaceti the return charge was considerable; with glass and sulphur it was much less; but with air, no decided effect of the kind could be obtained. As this was an effect which might interfere with the results, in the method the author adopted for deciding the question of specific inductive capacity, and as time was requisite for this penetration of the charge, its influence on these results was guarded against, by allowing, between the successive operations, as little time as possible for this peculiar action to arise.

The author thus states the question of specific inductive capacity which he had proposed to investigate:—I suppose A an electrified plate of metal suspended in the air, and B and C two exactly similar plates, placed parallel to and on each side of A, at equal distances, and un-insulated, A will then induce equally towards B and C. If in this position of the plates, some other dielectric than air, as shell-lac, be introduced between A and C, will the induction between them remain the same; or will the relation of C and B to A be altered by the difference of the dielectrics interposed between them?

The experiments of Coulomb, from which it appeared that a wire surrounded by shell-lac took exactly the same quantity of electricity from a charged body, as the same body took in air, seemed to the author to be no proof of the truth of the assumption, that, under such variation of the circumstances as he had supposed, no change would occur. Entertaining these doubts of the conclusions deducible from Coulomb's result, he had the apparatus previously described constructed, as being well adapted for this investigation. After rejecting glass, resin, wax, naphtha, oil of turpentine, and other substances, as unfit for the purpose in view, he chose shell-lac as the substance best calculated to serve as an experimental test of the question.

For the purpose of comparing the inductive capacities of shell-lac and air, a hemispherical cup of shell-lac was introduced into the lower hemisphere of one of the inductive apparatus, so as to nearly fill the lower half of the space between the two spheres; and their charges were divided in the manner already described; each apparatus being used in turn to receive the first charge, before its division with the other. As the two instruments were known to have equal inductive powers when air was contained in both, any deficiencies resulting from the introduction of the shell-lac would show a peculiar action in it, and if unequivocally referable to a specific inductive influence, would establish the point in question.

The air apparatus being charged, and its disposable charge being 290°, this charge was divided between the two. After the division the charge in the lac apparatus was 113°, and in the air apparatus 114°. From this it appears that whilst by the division the induction through the air lost 176°, that through lac gained only 113°. Assuming that this difference depends entirely on the greater facility possessed by shell-lac of allowing or causing inductive action through its substance than that possessed by air, then the capacity for electric induction would be inversely as the respective loss and gain; and assuming the capacity of the air apparatus as unity, that of the shell-lac apparatus would be $\frac{176}{113}$ or 1·55.

When the shell-lac apparatus was first charged, and then the charge divided with the air apparatus, it appeared that the lac apparatus, in communicating a charge of 118°, only lost a charge of 86°. This result gives 1·37 as the capacity of the lac apparatus.

Both these results, the author considers, require a correction; the former being in excess, the latter in defect. Applying this correction, they become 1·50 and 1·47. From a mean of these and several similar experiments, it is inferred that the inductive capacity of the apparatus having the hemisphere of lac is to that with air as 1·50 to 1.

As the lac only occupied one half of the apparatus containing it, the other half being filled with air, it would follow from the foregoing result, that the inductive capacity of shell-lac is to that of air as 2 to 1.

From all these experiments and from the constancy of their results the author deems the conclusion irresistible, that shell-lac does exhibit a case of *specific inductive capacity.*

Similar experiments with flint-glass gave its capacity 1·76 times that of air. Using in like manner a hemisphere of sulphur, it appeared that the inductive capacity of that substance was rather above 2·24 times that of air, and the author considers this result with sulphur as one of the most unexceptionable.

With liquids, as oil of turpentine and naphtha, although the results are not inconsistent with the belief, that these liquids have a greater specific inductive capacity than air, yet the author does not consider the proofs as perfectly conclusive.

A most interesting class of substances, in relation to specific inductive capacity, the gases or aeriform bodies, next came under the author's review.

With atmospheric air, and likewise with pure oxygen, change of density was found to occasion no change in the inductive capacity.

Nor was any change produced, either by an increase of temperature or by a variation in the hydrometric state.

The details are then given of a very elaborate series of experiments with atmospheric air, oxygen, hydrogen, nitrogen, muriatic acid, carbonic acid, sulphurous acid, sulphuretted hydrogen, and other gases, undertaken with the view of comparing one with another under a great variety of modifications.

In conclusion, the author remarks, "Thus induction appears to be essentially an action of contiguous particles, through the intermediation of which the electric force originating or appearing at a certain place, is propagated to or sustained at a distance, appearing there as a force of the same kind and exactly equal in amount, but opposite in its direction and tendencies. Induction requires no sensible thickness in the conductors which may be used to limit its extent, for an un-insulated leaf of gold may be made very highly positive on one surface, and as highly negative on the other while the induction continues without the least interference of the two states. But with regard to dielectrics, or insulating media, the results are very different; for their thickness has an immediate and important influence on the degree of induction. As to their quality, though all gases and vapours are alike, whatever be their state, amongst solid bodies, and between them and gases, there are differences which prove the existence of specific inductive capacities."

The author also refers to a transverse force with which the direct inductive force is accompanied. The experimental proof of the existence of such a force, in all cases of induction, is, from its bearing on the phænomena of electro-magnetism and magneto-electricity, of the highest importance, and we cannot but look forward with the greatest interest to the promised communication in which these and other phænomena relating to this subject will be reviewed.

ZOOLOGICAL SOCIETY.

[Continued from vol. xi. p. 474.]

January 10, 1837.—A paper was read, entitled "Observations on the Phosphorescence of the Ocean, made during a voyage from England to Sydney, N.S. Wales." By George Bennett, Esq., F.L.S., Corresp. Member of the Society.

The author commences this paper with adverting to the very slight progress which naturalists have made in their attempts to elucidate the history of the phænomena connected with the phosphorescence of the ocean, and notices some of the imaginary advantages which former observers have attributed to its presence; among others that of its indicating to mariners the existence of shoals and soundings, a circumstance which his own experience has not enabled him to confirm. He then proceeds to remark, that the sea, when phosphorescent, exhibits two distinct kinds of luminosity, one in which its surface appears studded with scintillations of the most vivid description, more particularly apparent as the waves are broken by the violence of the wind or by the passage of the ship through them, as though they were electric sparks produced by the collision, and which scintillations he considers are probably influenced, in some measure, by an electric condition of the atmosphere, as at those particular times they were observed to be much more vivid and incessant than at others. The other kind of luminosity spoken of has more the appearance of sheets or trains of whitish or greenish light, often sufficiently brilliant to illuminate the vessel as it passes through, being produced by various species of *Salpa*, *Beroë*, and other Molluscs, while in the former case the scintillations, which adhere in myriads to the towing net when drawn out of the water, probably originate in animalcules so minute that the only indication of their presence is the light which they emit.

The author remarks that " the luminosity of the ocean is often seen with greater constancy and brilliancy of effect between the latitudes 3° and 4° north and 3° or 4° south of the equator, than at any other part of the tropical regions. This circumstance, which I have observed myself, if found to be borne out by repeated observations, may be occasioned by the eddies arising from currents, for it is a curious fact worth noticing, that where currents are known to exist, the luminosity of the ocean has been observed to assume a higher degree of brilliancy. Now the westerly current is supposed to run between those parallels of latitudes from 20° or 22° west longitudes towards the Brazilian coast perpetually, and it is not improbable that nearly at the termination of the north-east trade wind a current joins with a similar current carried by the south-east trade wind; both uniting in forming the westerly current may thus cause a greater assemblage of the various tropical molluscs and crustaceous animals, a number of which possessing luminous properties may impart by their presence a higher degree of phosphorescence in that particular portion of the ocean than is observed in other situations except from similar causes. That the diffusion of the phosphoric light possessed by these molluscs does not solely depend on the creatures being disturbed (such as the passage of the ship through the water, or other somewhat similar causes,) is evident, as a luminous mass may frequently be observed to gradually diffuse its brilliant light, at some distance from the ship, without any apparent disturbance; and often during calm nights a similar glow of light is diffused over the water, without there being any collision of the waves to bring it forth; and if a light breeze springs up during the same night, the passage of the vessel leaves no brilliant trace in its wake, although the same spontaneous diffusion of light is observed in the water at some distance to be repeated as before; the phosphoric light being confined apparently solely to the occasional groups of molluscs, which when we succeeded in capturing them in the towing net, resembled for the most part pieces of crystal cut into various fantastic forms, round, oval, hexagonal, heptagonal, &c. From the bodies of these a faint or a bright light (according to the greater or less duration of time the animal may have been removed from the water, that is, we may say, by the intensity of its light we can judge of its healthy or vigorous state,) would be seen to issue in minute dots from various parts; and on the examination of both large and small specimens, the large with the naked eye and the small under a powerful lens, I could not detect any one peculiar secreting organ for this luminous excretion.

"It has often occurred during the voyage that the ocean became suddenly brilliantly luminous, and at other times merely a constant succession of scintillations were visible. Again, it was remarked that no luminosity of the ocean was visible except what proceeded from the wake of the ship, the other parts of the ocean exhibiting no phosphorescence.

"On the 15th of April, 1835, in lat. 8° 45′ north, and longitude 21° 02′ west, during the day large quantities of a beautiful pink *Medusa* were taken in the towing net, which species I was previously aware possessed luminous powers, and as expected, at night the ocean was brilliantly luminous, which luminosity continued until about 8 P.M., after which time it had almost totally disappeared. During the time the phosphorescence was visible, the *Medusa* before mentioned was captured in large numbers, but on the disappearance of the luminosity no more were caught, evidently showing that the phosphorescence of the sea this evening was occasioned by their presence. I have frequently remarked that when the ocean appears brilliantly luminous, besides the animals producing the phosphorescence, several crustaceous animals and a number of small fish are usually taken in large quantities: the presence of these may proceed from their being attracted by the phosphoric light. Sometimes during heavy rains within the tropics the sea would become suddenly luminous, as rapidly passing off again, and the effect of the sudden transitions was exceedingly splendid to the beholders. During its continuance luminous species of *Salpa, Beroë, Pyrosoma,* and other molluscs were captured in the towing net if the weather admitted of its being placed overboard."

On placing some of these luminous *Medusæ* in a bucket of water, Mr. Bennett observed that the phosphoric light is not emitted from any one particular part of the animal, but commences at different points, gradually extending over the whole body, sometimes suddenly disappearing, and at others slowly dying away. Upon squeezing the animal the hands became covered with a profusion of the luminous secretion, which could be communicated from one object to another. In conclusion several additional instances are related, occurring in different latitudes, of the beautiful and varied appearances presented by the phænomena of marine phosphorescence*.

Mr. Martin directed the attention of the Meeting to three specimens of the genus *Felis*, recently presented to the Society by Charles Darwin, Esq. One of these appeared to be a cat of the domestic race, shot in a wild state at Maldonado, differing only from our common cat in the elongation and greater size of the head. The second was the "Chat Pampa" of Azara, *Felis Pajeros* of Desmarest, shot at

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* On the subject of this paper see Prof. Macartney's Memoir in Phil. Mag., First Series, vol. xxxvii. p. 24; Mr. D. Sharpe's notice in Phil. Mag. and Annals, N.S., vol. ix. p. 144; also Mr. Brayley's paper in Lond. and Edinb. Phil. Mag., vol. vi. p. 241.

Bahia Blanca in latitude 33. The third and most interesting specimen, which had been shot at Buenos Ayres, Mr. Martin was disposed to consider as the Yagourondi or a closely allied species, since it agrees with that animal in its elongate form, stout limbs and small head, but differs from it in the greater proportionate length of tail, and also in its entire dimensions, as recorded by Desmarest, who gives the following:

	ft.	in.	lin.
Length from nose to the root of the tail .	1	11	0
Length of tail	1	1	9
Length from nose to the ear	0	3	2

In the present specimen, which is evidently adult, the measurements were found to be as follows:

	ft.	in.	lin.
Length from nose to root of tail........	2	2	0
——— of tail	1	8	0
——— from nose to ear...............	0	3	9
Height at shoulders....................	0	11	6
——— at haunches....................	1	0	6
Length of ear	0	1	2
Breadth of ear.......................	0	1	6
From nose to eye.....................	0	1	2

The hair is black, annulated with ochre, and sometimes with whitish yellow; each hair is pale brown at the base and then alternately black and yellow, the colours being repeated two or three times. Upon the head the yellow colour is most prevalent. The under fur is thick and of a pale brown colour. The hair is about the same length or rather shorter than in the domestic cat, and much harsher to the touch. The hind feet are black beneath from the heel to the toes, and there is a streak of black about an inch and a half in length, passing upwards from the front paw on the outer side. The hair of the tail is long and bushy; the legs thick and moderately long; the general form is slender; the head small in proportion to the body, and considerably arched above. The region of the anterior angle of the eye is black, with a yellowish white spot immediately above it. The eyes are very small; the ears short, broad, and obtusely pointed, thickly covered with hair, which on the outside is of a similar colour to that on the top of the head, excepting at the tip, where it is margined with black. Inside the ears the hair is of a paler hue. The under parts of the body are of the same general hue as the sides. The tail is of the same general colour as the body, but the hairs become gradually less annulated towards the tip, their basal portions being brown and the apices black; the under side is of a somewhat paler hue than the upper. The lips and nose are black.

Mr. Martin remarked, that there was some reason for supposing two species were confounded under the same name, for he was aware of the existence of a cat with a shorter tail, agreeing very closely with Azara's description of the Yagourondi. Without, however, being in possession of more ample materials he did not like to characterize the present specimen as a new species, but in the event of its ultimately being considered distinct, he proposed that it should be called *Felis Darwinii*.

Mr. James Reid read some notes on several quadrupeds, also from the collection of Mr. Darwin, including a new species of *Opossum*, which he characterized as *Didelphis hortensis*. He also noticed a very young specimen of the *Viscache, Lagostomus trichodactylus* of Brooks. This example, not much larger than our common *Rat*, differs from the adult in wanting the ridge of stiff black hairs over the eyes so conspicuous in old specimens, and in wanting also the grooves on the teeth.

Mr. Gould exhibited from Mr. Darwin's collection of *Birds*, a series of *Ground Finches*, so peculiar in form that he was induced to regard them as constituting an entirely new group, containing 14 species, and appearing to be strictly confined to the Galapagos Islands. Mr. Gould believed the whole of these *Birds* to be undescribed, and remarked that their principal peculiarity consisted in the bill presenting several distinct modifications of form, while the general contour of the species closely assimilated. He proposed to characterize them under the separate generic appellations of *Geospiza, Camarhynchus, Cactornis*, and *Certhidea*. Their characters will be found in No. xlix. of the Proceedings.

Mr. Gould then resumed the exhibition of a portion of his own collection of *Birds* from Australia, and characterized as a new species Hemipodius *melanogaster*, the characters of which are given in the Proceedings.

Mr. Gould also exhibited a new and interesting species of *Parrot*, presented to the Society by Mr. John Leadbeater, and which he characterized on behalf of the donor, as *Platycercus ignitus*, its characters being given in the Proceedings.

January 24, 1837.—Mr. Gould exhibited the Raptorial *Birds* included in the collection recently presented to the Society by Charles Darwin, Esq., and after some general observations upon the geographical distribution of the known species, proceeded to characterize the following as new to science:

POLYBORUS *galapagoensis*.

Were I not assured by Mr. Darwin that the habits of this bird strictly coincide with those of the *Caracara* (*Polyborus Brasiliensis*), its mode of flight and cry being precisely the same, I should have been induced to regard it as rather belonging to the genus *Buteo* than to *Polyborus*; but as I have satisfactorily ascertained by a close investigation, it forms a beautiful intervening link between these genera, as is evidenced by the scaling of the tarsi and the produced form of the beak; while its habits place it within the limits of the latter genus.

It is on the authority of Mr. Darwin also that I rely for the assurance of the two birds above described being the male and the female of the same species, so great is the difference between them both in size and colour.

Polyb. (Phalcobænus) *albogularis.*

I have some doubts as to whether this bird may not eventually prove to be a variety of *Phalcobænus montana*, D'Orb. The principal difference between this bird and the one described and figured by M. D'Orbigny is, that the throat and chest of the latter are brownish black, while the same parts in this bird are white.

Buteo *varius* and *But. ventralis*; Circus *megaspilus* and Otus (Brachyotus) *galapagoensis*. On the last-named bird Mr. Gould observes:

This species belongs to that section of the horned owls which comprehends the short-eared owl of England, and numerous other nearly allied species which are distributed universally over the globe, from all of which it may be distinguished by its smaller size and darker colouring. I am led to regard the members of this section as possessing characters of sufficient value to justify their being separated into a distinct genus, for which I propose the name of *Brachyotus.*

XXXI. *Intelligence and Miscellaneous Articles.*

PREPARATION OF PROTOXIDE OF TIN.

OWING to the great difficulty of preparing protoxide of tin according to the directions generally given in chemical works, I was led to make some experiments on the subject. I find the following process to be that which yields the purest oxide. Prepare a solution of protochloride of tin by dissolving the metal in hydrochloric acid, taking care always to have a great excess of the metal; the solution is then evaporated to dryness, together with a lump of tin to prevent the formation of perchloride. The tin is then separated, and the chloride weighed, and rubbed in a mortar with its equivalent, or rather more, of crystallized carbonate of soda; the mixture soon becomes fluid; it is then put into an evaporating dish and heated on the sand-bath, frequently stirring, till it becomes thoroughly black; it is then removed and well washed with boiling water, filtered, and dried at a gentle heat on the sand-bath. The oxide thus prepared is of a beautiful blue-black or slate colour; it is very soluble in hydrochloric acid, and when heated to dull redness in the air, it takes fire and burns, and is converted into peroxide.

St. Thomas's Hospital, January 3rd, 1838. S. A. Sandall.

PREPARATION OF BICARBONATE OF POTASH, BY PROF. WŒHLER.

Carbonate of potash, both in the dry state and in solution, combines very slowly with the second equivalent of carbonic acid to form the bicarbonate of potash. By means of charcoal in a finely divided state the combination may be made to take place very easily. It can be performed in the following manner: bitartrate of potash is to be heated in a covered crucible, the burnt mass to be moistened with water, put into a receiver, and carbonic acid passed through it. The absorption takes place with such rapidity that the mass becomes strongly heated, so much so that it is necessary to surround the receiver with cold water to prevent the reconversion of it into carbonate of potash. The saturation is complete when it ceases to give out heat. It is then dissolved in the smallest possible quantity of water at the temperature of 100° to 120° Fahr.; upon the cooling of the filtered solution, the greater part of the bicarbonate separates in fine crystals.—*Poggendorff's Annals.*

NEW LOCALITY OF ARSENICAL COPPER IN CHILI, BY VON ZINKEN.

Amongst some Chilian minerals from San Antonio, near Copiapo, was some massive native silver, accompanied by a tin-white substance, which, when broken, has somewhat the appearance of copper pyrites; this locality also furnishes native copper, native silver containing copper, polybarite, and calcareous spar. The fracture of this new mineral is uneven; it occurs tabular, kidney-shaped, and massive; it scratches calcareous spar, but is scratched by fluor spar and a knife; its specific gravity is not easily determined, as it is always accompanied by native silver. Exposed to heat in an open tube, it gives out arsenious acid, then a white vapour, (oxide of antimony?); upon increasing the heat it is converted into a red scoriaceous mass, which attacks the glass, and imparts to it the colour of oxide of copper; the fumes also smell of sulphureous acid. The roasted mineral fused with soda gives a button of copper not containing silver. Melted with borax, it gives a red and yellow scoriaceous mass, and a bead of copper. It is acted upon violently by nitric acid, which dissolves it, leaving a black flocculent residue recognisable as sulphur, mixed with a little arsenic. This mineral is therefore a compound of arsenic, sulphur, antimony, and copper, and is somewhat similar to condurrite, an arsenical copper found at Condurrow, near Camborne, Cornwall.—*Poggendorff's Annals*, vol. 41, p. 392.

DEFINITE COMBINATION OF OXIDE OF SILVER AND OXIDE OF LEAD, BY PROF. WŒHLER.

When a salt of silver is contained in a solution of a salt of lead, caustic alkali throws down a yellow precipitate,—a reaction which is of great interest in analytical chemistry. This yellow precipitate is insoluble in excess of caustic alkali; but by digestion in the latter, any free oxide of lead precipitated with it may be separated. This yellow substance, according to analysis, is a combination of 1 equivalent of oxide of silver, and 2 equivalents of oxide of lead; and 100 parts consist of 34·23 oxide of silver, and 65·77 oxide of lead. Upon exposure to the light it turns black. Upon heating to redness it gives a mixture of metallic silver and oxide of lead. In hydrogen gas it is reduced to an alloy with a very gentle heat. It is easily soluble in nitric acid.

In a mixture of a salt of silver with a salt of protoxide of manganese, a caustic alkali throws down a black precipitate. This appears to be a very intimate mixture of metallic silver with peroxide of manganese. It is dissolved by acids without the disengagement of any gas, and therefore without the decomposition of the acids, because the silver is oxidated by the oxygen of the peroxide of manganese.—*Poggendorff's Annals*, v. 41, p. 344.

A NEW METHOD OF OBTAINING CHROME ALUM, BY R. F. MARCHAND.

When bichromate of potash is dissolved in hot hydrochloric acid, upon cooling, the crystallized compound, discovered by Peligot, of chromic acid with chloride of potassium is formed, and there remains in the mother liquor chloride of potassium and chloride of chrome. If, instead of the bichromate, the chromate of potash is substituted, no compound of chromic acid with chloride of potassium is formed, but a mixture of chloride of chrome and chloride of potassium is obtained; the latter salt may be completely separated by crystallization.

If to the hydrochloric acid about one-eighth of its weight of sulphuric acid is added, one half of the chromic acid is converted into oxide of chrome, which combines with the sulphuric acid and the sulphate of potash, and the double salt of chrome alum is obtained; at the same time there is formed chloride of potassium, which, if sulphuric acid is added in excess, is converted into sulphate of potash, and bichloride of chrome is formed, which, upon evaporation, disengages chlorine with the production of chloride of chrome.

Fischer's method of preparing chrome alum by means of bichromate of potash, alcohol, and sulphuric acid, is so simple and advantageous, that any other method of procuring it is rendered unnecessary; but still the formation of oxide of chrome from chromic acid, by this method, is not altogether without theoretical interest, even if, in practice, it cannot be turned to account.—*Poggendorff's Annals*, p. 594.

EXAMINATION OF MALT LIQUORS.

Professor J. N. Füchs of Munich has devised the following method of examining malt liquor with regard to the ingredients contained in it, which he calls a "Hallymetrische Bierprobe." It is founded on the solubility of common salt in water; 36 parts of salt chemically pure being dissolved by exactly 100 parts of water.

A graduated tube is previously prepared, called a "Hallymeter." It consists of two glass tubes, one narrow, the other wide, joined together, somewhat funnel-shaped, of a capacity rather more than sufficient to hold the required fluid and the undissolved salt. The smaller and lower part of the tube is so graduated as for the larger divisions to contain 5 grains of prepared salt with space between each division, so that it may be divided into tenths: it is graduated with powdered salt in a saturated solution of salt, the salt being previously sifted through a fine sieve.

First process.—330 grains of common salt are added to 1000 grains of the beer under examination in a covered glass vessel. It is heated in warm water to the temperature of 100° Fahrenheit; the salt is thereby dissolved in about five minutes. It is cooled, and by carefully blowing through it, all the carbonic acid is expelled. The solution is weighed, and the loss of weight is the carbonic acid, which suppose to be 1½ grain, which is about the quantity contained in good beer.

The contents of the glass vessel are poured into the "Hallymeter," taking care that no portion of the undissolved salt is left behind; the undissolved salt must be brought into the smallest possible space by being shaken down until it will not sink any lower in the tube, which will require 15 minutes. By referring to the graduated scale on the tube, it will show how much salt remains undissolved; this quantity, deducted from the quantity first taken, will give the quantity dissolved. Thus 330 grains of salt added to 1000 grains of beer, gave, by trial, 17·3 grains of salt undissolved; therefore 17·3 from 330 give 312·7 grains dissolved, which is equal to 868·61 grains of water, (36 grains of salt being dissolved by 100 of water); this 868·61 grains, deducted from the 1000 grains of beer under examination, leaves 131·39 grains as the whole of the ingredients contained in the beer.

Second process (*for ascertaining the weight of the extract only*).—Another portion of 1000 grains of beer is taken and evaporated down to one half, by which means all the spirit of wine is driven off; it is cooled and weighed, and treated with a proportionate quantity of salt as before. In ordinary cases it is advisable to add 180 grains of salt to the evaporated solution; thus, if to the 500 grains 180 grains of salt have been added, and there remain 21·3 grains of salt undissolved, then 158·7 grains have been dissolved, which is equal to 440·83 grains of water; this, deducted from 500 grains, *gives* 59·17 *for the extract*; now add to the extract 1·5 for carbonic acid (as by first process), and deduct this amount 60·67 from the quantity found by the first process, 131·39; there remains 70·72 *grains for the spirit of wine.*

The 1000 grains of beer therefore consist of

868·61	water,
70·72	spirit of wine,
59·17	carbonic acid,
1·50	extract.
1000·	

As it is necessary that the common salt should be chemically pure, Mr. Füchs has given the following process for obtaining it from ordinary salt.

Purification of Common Salt.

The salt is dissolved in lime-water, or if it contains much magnesia, in very weak cream of lime. To the clear solution muriate of barytes is added as long as any precipitate falls; it is then filtered, and carbonate of ammonia, with a little caustic ammonia, is added, by which the lime and excess of barytes are precipitated. After standing 24 hours, it is tested by oxalate of ammonia; and if no cloudiness ensues after two hours, it is free from lime. The clear solution is then evaporated, and the residue submitted to a low red heat; a saturated solution, exposed to a temperature of about 50° Fahrenheit, [query 30°,] deposits crystals of pure salt.

ON SOME NEW COMPOUNDS OF CHLORINE. BY MONS. H. ROSE.

Hitherto the composition of the volatile compounds of chlorine has been determined by means of the known composition of the oxide or oxacid, formed by the decomposition of water, at the same time as hydrochloric acid. Since, however, the discovery of chromate of chloride of chromium, $(2\,\dddot{\mathrm{Cr}} + \mathrm{Cr\,Cl^3},)$* it is no longer possible to apply the same mode of determination to the composition of all the volatile compounds of chlorine, and it became necessary to submit to a quantitative analysis such of these combinations, in the formation of which a substance containing oxygen was employed. M. Rose, proceeding on this principle, has discovered that the two bodies which are formed by the reaction of chlorine gas upon the oxides of tungsten and molybdena, the chloride of tungsten and the chloride of molybdena, possess a composition analogous to the chromate of chloride of chromium. As they are converted into hydrochloric acid, and tungstic or molybdic acid, when treated with water, it was supposed that their composition was analogous to that of the two last acids. In fact, during the preparation of chloride of tungsten there is obtained, besides the chloride, another more volatile chloride corresponding to the oxide of the same metal, and also a tungstic acid which is not volatile. The same products, formed by the decomposition of the chloride, appear when it is suddenly exposed to a strong heat. Thus, it is not entirely composed of tungsten and chlorine, but must contain oxygen; the volatile compound cannot, however, be obtained perfectly free from excess of tungstic acid, which mixes with it during its formation. The same happens with hyperchloride of molybdena as with chloride of tungsten; their composition may therefore be represented by $2\,\dddot{\mathrm{W}} + \mathrm{W\,Cl^3}$ and by $\mathrm{Mo\,Cl^3}$.

The chromate of chloride of chromium, above described, is the result of the reaction of chromate of potash, chloride of sodium, and sulphuric acid. If, instead of employing chloride of sodium, bromide of sodium or potassium be substituted for it, bromine is obtained quite free from chromium. This difference of reaction admits of the detection of slight traces of a metallic chloride in large quantities of metallic bromides, which would otherwise be extremely difficult. If bromide of potassium or sodium be submitted to distillation with chromate of potash and sulphuric acid, and if the product of the distillation be received in ammonia, no trace of chromium will be found in it, if the salt was quite free from chloride of potassium or sodium. —*Journal de Chimie Médicale, December* 1837.

NEW ACID FORMED BY THE COMBUSTION OF ALCOHOL AROUND AN INCANDESCENT PLATINA WIRE.

M. Leroy states that this acid is liquid, and of a consistence similar to that of the oil of sweet almonds or olives, and that it is perfectly limpid. It is greasy and unctuous to the touch; it spots paper like a fat substance, and the spot remains for shorter or longer time, according to the temperature. It has a slight smell when totally freed from acetic acid. This odour is peculiar, and not at all aromatic; it has a bitter taste; its after taste is sharp, and resembles that termed *metallic*. Its specific gravity at 47° Fahr. is 1·1315; it is slightly volatile, and reddens litmus paper strongly.

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* See Mr. Walter's paper, p. 83 of our last number.

The chemical properties of this acid are, that it boils between 122° and 131° Fahr., and gives off pungent vapours which affect the eyes. It is, however, less volatile than water and concentrated acetic acid; it becomes more viscid at a few degrees below 32°, and is nearly as thick as soft butter. Light does not appear to act upon it; the action of the air is but little known. This acid, when kept in half-filled bottles, appears to be converted into very concentrated acetic acid, and a volatile product. Do these result from the absorption of oxygen? It combines with water in all proportions: the solution reddens litmus paper strongly.

When mixed with liquid ammonia, it does not at first appear to suffer any alteration; but if the mixture be heated to 80° Fahr., it soon becomes of a brown colour; if the temperature be raised to 160°, the colour becomes deeper and deeper; if it be then suddenly cooled, a glutinous product is obtained, which is of a dark colour, and in which numerous crystals may be perceived. This production of this colour, says the author, I had attributed to the action of the heat; but having taken fresh portions of ammonia and the acid in the same proportions, and left them exposed to the atmosphere in a watch-glass, it became coloured in twenty-four hours, but no crystals were formed. This mixture is volatile. Does this acid contain aldehyde? Are the crystals the ammonaldehyde of Liebig? In the absence of proof, opinion should be suspended. I thought, at first, the brown-coloured product the aldehydarz of Liebig: I then threw it into water, and it dissolved totally without the slightest residue.

When this acid is put into contact with fine crystals of nitrate of mercury, the mixture becomes milk-white; but if heated to a temperature between 140° and 160°, ebullition occurs, vapours are given off which affect the eyes very powerfully, and a globule of a greyish-blue colour soon forms at the bottom. This globule, says M. Leroy, seems to be cemented by a fatty matter, and on examining it with a strong glass, small brilliant points were perceptible, which appeared to be globules of mercury. When a lighted taper is presented to this acid, it burns with a white flame.—*Journal de Chimie Médicale, December* 1837.

ON GENTIANIN. BY M. TROMMSDORFF.

MM. Henry and Caventou, in 1821, first separated a crystallizable substance from gentian root. They attributed all the bitterness of the plant to it, and called it gentianin. This substance was a mixture. According to Trommsdorff, it may be obtained pure by the following process. Treat two pounds of gentian root with æther till it ceases to dissolve anything; the greater part of the æther is to be separated by distillation, and the remainder is to be treated with alcohol. The evaporation of the filtered liquor gives a deep yellow-coloured residue of a crystalline appearance, and containing much resin; to deprive it of which it is to be repeatedly washed with small portions of cold æther. In this operation, the æther acquires a yellowish-brown colour, and is most intensely bitter: by evaporation it leaves an uncrystallizable residue.

The gentianin thus obtained has a decidedly bitter taste, derived from a small quantity of resin. It is to be dissolved in a small quantity of alcohol, to be crystallized, and again washed with cold æther. The crystals, after each washing, are to be pressed between filtering paper. Two pounds of gentian yielded only about 120 grains of gentianin.

Gentianin is in the state of fine silky needle-form crystals, and of a sulphur-yellow colour; it is but very slightly bitter, and this bitterness is entirely got rid of by repeated solutions in, and crystallizations from alcohol, after which it is insipid, even when in solution.

Gentianin is insoluble in cold water, very slightly dissolved by boiling water, and much more soluble in alcohol and æther. When heated to a temperature of above 212°, it sublimes entirely, and condenses into silky yellow crystals.

The subacetate of lead and nitrate of silver do not precipitate it from solution. The chloride of iron and the salts of copper give a precipitate. It strongly resists the action of acids. Some very concentrated mineral acids only appear to decompose it. Some seem also to render it more soluble in water. It does not act upon litmus paper.

The most remarkable circumstance is, not only that it dissolves with the utmost facility in alkaline solutions, and becomes of a bright golden colour, but it combines with the alkalies in several definite proportions. These compounds are yellow, crystallizable, and possess an alkaline reaction. This acid (for it is one) decomposes the alkaline carbonates, expelling the carbonic acid.—*Journal de Chimie Médicale, December* 1837.

ON QUASSIN.

Wiggers gives the name of quassin to the bitter principle of the *Quassia amara* and *Quassia excelsa*. The following is the process which he employed to obtain it; boil the wood, cut into thin slices, in water; filter and evaporate the solution to three quarts; afterwards allow it to cool, and add some lime water to it, and leave them together for a day. The lime separates the pectin and the other substances. Filter and evaporate to dryness, and add to the residue about 80 or 90 per cent. of alcohol. This menstruum dissolves the quassin, a little chloride of sodium, nitrate of potash, and an organized brownish matter. By distillation a residue of a light yellow is obtained; this is to be treated with a mixture of absolute alcohol and æther, until the quassin appears pure. A little water is to be added to this solution, and it is to be allowed to evaporate in the air. A fresh quantity of quassin may be obtained by collecting the substances separated by the æther, and by treating them afresh with absolute alcohol and æther, as already described. The quassin has the form of small white prisms; it is very slightly soluble in water, 100 parts dissolving only 0·45. The aqueous solution of quassin is precipitated white by tannin; chlorine and iodine produce no effect upon it. It is scarcely soluble in æther; alcohol is the best solvent, the action of which is greater the more highly rectified it is, and as the temperature is higher. All its solutions are colourless. The quassin is a neutral body; the sulphuric and nitric acids dissolve it, but do not combine with it, nor are they neutralized by it; heat separates them and leaves the quassin pure. It is composed of

Hydrogen	6·827
Carbon	66·912
Oxygen	26·261
	100·

—*Journal de Chimie Médicale, December* 1837.

NEEDLES RENDERED MAGNETIC BY THE NERVES.

The *Compte Rendu* for January 2, 1838, which we have just received, contains the following important notice: M. Becquerel made the following communication to the French Academy from a letter he had received from M. De La Rive: "Dr. Prevost, of Geneva has succeeded in magnetizing very delicate soft-iron needles, by placing them near to the nerves, and perpendicular to the direction which he supposed the electric current took. The magnetizing took place at the moment when on irritating the spinal marrow a muscular contraction was effected in the animal."—[W. F.]

METEOROLOGICAL OBSERVATIONS FOR DECEMBER 1837.

Chiswick.—Dec. 1. Clear: very fine: frosty at night. 2—5. Dense fog. 6. Cloudy and cold: snow at night. 7. Sleet: hazy. 8. Foggy: showery. 9. Foggy. 10. Hazy: very fine. 11—13. Fine. 14. Frosty: very fine. 15, 16. Fine. 17. Rain. 18. Stormy and wet. 19. Overcast: rain. 20. Boisterous with rain. 21. Cloudy: very fine. 22. Drizzly. 23. Slight haze. 24. Hazy. 25. Very fine. 26. 27. Hazy and mild. 28—31. Very fine.

Boston.—Dec. 1. Fine: rain early A.M. 2. Fine. 3, 4. Foggy. 5. Cloudy. 6. Cloudy: snow P.M. 7. Cloudy: rain P.M. 8. Cloudy: rain early A.M. 9. Cloudy. 10, 11. Cloudy: rain early A.M. 12. Fine: rain P.M. 13, 14. Cloudy. 15, 16. Fine: rain P.M. 17. Cloudy. 18. Cloudy: rain early A.M. 19. Cloudy: rain P.M. 20. Rain: hurricane with rain P.M. 21. Cloudy. 22—24. Cloudy: rain early A.M. 25. Fine: rain early A.M. 26—29 Cloudy. 30. Fine. 31. Fine: rain early A.M.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; and by Mr. VEALL at Boston.

Days of Month. 1837. Dec.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8¾ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8¾ A.M.	London: Roy. Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering Max.	Self-registering Min.	Max.	Min.								
F. 1.	29·884	30·131	29·867	29·38	42·4	50·8	41·8	50	26	41	SW.	W.	calm	·038	...	·08	40
S. 2.	30·212	30·329	30·279	29·84	36·4	47·7	36·0	40	27	35	SW.	SW.	calm	...	...	...	36
⊙ 3.	30·428	30·446	30·424	30·08	37·2	42·6	35·7	39	24	36	ENE.	E.	calm	...	...	...	35
☽ M. 4.	30·500	30·534	30·464	30·15	33·2	41·3	32·4	37	28	28·5	N.	S.	calm	...	...	...	31
T. 5.	30·372	30·404	30·228	30·05	37·8	40·3	33·3	38	36	37	NE.	NE.	calm	...	...	...	33
W. 6.	30·126	30·167	29·946	29·88	37·9	40·0	36·8	36	30	39	N.	NE.	calm	...	·08	...	33
Th. 7.	29·794	29·835	29·749	29·53	34·3	38·9	31·7	34	30	38	NE.	NE.	NE.	·061	·15	·06	32
F. 8.	29·668	29·690	29·656	29·32	37·0	39·0	34·5	41	30	40	NNE.	S.	calm	·075	·01	·56	35
S. 9.	29·690	29·838	29·710	29·33	35·4	41·0	34·7	42	32	35	N.	N.	calm	·019	·01	...	34
⊙ 10.	30·024	30·102	30·048	29·70	36·2	40·2	34·6	41	35	37	N.	NE.	calm	...	...	·04	34
M. 11.	30·110	30·151	30·113	29·76	37·9	41·5	36·0	38	29	35	N.	NE.	calm	·027	...	·04	33
○ T. 12.	29·994	30·079	29·954	29·55	34·7	40·2	34·6	41	29	35	W.	W.	calm	...	·02	...	33
W. 13.	30·040	30·191	30·088	29·69	36·4	39·0	34·9	43	26	35	SW.	NW.	calm	...	...	·05	34
Th. 14.	30·256	30·284	30·194	29·85	37·2	43·0	36·0	44	33	36	SE.	S.	W.	...	...	...	34
F. 15.	30·156	30·180	30·110	29·73	37·7	43·4	36·6	44	27	36	SSE.	S.	NW.	...	...	...	37
S. 16.	29·886	29·906	29·789	29·52	38·8	40·4	37·0	50	42	38	ESE.	S.	calm	...	·12	·08	35
⊙ 17.	29·808	29·821	29·610	29·40	46·8	48·2	38·7	52	46	46·5	SE.	S.	calm	·063	·37	·10	40
M. 18.	29·362	29·571	29·373	28·87	50·5	51·4	46·5	54	42	50	SW.	SW.	calm	·191	·11	·16	46
T. 19.	29·976	29·961	29·697	29·43	45·2	53·6	45·2	55	44	47	WSW.	SW.	calm	·166	·38	·22	44
☾ W. 20.	29·560	29·603	29·273	29·12	53·2	54·2	45·0	54	40	53	SW.	SW.	calm	·286	·10	·29	46
Th. 21.	29·964	30·232	29·959	29·53	42·2	55·2	42·0	46	38	42	N.	NE.	W.	·055	...	·73	42
F. 22.	30·100	30·124	29·862	29·62	44·9	45·7	41·0	52	46	43	SSW.	SW.	calm	...	...	·06	44
S. 23.	29·942	29·957	29·855	69·39	49·4	52·7	44·9	50	42	48·5	W.	SW.	calm	...	...	·04	47
⊙ 24.	30·020	30·019	29·766	29·51	47·3	50·8	46·0	52	48	44	SSW.	W.	W.	...	...	·02	45
M. 25.	29·796	29·963	29·798	29·16	52·2	53·3	47·0	54	37	53	SW. var.	W.	calm	...	...	·03	47
T. 26.	29·950	29·952	29·761	29·42	47·6	55·3	45·2	49	42	45	S.	S.	calm	...	...	...	45
● W. 27.	29·806	29·823	29·803	29·39	44·5	50·4	43·6	49	45	42	ENE.	SE.	calm	...	...	...	44
Th. 28.	29·896	29·904	29·794	29·42	48·8	49·7	44·3	50	43	47	S.	S.	calm	...	...	...	45
F. 29.	29·860	29·852	29·846	29·45	47·5	50·7	44·3	51	45	45	SE.	SE.	calm	...	...	...	45
S. 30.	29·966	29·965	29·903	29·47	49·8	50·7	47·0	51	39	45·5	SE.	SW.	calm	...	...	...	47
⊙ 31.	29·994	30·014	29·980	29·50	46·7	51·8	46·3	50	39	43	SW.	SW.	calm	...	...	·02	45
	29·972	30·033	29·899	29·54	42·2	46·6	39·8	46·03	36·12	41·1				Sum ·981	1·35	2·58	39·4

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XXXII. *On the mutual Voltaic Relations of certain Peroxides, Platina, and inactive Iron. By Professor* SCHŒNBEIN.*

To Dr. Michael Faraday.

MY DEAR SIR,

FROM a series of experiments lately made by me with the view of ascertaining the voltaic relations of some peroxides, platina, and inactive iron to one another, I have obtained results, which, in my opinion, are such as to throw some additional light upon the cause of voltaic electricity, and to modify, to a certain degree at least, the notions we have hitherto entertained about that interesting subject. You will recollect that the voltaic relation of peroxide of lead to iron engaged my attention some time ago †, and you are perhaps also aware of the fact stated by me in Poggendorff's *Annalen*, that the peroxide in question, if voltaically associated with iron, disappears by degrees when plunged into nitric acid of any strength. Now as we know that no chemical action whatever takes place under the circumstances mentioned, iron being in its peculiar condition, and having, in a voltaic point of view, all the properties of platina, I could not but be very much surprised at the disappearance of the peroxide of lead. Although I was not able to trace at the time any voltaic current or to account for any disturbance of the electric equilibrium of the arrangement alluded to, I nevertheless suspected that the solution of the substance mentioned was effected under the influence of current electricity. Having now at my disposal a galvanometer which is provided with 2000 coils and made in other respects very delicate, I have taken up that subject again, and attempted first to ascertain whether there is any voltaic relation of platina to inactive iron. In contradiction to the results which you and I obtained some time ago*, I have found by means of my galvanometer, that iron being in its peculiar condition and associated with platina gives rise to a sensible current if put into nitric acid, be the latter ever so strong or somewhat diluted with water. Making use of an acid of sp. gr. 1·4, the deviation of the needle (on putting the iron and platina wires in connection with the galvanometer) amounted to about 90°. I must not omit to state that the current excited under the circumstances mentioned is not a momentary but a continuous one, and at the same time quite independent of any oxidation of the iron. The direction of the current in question is such as it would be if the latter metal was attacked by the acid, that is to say, inactive iron is positive to platina. Another fact, as curious and interesting as that just stated, is the following one. Two platina wires being connected by one set of their ends with the galvanometer, and by the other set with nitric acid or an aqueous solution of sulphate of copper, excite a current, provided one of the ends (immersed in the fluid) of one of the platina wires be covered with a film of peroxide of lead. The current passes from the platina through the fluid to the peroxide: when the said film is so thin as to produce what are called "Nobili's colours," it disappears within a very few seconds after having been immersed in nitric acid, and the whole arrangement connected with the wire of the galvanometer. From the facts stated it appears, that platina is positive with regard to peroxide of lead, and that the disappearance of that compound is caused by a current which eliminates hydrogen at the negative peroxide, by which means the latter is reduced to protoxide of lead and rendered soluble in nitric acid. In a similar manner I have ascertained that the voltaic relation of inactive iron to peroxide of lead is exactly the same as that of platina to the said peroxide. In using peroxide of silver instead of that of lead voltaic effects are produced quite the same as those which were just spoken of, that is to say, a continuous current is excited, to which the peroxide acts the part of the cathode, and either of the metals in question that of the anode. As to the voltaic relation which one of the peroxides mentioned bears to the other, my experiments have shown that peroxide of silver is always negative with regard to the peroxide of lead, be the fluid made use of nitric acid or a solution of blue vitriol. Now, from all the facts above stated, I think we may be allowed to draw two important inferences: 1. That peroxide of silver, peroxide of lead, platina, and inactive iron constitute a series of substances, in which the preceding one is always negative with regard to that which follows in the list. 2. That any two of the four substances mentioned being voltaically associated with one another, and put either into nitric acid or a solution of sulphate of copper, excite a continuous current, which is not due to oxidation or any chemical change. It is hardly necessary to add, that the currents produced under the said circumstances are extremely feeble, being only indicated by most delicate galvanometers.

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* Communicated by Dr. Faraday.

† See Lond. and Edinb. Phil. Mag., vol. x. p. 425.—EDIT.

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* See Lond. and Edinb. Phil. Mag., vol. ix. p. 59; vol. x. p. 275, &c.—Ed.

You will agree with me, that the facts spoken of are highly important in a scientific point of view, as they seem to produce evidence in favour of that theory which asserts, that by the mere contact of heterogeneous substances their electrical equilibrium can be disturbed quite independently of any chemical action taking place between them. All chemists certainly maintain, that pure nitric acid, for instance, does not chemically affect at all either platina or peroxide of lead; and inactive iron too, as we now well know, is not in the least attacked by the said acid. Now, I ask, whence does the current originate which is produced when we combine the substances in question in such a manner as to form with them a voltaic arrangement?

I have attempted to answer this puzzling question in a paper which before long will be published in Poggendorff's *Annalen*, as well as in the *Biblioth. Univ.*, and in which you will find besides a detailed account of all the experiments made by me upon the subject spoken of. If my time was not so much taken up with a variety of business, I would have drawn up a memoir in English, and sent it to the Editors of your excellent Philosophical Magazine, for insertion; but those gentlemen will, perhaps, give a translation of the paper.

Before closing my letter allow me to communicate to you in a general manner the view which I have taken of the subject in question. In the first place, I must tell you that I am by no means inclined to consider mere contact in any case as the cause of the excitement of even the most feeble current. I maintain on the contrary, in accordance with the principles of the chemical theory, that any current produced in a hydro-electric voltaic circle is always due to some chemical action. But as to the idea which I attach to the term "chemical action," I go further than you and M. de la Rive seem to go; for I maintain, that any tendency of two different substances to unite chemically with one another must be considered as a chemical action, be that tendency followed up by the actual combination of those substances or be it not; and that such a tendency is capable of putting electricity into circulation. I will try to render this idea of mine somewhat clearer by applying it to some particular cases. Supposing a voltaic circle to be composed of platina, peroxide of lead, and nitric acid; I say that the current excited in such an arrangement is due, first, to the tendency of the acid to unite with the protoxide of lead, or, what is the same, to the tendency of one proportion of the oxygen to separate from the peroxide; secondly, to the tendency of water to combine with the same protoxide to form a hydrate; and thirdly, to the tendency of water to withdraw a proportion of oxygen from the peroxide to produce peroxide of hydrogen, which tendency, from very well-known chemical reasons, is yet increased by the presence of the acid. It is true that none of the said tendencies do lead to any chemical result, for no nitrate of lead, no hydrate, no peroxide of hydrogen is actually produced; but are we allowed to infer, from the want of a practical result, that no chemical action whatever takes place when nitric acid and peroxide of lead are put in contact with one another? I ask, are we to suppose that the chemical affinities alluded to are entirely dormant, and incapable of any exertion? The results from my late experiments induce me to answer the question in the negative. Being quite of your opinion, that chemical affinity and current electricity are but different forms of the same thing, I cannot help thinking that any sort of chemical action or tendency must be capable of being transformed into the shape of a current. For that current which is produced by inactive iron (being voltaically associated with platina) I likewise account by a chemical tendency on the part of the former metal. Though inactive iron be not in the least attacked by nitric acid, its affinity for the oxygen of the latter is, on that account, by no means entirely destroyed; the metal whilst surrounded by the acid is continually tending to oxidize itself, and the current excited in such a case is nothing else but as it were the electrical translation of a chemical exertion.

All the cases above stated, where currents are observed independently of any chemical change, can easily be explained by applying to them the same principle as that by means of which we have accounted for the current produced by nitric acid and peroxide of lead, &c. Having already passed the usual limits of a letter, I add only one more observation to my former, and I have done.

According to my experiments peroxide of silver proves to be the most powerful means for exciting in iron its peculiar voltaic condition. It surpasses in this respect even the peroxide of lead. An iron wire, for instance, one end of which is covered with only a small particle of the first-mentioned substance, will not be attacked either by nitric acid of any degree of dilution, nor by a solution of blue vitriol. The voltaic association of one substance with the other is easily effected by connecting one end of an iron wire with the positive electrode of a pile, and by plunging for a few minutes the other end of the wire into a solution of nitrate of silver.

I am just about to write a paper on this interesting subject. I am, my dear Sir, yours very truly,

Bâle, Dec. 31, 1837. C. F. SCHŒNBEIN.

XXXIII. *Observations on indirect Chemical Analysis.* By GOLDING BIRD, *F.L.S. F.G.S., Lecturer on Experimental Philosophy at Guy's Hospital, &c.*[*]

CASES constantly fall under the notice of the analytic chemist, in which, from the difficulty of separating one substance from another without a tedious operation, or from the absolute impossibility of effecting this separation in any thing like a perfect manner, the process of indirect analysis, as it has been termed, is particularly applicable. Thus, if whilst engaged in the analysis of the ash of an organic substance in which both potash and soda are contained, we require the quantitative amount of each base, almost every proposed process will to a certain extent fail. It is true that by the aid of hydro-fluosilicic acid, or the double chloride of platinum and sodium, we may obtain approximations to the truth, but even then the results obtained by these necessarily tedious processes are by no means so exact as those obtained by the aid of indirect analysis. A formula for the resolution of the last-mentioned problem (the quantitative estimation of potash and soda in mixtures of these bases) was proposed in Berzelius's work, *Sur l'Analyse des Corps inorganiques*, 1827, by the French translator; but this was certainly very tedious and not sufficiently comprehensive in its details. Poggendorff, in the third number of the invaluable "*Handwörterbuch der reinen und angewandten Chemie* von Liebig u. Poggendorff," has given two formulæ for the same purpose, which have the advantage of possessing considerable simplicity and of being applicable to the resolution of several analogous chemical problems. For the assistance of those who may not have an opportunity of consulting the original work, I have made the following abstract of the mode proposed by Poggendorff.

If the substances are bases which form with acids neutral salts of known composition, we take an exactly weighed quantity of the mixture of the two bases and saturate it with an acid; and afterwards the same or an exactly equal quantity is to be saturated with a second acid. From the weight of the salts formed with the two acids, we get the weight of each base by calculation in the following manner.

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[*] Communicated by the Author.

		The base A.	The base B.	Known weight of the mixed salts.
Let the unknown quantity by weight of	=	x,	y,	
Let the unknown proportion of salt formed by the acid C	=	$a\,x$,	$b\,y$	h
Ditto by the acid D	=	$a'\,x$,	$b'\,y$	h'

Then we have first,

$$a\,x + b\,y = h \quad (1.)$$
$$a'\,x + b'\,y = h' \quad (2.)$$

and consequently

$$x = \frac{h\,b' - h'\,b}{a\,b' - a'\,b} \quad (3.) \qquad y = \frac{h'\,a - h\,a'}{a\,b' - a'b} \quad (4.)$$

Poggendorff then directs the respective values of a, b, a', b' to be deduced in the following manner.

If the atomic weights of

	The Bases.		The Salts.				
	A	B	A C	B C	A D	B D	are expressed
by	M	N	P	Q	P'	Q	

it is clear that $a = \frac{P}{m}$ $\quad b = \frac{Q}{n}$

$$a' = \frac{P'}{m} \qquad b' = \frac{Q'}{n};$$

that is, the unknown quantity of the salt $a\,x$, which the unknown quantity x of the base A forms with the acid C, must bear the same proportion to the quantity x, as the atomic weight of the A C bears to that of the base A. By means of the known atomic proportions are the quantities a, b, a', b' given, and with them by the formulæ (3.) and (4.) are the sought for proportions by weight, x and y, of both bases found.

An example will render this clearer. Suppose that we have a mixture of potassa and soda and it is required to ascertain the exact proportions in which the two bases exist. We take two exactly equal portions of the mixture, and saturate one with sulphuric acid, the other with hydrochloric acid. Now, if the weight of the sulphates thus obtained is $2·978656 = h$.

Do. of hydrochlorates thus obtained is $2·479338 = h'$.

From their atomic weights we get the following proportions.

For sulphate of potassa.	For sulphate of soda.
$a = \frac{1091·081}{589·916} = 1·84955$	$b = \frac{892·062}{390·897} = 2·28209$
For hydrochlorate potass.	For hydrochlorate soda.
$a' = \frac{932·566}{589·916} = 1·58085$	$b' = \frac{733·547}{390·897} = 1·87657$

Then by the equations 3. and 4. we have

Potass .... $x = 0·5$
Soda .... $y = 0·9$.

It is evident that mixed carbonates, acetates, &c. of potass and soda may be used in the above process quite as easily as the pure bases, with which we rarely have to deal in analysis.

The above calculations become simpler, if, instead of using two portions of the mixed bases, we take but one and convert it into sulphates, if not already in that state; then knowing the weight of the mixed salts, we determine the quantity of sulphuric acid present by means of chloride of barium; then the weight of the mixed sulphates *minus* the quantity of sulphuric acid gives us the weight of the mixed bases: let this weight be called h' in the equation (2.), and then consider a' and b' as equal to 1; h retaining its former value. The simplification of the formulæ thus proposed by Poggendorff will be rendered more obvious by copying them in this improved state; the only calculations required after determining the weights of the mixed sulphates and sulphuric acid being

$$x = \frac{h - h'b}{a - b} \qquad y = \frac{h'a - h}{a - b}.$$

Even these calculations may be still further reduced if we substitute the following equation for finding the value of y; for that proposed by Poggendorff,

$$h' - x = y,$$

and when we recollect that (in the case of potass and soda) a, and b, are constant quantities, we shall scarcely desire greater simplicity of calculation. As an example of this improved mode, I will take the following case, as it will serve to contrast the two processes.

We have a mixture of the sulphates of potass and soda whose united weights equal 53; on dissolving them in water and precipitating by chloride of barium, the quantity of sulphuric acid present was found to be 25. Then $53-25=28$, the weight of the mixed bases. Then letting, as mentioned above,

$$\left.\begin{array}{l}\text{the weight of sulphate } \quad 53 = h;\ a = 1{\cdot}84955\\ \text{———— of mixed bases } 28 = h';\ b = 2{\cdot}28209\end{array}\right\}\text{ as before;}$$

then

$$(2{\cdot}28209 \times 28 = 63{\cdot}89852) = (h'b)$$

$$(63{\cdot}89852 - 53 = 10{\cdot}89852) = (h'b - h)$$

$$(2{\cdot}28209 - 1{\cdot}84955 = 0{\cdot}43254) = (a - b)$$

$$\left(\frac{h - h'b}{a - b}\right) = \frac{10{\cdot}89852}{{\cdot}43254} = 25{\cdot}19 = x, \text{ quantity of potass and}$$

$$(h' - x) = 28 - 25{\cdot}19 = y, \text{ quantity of soda.}$$

This mode it is obvious is equally applicable to mixtures of barytes and strontia or lead, lime and magnesia, &c., although it is evident that it will only give exact results when the atomic weights of the mixed bases differ considerably from each other; the greater this difference is, the more exact are the results. Still, however, it is much to be doubted whether the analytic chemist would not prefer the results of experiment to those of calculation, excepting in those cases where the mixed bases, as potass and soda, scarcely admit of quantitative estimation by direct experiment.

22, Wilmington Square, Jan. 3, 1838.

XXXIV. *Further Observations on the ultimate Analysis of Organic Compounds.* By Robert Rigg, *M.R.I.*[*]

In my short paper on ultimate analysis which has already appeared in the Philosophical Magazine, p. 31, I described the method which I adopt in the examination of solid bodies only; I therefore now propose to submit to the analytical chemist my equally simple method of analysing liquids. Premising that the apparatus of tubes, &c., together with the black oxide of copper, are such as have been heretofore described, I observe, in the first place, that the liquid to be analysed is accurately weighed in a small tube, whose length is from one to two inches, and whose diameter is such that it easily slides within the analysing tube. Round a slender wire from six to eight inches long is folded a little dry amianthus covering about one inch of its length; this amianthus is *then rolled in black oxide of copper*, and is with the wire put into the small tube which contains the liquid to be analysed. The whole is then placed within the analysing tube, which is filled up with black oxide of copper and amianthus, and proceeded with as in the analysis of solids.

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[*] Communicated by the Author.

The flame of the lamp being first applied to the part of the analysing tube which is most remote from the liquid, the oxide and the wire become ignited in this part; and as they gradually conduct the heat to the other extremity of the tube, the vapour of the liquid is by slow degrees given off, and in its passage through the ignited oxide is decomposed. When the experiment is nearly finished the flame of the lamp is extended to that part of the tube where the liquid under analysis was placed, and thus every portion of the vapour of the compound is brought into contact with the oxide in a state of ignition. This constitutes the analytical process: the methods of measuring and calculating the gaseous products are the same as in the analysis of solids. I may add, that no dependence whatever can be placed upon an experiment wherein the compound under analysis has been subjected to a quick process. It will be superfluous also for me to say, that in the analysis of volatile liquids everything must be refrigerated.

In this way I have analysed æther, alcohol, spirit of different kinds, acetic acid, sap of plants*, the liquid separated by the drying of plants, &c., and the following are among the results which I have obtained.

	Sp. gr. at 60°.	Carbon.	Hydrogen.	Water.	
Æther...	725	66·8	10·9	22·3	100
Do.......	740	64·4	10·5	25·1	100
Do.......	758	61·7	10·1	28·2	100
Alcohol	804	53·6	8·7	37·7	100
Do.......	819	50·6	8·3	41·1	100
Do.......	825	50·0	8·2	41·8	100
Spirit ...	839	46·6	7·6	45·8	100
Do.......	920	27·6	4·5	67·9	100

From these I think we are warranted in drawing the conclusion, that the following will be the constitution of

	Sp. gr. at 60°.	Carbon.	Hydrogen.	Water.	
Æther......	700	70·6	11·5	17·9	100
Do.	720	67·6	11·1	21·3	100
Alcohol ...	796	55·1	8·9	36·0	100

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* On evaporating to dryness the sap of plants at or near the boiling temperature, the dry matter which is left does not contain more than from two to four tenths of the compound of carbon, hydrogen, and nitrogen which the sap at first contained.

The liquid which I obtained by gentle distillation from the roots of the hyacinth *when young* I found to be constituted as under :[*]

Carbon	·250
Hydrogen...........	35
Nitrogen	·158
Water	99·557
	100·

It had a specific gravity of 1001·6, that of water being 1000. The dry solid matter of the same young roots examined in the mass was constituted of

Carbon	40·8
Hydrogen	1·4
Nitrogen	5·5
Residual	3·5
Water	48·8
	100·

Thus showing an excess of hydrogen in these young roots; but when the plant is full grown, I find the hydrogen predominating in the *spiral vessels only*[†].

The valuable suggestions which I have received from the Rev. J. B. Reade of Peckham, have enabled me to prove that which appears to be a principle in vegetable physiology, viz. that the chemical composition of the roots of plants varies with the season and the stage of their growth, and that the constituent parts of the full-grown roots have their own peculiar chemical characters.

In the analysis both of liquids and solids by the method now proposed, it must be observed that the amianthus which is used for condensing the vapour of water condenses also carbonic acid gas, and this too in quantities proportionate to the quantity of water condensed in the amianthus. The quantity of carbonic acid gas so condensed varies also with the mode of conducting the experiment. Every analyst must find out this quantity for himself. By my mode of conducting experiments, the gas which remains in the analysing tube is equal to about $\frac{2}{10}$ of the interstices of the tube which are not filled up. This I ascertain by driving off the condensed water and collecting the gaseous products over mercury in the usual way.

A very satisfactory mode of proving the correctness of the analysis of any compound is to repeat the experiment, and suffer the gaseous products to pass off in their moist state, and in calculating the products of the analysis to allow for the increase in volume by moisture.

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[*] My best analysis of this and similar liquids can only be considered as an approximation to their real constitution.

[†] See page 422 of the last volume of Phil. Mag.

The results arising from my analysis of alcohol and æther do not favour the view which is very generally taken in the present day of the *vinous fermentation* and its products, to prove the inaccuracy of which, it is only necessary to make experiments and to examine them *in all their parts* with ordinary attention. Indeed the erroneousness of the commonly received theory is evidenced by the combination of carbonic acid gas with vinous liquors, with æther and water, and with alcohol and water, when a compound very different from sugar is the product.

The true theory which appears to run through every part of the composition and decomposition of vegetable matter can only be obtained by continuous and extensive observation. An outline of a part of the necessary course of experiment was laid before the Royal Society about two years ago. It must ever be borne in mind that the entire series of results presents itself in one continued chain, each link holding its necessary position and its just proportion. Isolated experiments, like broken links, lose their value, and bewilder and mislead the inquirer.

Walworth Road, Jan. 13, 1838.

XXXV. *Analysis of some Double Salts of Mercury.* By R. H. Brett, *Esq., F.L.S., M.R.C.S., &c.*[*]

THE combinations of iodide, bromide, and chloride of potassium with bicyanide of mercury were spoken of in a former paper. The iodo-cyanide of potassium and mercury had been described by Liebig and Dr. Apjohn; the bromo-cyanide of potassium, together with the combinations of the bromides of the other alkaline and earthy metals and bicyanide of mercury, by Caillot in the *Journal de Pharmacie*: the new salt which I then described, and of which I was not able to find any previous mention, was the chloro-cyanide of potassium and mercury. All these salts possess the same crystalline form and atomic constitution: they are therefore isomorphous. The salts about to be described are also isomorphous with those already noticed, and atomically considered differ only from those described by Caillot in the substitution of the elementary atom *chlorine* for *bromine*; they are double salts, in which one haloid salt, viz. bicyanide of mercury, is constant, and probably plays the part of an electro-negative or acid element; whilst the other haloid salt, viz. a chloride, varies, and probably is electro-positive or basic; a mode of regarding the constitution of such salts propounded by Caillot. These salts are remarkable for the silky lustre which they possess, and this feature is more strongly marked in those which contain potassium as an element than in any of the others, and is best observed in crystals obtained from their alcoholic solutions.

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[*] Communicated by the Author.

The Chloro-cyanide of Ammonium and Mercury.

If we dissolve in water 13 parts by weight of sal-ammoniac with 60 parts of bicyanide of mercury and evaporate the solution, we obtain after a time a salt, which crystallizes in flattened quadrangular prisms, possessing a somewhat silky lustre when dry: if a solution of the salt be evaporated to a small bulk, it crystallizes upon cooling in small prisms of the same form as those obtained by a more gradual evaporation. This salt undergoes fusion by heat and is decomposed, ammonia and hydrocyanic acid being evolved. Alcohol is capable of dissolving this salt. When simply mixed in the cold with the mineral acids, it does not appear to suffer decomposition; when heated for some time with them its decomposition is effected.

In order to ascertain its atomic constitution, which analogy and the proportions used in its preparation would lead us to regard as an atom of each of its ingredients, the following plan was adopted.

Ten grains of the salt carefully dried over a sand-bath were dissolved in water, and a current of sulphuretted hydrogen passed through the solution; the whole was then, after being boiled for a short time, thrown upon a weighed filter, and the latter with its contents well washed, the washings being added to the filtered fluid: the bisulphuret of mercury thus obtained was black, and when dry weighed 7·3 grains = 7·94 grains of bicyanide. The quantity of bisulphuret of mercury, by calculation, assuming the salt to be compounded of 1 atom of each of its ingredients, is 7·599 = 8·26 grains of bicyanide.

The filtered fluid with the washings was kept for some time exposed to the temperature of boiling water in order to get rid of sulphuretted hydrogen; it was then mixed with a slight excess of caustic potass; the whole was then evaporated to dryness, ignited, and redissolved in water; the aqueous solution was then precipitated by nitrate of silver, rendered acid by means of nitric acid. The precipitated chloride was washed, dried, and fused; it weighed 4·5 grs. = 1·68 of chloride of ammonium, the quantity of the latter by calculation being 1·74 grains.

The results of experiment and calculation will therefore give

	Experiment.	Calculation.
Bicyanide of mercury......	7·94	8·26
Chloride of ammonium ...	1·68	1·74
Loss	·38	
	10·00	10·00

These results sufficiently approximate to warrant us in concluding that the atomic representation of the salt would be as follows:

Bicyanide of mercury .... 255·72 = 1 atom.
Chloride of ammonium........ 53·65 = 1 atom.

309·37 atomic weight of double salt.

Its symbol will therefore be $(\mathrm{Hg}\,\overset{2}{\mathrm{Cy}} + \mathrm{N}\,\overset{4}{\mathrm{H}}\,\mathrm{Cl})$.

I have adopted the term chloride of ammonium instead of that of muriate of ammonia, because the idea of the chlorine in this case being united with a metal (ammonium) $(\mathrm{N}\,\overset{4}{\mathrm{H}})$ is strictly in accordance with what actually exists in all other haloid salts, as also in those combinations of haloid salts which constitute the double salts under consideration: besides, as chloride of ammonium is isomorphous with the chlorides, iodides, and bromides of potassium and sodium, and as the ammoniacal double salt is isomorphous with the other double salts into which the metals of the fixed alkalis and earths enter as ingredients, so it is in the highest degree probable that the atom of hydrogen in the ammoniacal salt which exists over and above what is sufficient to form ammonia with the nitrogen, is not united to the chlorine as hydrochloric acid, but to the ammonia to form the metal ammonium.

The Chloro-cyanide of Sodium and Mercury.

This salt may be obtained by dissolving in water 15 parts by weight of chloride of sodium and 60 parts of bicyanide of mercury, evaporating and crystallizing. It crystallizes also in flattened quadrangular prisms of a somewhat silky lustre, is soluble in weak alcohol, from which it readily crystallizes.

Ten grains of the dried salt were dissolved in water and precipitated by sulphuretted hydrogen: the washed and dried bisulphuret of mercury weighed 7·30 grs. = 7·94 grs. of bicyanide; the calculated proportions are 7·4753 bisulphuret = 8·13 bicyanide of mercury. The aqueous solution freed from bisulphuret of mercury, together with the washings were evaporated to dryness, and heated to a dull red heat in a counterpoised platinum crucible: the resulting chloride of sodium weighed 1·78 grs., the quantity by calculation being 1·87.

The results of experiment and calculation will therefore give

	Experiment.	Calculation.
Bicyanide of mercury	7·94	8·13
Chloride of sodium	1·78	1·87
Loss	·28	
	10·00	10·00.

The atomic representation of the salt will therefore be

Bicyanide of mercury...... 255·72 = 1 atom.
Chloride of sodium ...... 53·65 = 1 atom.

309·37 atomic weight of double salt; and its symbol $(\text{Hg}\,\overset{2}{\text{Cy}}+\text{Na Cl})$.

The Chloro-cyanide of Calcium and Mercury.

This salt may be obtained by dissolving in water 14 parts of chloride of calcium and 60 parts of bicyanide of mercury, evaporating and crystallizing; its crystalline form and lustre is the same as the preceding: it is not deliquescent, is soluble in alcohol, and its aqueous solution is precipitated by oxalate of ammonia.

Ten grains were analysed in the same manner as the last salt. The quantity of bisulphuret of mercury obtained was 7·496 = 8·11 of bicyanide: the calculated proportion of bisulphuret is 7·542 = 8·203 bicyanide of mercury.

The quantity of chloride of calcium obtained was 1·7, the calculated proportion being 1·797.

The results of experiment and calculation will therefore give

	Experiment.	Calculation.
Bicyanide of mercury......	8·11	8·203
Chloride of calcium	1·70	1·797
Loss...........................	·19	
	10·00	10·000

The atomic representation of the salt will accordingly be

Bicyanide of mercury ... 255·72 = 1 atom.
Chloride of calcium...... 55·98 = 1 atom.

311·70 atomic weight of salt.

And its symbol $(\text{Hg}\,\overset{2}{\text{Cy}}+\text{Ca Cl})$.

The Chloro-cyanide of Magnesium and Mercury.

This salt is obtained by dissolving together in water 12 parts of chloride of magnesium with 60 parts of bicyanide of mercury. It crystallizes like the other salts in flattened quadrangular prisms; it is not deliquescent, easily dissolves in weak alcohol, from which it readily crystallizes. 10 grains of the salt were submitted to the same mode of analysis as that adopted for the salts above described.

7·45 grains of bisulphuret of mercury = 8·10 of bicyanide, the calculated proportions being 7·736 bisulphuret = 8·41 bicyanide of mercury. The chloride of magnesium obtained weighed 1·5 grain, the calculated proportion being 1·59.

The results of experiment and calculation will therefore give

	Experiment.	Calculation.
Bicyanide of mercury	8·10	8·41
Chloride of magnesium ...	1·50	1·59
Loss	·40	
	10·00	10·00

The atomic representation of this salt will therefore be

Bicyanide of mercury......	255·72	= 1 atom.
Chloride of magnesium ...	48·15	= 1 atom.
	303·87	atom. weight of salt.

And its symbol $(\mathrm{Hg}\,\overset{2}{\mathrm{Cy}} + \mathrm{Mg\,Cl})$.

The Chloro-cyanide of Barium and Mercury,

Obtained by dissolving together in water 24 parts of chloride of barium and 60 parts of bicyanide of mercury, evaporating and crystallizing; it assumes the quadrangular flattened prismatic form, and is precipitated by the solutions of the soluble sulphates. It dissolves easily in weak alcohol, from which it may be readily obtained in crystals.

Ten grains submitted to analysis yielded 6·5 grains of bisulphuret of mercury = 7·07 of bicyanide, the calculated proportions being 6·53 bisulphuret = 7·10 bicyanide of mercury. The chloride of barium obtained weighed 2·8, the calculated proportion being 2·9 grains.

The results of experiment and calculation therefore give

	Experiment.	Calculation.
Bicyanide of mercury ...	7·07	7·10
Chloride of barium	2·80	2·90
Loss	·13	
	10·00	10·00

The atomic representation of the salt will therefore be

Bicyanide of mercury ... 255·720 = 1 atom.
Chloride of barium ...... 104·132 = 1 atom.
359·852 atomic weight of double salt.

And its symbol $(\mathrm{Hg}\ \overset{2}{\mathrm{Cy}} + \mathrm{Ba\ Cl})$.

The Chloro-cyanide of Strontium and Mercury,

Obtained by dissolving together in water 19 parts by weight of chloride of strontium and 60 parts of bicyanide of mercury, evaporating and crystallizing. This salt easily crystallizes in long flat quadrangular prisms, having a silky lustre; it dissolves readily in weak alcohol, and very well marked crystals may be obtained from such a solution: the salt is not deliquescent.

Ten grains submitted to analysis yielded 6·95 grains of bisulphuret of mercury = 7·55 bicyanide, the calculated proportions being 7·01 bisulphuret = 7·62 bicyanide of mercury.

The chloride of strontium obtained weighed 2·37 grains, the calculated proportion being 2·38.

The results of experiment and calculation therefore give

	Experiment.	Calculation.
Bicyanide of mercury...	7·55	7·62
Chloride of strontium...	2·37	2·38
Loss	·08	
	10·00	10·00

The atomic representation of the salt will accordingly be

Bicyanide of mercury... 255·72 = 1 atom.
Chloride of strontium... 79·32 = 1 atom.
335·04 atomic weight of double salt;

and its symbol $(\mathrm{Hg}\ \overset{2}{\mathrm{Cy}} + \mathrm{Sr\ Cl})$.

There yet remain to be examined the compounds formed by the union of the different iodides of the alkaline and earthy metals with bicyanide of mercury; these however I hope to make the subject of another communication.

January 8th, 1838. R. H. B.

XXXVI. *Some Observations on the Development of the Organization in Phænogamous Plants.* *By* Dr. M. J. Schleiden.

[Continued from p. 189.]

ALTHOUGH we cannot remain one moment in doubt that in plants possessing a true *placenta centralis libera* (still less in such where, as in the *Polygoneæ*, *Taxus*, *Juglans*, *Myrica*, the placenta cannot be supposed to exist as a separate organ), the *nucleus* of the ovule is only the summit of the axis, yet the question suggests itself as to how the parietal placenta is to be understood; and I do not consider the explanation to be very difficult. We find in many of the *Aroideæ* that the axis is spread out at its summit, forming a kind of disc; upon this surface are a number of buds as ovules, similar to the arrangement which is found, in the *Synantheræ* and other families, to take place among the flower-buds; we next observe these discs expanded into lobular processes, and adherent to the edges of the carpellary leaves in all parietal or pseudocentral placentæ, a modification of the axis which is met with in *Dorstenia*, the parietal placentæ may be explained equally well, and perhaps with greater simplicity and consistency, as a mere ramification of the axis. It will not therefore surprise us, that the buds of these branches (*ovula*) grow only upon their inner side, viz., that directed towards the axis, since the same is observed in the inflorescence of many plants, for instance, in *Æsculus*. Lastly, we find the axis expanded somewhat in the shape of a basin in those plants in which the entire wall of the simple ovarium is occupied with ovules, as may also be seen in the similar modification of the stalk in many *Rosaceæ* and in *Ficus*. There cannot however be any reason adduced why such deviations in the form of the axis should be assumed as occurring in a lower internodium between the leaves and bud, whilst they are denied existence in a higher one between the carpellary leaves and ovule-bud, or are said to be impossible.

But we find in nature, that in parietal placentæ the edges of the leaves are never laid upon one another in their entire length, and adhere in that manner, but become united from below upwards by the subsequent growth of a more or less distinctly interposed substance. This interposed substance is very evident in the *Fumariaceæ* and *Cruciferæ*, in which it appears much later than the carpellary leaves, stands exactly within them, and in the latter family forms the spurious partition, by its gradual extension towards the middle and its subsequent adhesion. The placenta shows itself to be independent of the carpellary leaves, during its growth, most strikingly in the *Abietineæ*. My investigations of the earliest conditions have shown me that the organ which, since the researches of R. Brown, has been considered as an open ovarium, is only a scale-like expanded placenta, and that the organ which R. Brown has named *bractea* is the actual carpellary leaf (fig. 18). This result has been confirmed to me in a most beautiful manner by a cone of *Pinus alba*, found this spring, which upon the upper half was covered with female and upon the lower with male flowers. In the *Abietineæ* the placenta, left without the least constraint, develops itself to such an extent, that at length the carpellary leaf itself appears as a mere supplementary part. The more extended detail of these investigations being here out of place, I must beg to refer to a work on which I am at present engaged, upon which I have been occupied some years with great interest, and which is intended to include the perfect history of vegetable development in every department.

In all this variety of forms of the ovulum-bearing axis, whether it grows upwards upon the carpellary leaves or elevates itself free in the middle, there frequently occurs the additional peculiarity, that besides the reflexion sustained by the axis, which has been so often alluded to, there is another to which it is subject in consequence of the space being too limited superiorly for the development of the ovular-bud; the *ovulum horizontale* and *pendulum*, with their various intermediate states, are hereby formed. This modification, however, proceeding, as it appears to do, merely from an external necessity, viz., the extent of space allotted to it, is of far less consequence than the first-mentioned reflexion; and we find accordingly in one and the same family (in the *Dryadeæ*, for instance) both pendent and erect ovules, but it seldom occurs in a highly developed family, and probably only in the *Aroideæ*, that atropous and anatropous ovules are found together. For this reason the definition of a *radicula supera* or *infera* in botanical descriptions possesses little or no value, when regard has not at the same time been had to the internal formation of the ovule.

As we found that there was a peculiar development of the cellular tissue in the anthers, by means of which the leaf becomes converted into an organ for the production of pollen, so we observe also that there is a peculiar modification of cellular tissue in the summit of the axis or *nucleus*, by which it likewise becomes adapted to the taking on of a new organism. One of the parenchymatous cells, namely, develops itself to a much greater extent than the others, indeed out of all proportion, since it becomes subsequently converted into the sac of the embryo. This takes place in all Phanerogamia without exception, and at a period long previous to impregnation; no more than this, however, constitutes the essence of this formation. In other respects this sac is subject to the most manifold varieties: 1. In relation to form, being sometimes round, sometimes oval, cylindrical, bottle-shaped, or sometimes fiddle-shaped, or, as in *Lathræa squamaria*, where the excavations are shapeless. 2. In relation to the point of the nucleus, which is sometimes nearer, at other times farther off. 3. As to contents, at one time clear as water, homogeneous and fluid, at another opake and granular, and sometimes cellular. 4. With respect to the time of its formation, whether a longer or shorter interval before the opening of the flower. And lastly, in the greater or less compression of the nucleus. But a treatise may easily be written concerning the varieties of the embryo-sac previous to impregnation.

We have now proceeded so far in the process of development of the plant that we already stand at the door of the sanctum. The process by means of which the new organism should be formed out of the parent plant, remained during a very long time an object of the fantastic sports of the imagination, or of falsely-grounded analogies taken from the animal kingdom, which arose partly from the impossibility of actual observation on account of the imperfection of instruments; until at length Amici, Brongniart, and R. Brown threw an entirely new light on the matter by their beautiful discoveries. Yet the most important part of the secret remained undisclosed. I have prosecuted and repeated with untiring zeal the discoveries of those great men, and have not only found the most important of their individual observations confirmed as general laws, but believe that I have advanced a not unimportant step in the inquiry. I have followed the pollen-tubes (*pollens-chläuche*) already in so many (upwards of 100) different families, with the most patient investigation from the stigma into the ovulum, that there can be no doubt concerning this being the general process in all Phanerogamia. R. Brown has described more than one pollen-tube as entering into one micropyle; I have observed two to three in many plants—in *Phormium tenax* three to five, in *Lathræa squamaria* scarcely ever less than three, and once even seven.

If the pollen-tubes be followed farther into the ovulum, a process perhaps the most delicate that occurs in botanical investigations (fig. 3 and 24), it will be found that usually only one, rarely a greater number*, of the pollen-tubes entering into the micropyle penetrates the intercellular passages of the *nucleus* and reaches the embryo-sac, which being forced forwards presses it, indents it, and forms the cylindrical bag, which has already been described, in the commencement of this paper, as constituting the embryo in the first stage of its development, which consequently consists solely of a cell of leaf parenchyma supported upon the summit of the axis. It is therefore formed of a double membrane (excepting the open radicular end), viz. the indented embryo-sac and the membrane of the pollen tube itself (fig. 12, 13.). I can from direct investigation refer for corroboration of this fact to the following species: *Taxus*, *Abies*, *Juniperus*, *Lathræa*, *Phormium tenax*, *Canna Sellowii*, *Œnothera crassipes*, *Mirabilis longiflora* and *Jalappa*, *Veronica serpyllifolia*, *Limnanthes Douglasii*, and less evidently in *Martynia diandra* and *Cynanchum nigrum*; on the other hand most beautifully clear in *Orchis Morio* and *latifolia*. In all these plants I have observed the entrance of the pollen-tube into the embryo-sac and the gradual conversion of its end directly into the embryo; and in *Taxus*, and very easily in *Orchis*, I was even able to withdraw that portion of the tube which represented the first stage of the embryo out of the embryo-sac and that indeed at a tolerably advanced period*.

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* As is the case in the regular and accidental *Polyembryonaiæ*, to the

The tracing of the pollen-tube into the interior of the embryo-sac is not so easy in all plants, because the cells of the nucleus which are arranged around the summit of the embryo-sac are very firm and opake, so that it and the pollen-tube cannot be exhibited quite free. In these cases, however, three circumstances speak for the identity of the embryo with the pollen-tube: 1st, the constantly equal diameter of the latter exterior to the embryo-sac and of the former just within it. 2nd, The invariable chemical similarity of their contents shown by the reactions produced on the application of water, oil of sweet almonds, iodine, sulphuric acid, and alkalies. The general contents of the grain of pollen granule is starch; and this either proceeds unchanged downwards through the pollen-tube or else passes along, being previously changed by a chemico-vital process into a transparent and colourless fluid, which becomes gradually more and more opake and is coagulable by the application of alcohol: out of this, by an organizing process, the cells are formed which fill the end of the pollen-tube, extending in *Orchis Morio* far be-

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latter of which the genus *Cynanchum* especially belongs. In the summer of 1835 I found *Cynanchum nigrum et fuscatum* to contain from two to five embryos in at least every third seed.

[* Our readers are requested to refer to p. 276 for a note omitted in the translation.—Edit.]

yond the ovule, and thus form the parenchyma of the embryo: but I should exceed the limits of this essay were I farther to follow up the formation of these cells. 3rd. Lastly, the identity of the embryo and the pollen-tube is farther supported by the fact, that in such plants as bear several embryos there are always precisely the same number of pollen-tubes present as we find embryos developed.

The most important result of these facts, and which I shall not now attempt to carry out in its full extent, but content myself with alluding to, is that the sexual classification hitherto adopted in botany is directly false. For if the ovulum be understood in physiology to represent that material foundation from which the new being becomes immediately developed, and if we term that portion of the organism in which this material commencement is deposited before it becomes developed *the female organ*, whilst that part which calls into action or promotes the development of the germ by means of its potential effects is termed the *male organ*, it is evident that the anther of the plant is nothing but a female ovarium and each grain of pollen the germ of a new individual. On the other hand, the embryo-sac only works potentially, determining the organization and development of the material foundation, and for this reason therefore ought to be termed a male principle, were we not to consider, perhaps more correctly, (without embarrassing ourselves with lame analogies taken from the animal kingdom) that the embryo-sac merely conveys new organizable fluids by means of transudation and thus only serves the office of nourishment*.

Secondly, the process of the development of the embryo, already described, easily establishes the fundamental unity of the Phanerogamia and those Cryptogamia in which the sporules are evident conversions of the cellular tissue of the foliaceous organs or leafy expansions, since the same part in both furnishes the groundwork of the new plant in both groups, and the only difference existing between the two is this;—in the Phanerogamia a previous formative process in the interior of the plant precedes the period of latent vegetation, whilst in the Cryptogamia the sporule (the grain of pollen) develops itself to a plant without previous preparation. Difficulties nevertheless occur here in the consideration of mosses and *Hepaticæ*, and more particularly in the enigmatical *Rhizocarpeæ*. It appears to me, however, that in this last-named family especially, there still remains much to be observed.

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* The embryo-sac retains this nourishing function in most of the albuminous seeds until a later period, that of germination, for nutriment accumulates in the cells, which gradually fill up the whole of the interior of the embryo-sac, which becoming afterwards converted into fluid is appropriated to the demands of the young plant. In the seeds which have a central albumen (the *embryo periphericus*), the albumen is merely a residuum of the *nucleus*, and the space which was occupied during the earlier stages by the embryo-sac is now entirely occupied, in the mature seed, by the embryo alone.

Lastly, this detailed process explains simply and naturally the formation of buds on leaves, although it so seldom occurs (whether it shows itself as a peculiarity of the plant or is an abnormal phænomenon), as being merely a partial retrogradation into a lower (cryptogamic) organization.

In closing this brief exposition I must subjoin a few remarks, partly for the purpose of anticipating unjust interpretations, and partly to afford a more correct apprehension of this essay.

In the first place, I am far from meaning to lay claim to all the views which have been developed in these pages as my own discoveries; I desire to give every one his due; and not laying so very great value on mere priority, I consider it much more honourable in founding a new view to extend it over the entire department of the science and render it insubvertible by patient investigation, than merely to be the discoverer of something new, in which good fortune so often plays the most conspicuous part. The narrow limits afforded to a memoir of this nature, and not any want of information as to what and how much various celebrated men had communicated to the public before me, has been the cause of my entering so slightly into the historical detail of all that has been done in this branch of science. These points, together with the perfect completion of my investigations, I withhold until the appearance of the work above alluded to, from which my only intention was to give here a small portion of the results.

On the other hand, I must remark, secondly, that everything which I have included here is the result of my own investigation, and I have not received the smallest point, even upon the best authority, without myself proving its correctness.

Thirdly and lastly, I must state that everything I have put forward is the result of actual observation, and that speculation (immediate consequence in its strict logical sense excepted) has not had the least share in these observations. Whatever of interest occurs that can lay claim to novelty has been known to me for years, but I postponed its publication in order to afford me time to take the utmost advantage of the numerous and valuable resources which were placed at my disposal in Berlin, in order to give my work such an extension that the results may not appear as isolated facts, but assume the shape of laws for the entire vegetable organism.

It is of course evident that I could subjoin but few drawings, necessary in explaining some of the most important points of my investigation; and I will only hope that by reason of this deficiency I have not become too frequently unintelligible.

I am only desirous of having such persons as judges of my work who have recourse to nature as umpire, and who have no other object in view than *Truth*, the only praiseworthy motive in scientific pursuits and which alone has been my guide in all my investigations; if by this I have been the means of contributing but a little to the cause of science, I shall consider myself as eminently fortunate.

> ——— si quid novisti rectius istis,
> Candidus imperti; si non, his utere mecum.

Appendix.—I have referred frequently in the course of the foregoing treatise to *Lathræa squamaria*, which I have done in preference to other plants on account of the clear and evident manner in which I have seen many parts in this. Now it has just met my eye that Unger (*Beiträge zur Kenntniss*, etc. *Ann. de Wiener Mus.*, vol. ii. p. 50,) denies the existence of cotyledons and radicle in the embryo of *Lathræa*; any one may therefore naturally object that I have selected but a poor subject as an example. I must, however, confess that I cannot comprehend Unger's assertion, for the embryo of *Lathræa* has such evident cotyledons that they may clearly be perceived with the help of a lens of from six to eight times magnifying power, and an acute observer may recognise them without the aid of a glass. The cotyledons are at least equally long with the other parts of the embryo, as they have been figured by Gærtner. I can scarcely imagine, I must confess, that Unger should have entirely overlooked the embryo and have taken the very firm albūmen for it. Generally speaking, the acotyledonous plants must not be understood as forming a third division in opposition to the monocotyledonous and dicotyledonous, and indeed the importance of this characteristic is very subordinate; it is a phænomenon which may occur in every sort of plant. The matter consists merely in the period of latent vegetation commencing somewhat earlier, whilst the completion of the embryo in the fruit only proceeds as far as the point, where it becomes of a globular shape; but the farther development passes over the fruit into the germination, as is the case in the entire family of the *Orchideæ*.

In page 51 Unger expresses his opinion that the *Orobancheæ* should be united to the *Labiatæ*; now the construction of the ovarium is precisely the distinctive character of the *Labiatæ*, and which is wanting in the *Orobancheæ*. On the other hand, *Lathræa* (which likewise possesses stomata) and *Orobanche* agree so completely with the *Scrophularineæ* in every respect excepting the habitus, solely to be ascribed to their locality, that I cannot find anything like a sufficient ground to keep them disunited. It would certainly not occur to any zoologist to separate an animal from its natural family merely because it was a parasite: wherefore then should it be otherwise with the vegetable kingdom*?

Explanation of the Engraving (*Plate* III.)

Fig. 1. A longitudinal section of the flower-bud of *Taxus bacca* (*femina*). *aa.* leaves. *b.* the rudiment of the second integument, which forms the berry. *c.* the first or inner integument. *d. nucleus.* I have represented the course of the epidermis by a fine line upon the ovule and the two interior leaves, as likewise in figs. 4, 18, 22, and 23.

Fig. 2. Longitudinal section of a very young pistil of *Salvia Clusii. a.* carpellary leaves. *b.* ovule. *c.* canal of the style.

Fig. 3. The inferior portion of a newly impregnated ovule of *Mirabilis longiflora*, also a longitudinal section. *a. funiculus. b.* remains of the *nucleus. c. integumentum simplex. d.* embryo-sac. *e.* pollen-tube, whose extremity expands to form the embryo. *f.* an abortive pollen-tube.

Fig. 4. Longitudinal section of a young ovule of *Polygonum orientale. a.* nucleus. *b.* protuberance out of which the *integumentum internum* is formed. *c.* commencement of the *integumentum externum.*

Fig. 5. A very minute ovule of *Goodyera procera. a. integument. extern. b. integum. intern. c.* remains of the *nucleus. d.* embryo-sac.

Fig. 6 and 7. Early conditions of the embryo of *Potamogeton lucens.*

Fig. 8. *Potamogeton heterophyllus* at a later period than the preceding. *a. plumula. b.* cotyledon which is still unclosed (*ungeschlossen*).

Fig. 9—11. Different grades of development of the embryo of *Echium vulgare. a.* embryo-sac. *b.* embryo.

Fig. 12. Summit of the embryo-sac of *Phormium tenax* with the embryo in course of development. *a.* embryo-sac. *b.* pollen-tube. *c.* embryo.

Fig. 13—17. Formation of the embryo of *Œnothera crassipes. a.* embryo-sac. *b.* pollen-tube. *c.* embryo. *d.* terminal shoots (*punct. vegetationis*, Wolff). *e.* cotyledon.

Fig. 18. Longitudinal section of the female flower of *Pinus abies*, from a cone about three quarters of an inch in length. *a.* carpellary leaf (bractea of R. Brown). *b.* placenta (open *ovarium* of R. Brown). *c. nucleus. d.* commencing integument (*cupula* auct.). *e.* embryo-sac. About this time the carpellary leaf has already acquired its green colour, but the *placenta* consists of colourless succulent cellular tissue.

Fig. 19—23. Different periods in the development of *Statice atropurpurea.* Fig. 19, interior of a very young bud. *a, a. stamina. b.* carpellary leaves. Fig. 20, the same at a somewhat later period. *a.* four carpellary leaves, still disunited. *b.* commencement of the formation of the ovule. *c.* base of the fifth carpellary leaf which has been cut off. Fig. 21, a recent ovule, in which the first tumefaction for the development of the inner integument is already evident. Fig. 22, longitudinal section of the same at a later period. The inner integument *a.* has already extended itself over the entire *nucleus b*, whilst the external integument *c.* is scarcely visible. Fig. 23, the same at a later period: *a, b, c,* as before.

Fig. 24. Longitudinal section of an ovule of *Lathræa squamaria* soon after its impregnation. *a. integumentum simplex. b.* remains of the *nucleus* (*membrana nuclei*, R. Brown). *c.* embryo-sac already filled with

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[* On this subject see Dr. Lindley's paper on the Botanical Affinities of *Orobanche*, in our last volume, p. 409.—Edit.]

cells. *d.* pollen-tubes. *e.* embryo. *f.* cæcal cavities of the embryo-sac in the parenchyma of the ovule. *g.* *funiculus.*

Fig. 25. Anther-cells of *Pinus abies* inclosing four pollen-forming cells.

Fig. 26. The same, after absorption of the parent cells: a grain of pollen may be perceived in each.

Fig. 27. The same after they have been immersed in water: two grains of pollen are just about to leave the cells; having burst the parietes.

Fig. 28. A single grain of pollen from the same.

Fig. 29. Two pollen-bearing cells of *Podostemon ceratophyllum.*

Fig. 30. Pollen of *Podostemon ceratophyllum* taken from the stigma, from one of which a pollen-tube already proceeds.

XXXVII. *Description of the Kauri or Cowdee Resin, from New Zealand; with Experiments in Relation to its Employment in the Arts.* *By* J. PRIDEAUX, *M. Plym. Instit., &c.*[*]

THE Kauri wood was noticed by Captain Cook as a very fine mast timber, and has since taken the attention of other navigators. Missionaries have been particularly struck with it, and attempts have been made to bring home cargoes of it to this country. These attempts are at last successful, and some cargoes have arrived for the dockyard here (Plymouth), fully bearing out the high reputation it had previously attained.

Mr. Yate[†] describes the tree under the name of "*Dammara australis*, or *Pinus Kauri*," as running from 85 to 95 feet high without a branch, and sometimes 12 feet diameter, yielding a log of heart timber 11 feet diameter. One he measured, perfectly sound, 40 feet 11 inches circumference.

The wood has much the appearance of deal, and works well under the plane, yielding a strong odour of the resin.

The appearance of the tree he describes as most majestic, raising its head far above the other trees of the forest, and crowned with the most splendid foliage; its leaves small and numerous, not unlike those of the English box.

From the trunk, he says, oozes a gum insoluble in water, and, he believes, in rectified spirit; also a kind of resin, answering the purpose of resin in ship-building: both having a strong resinous smell; the gum very fragrant, and chewed[‡] on that account by the natives. Both gum and resin diffuse themselves over the whole tree, the cone and leaf being equally tinctured with it, whilst it may be seen exuding from the tips of the leaves on the highest branches.

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[*] Communicated by the Author.

[†] Account of New Zealand, &c. Seeley and Burnside. London, 2nd edition, 1835. p. 36. [An account of the *Dammara Australis*, and a notice of its resin, will be found in Lambert's Genus *Pinus*, vol. ii. p. 65.—EDIT.]

[‡] A material brought home by Mr. George Bennett, to whom we are indebted for much information on the Oceanic Islands, and said to be used by the New Zealanders as a masticatory, under the name of *Mimika*, was put into my hands two or three years since by Lieut.-Col. Hamilton Smith.

The term "gum" appears to be here misapplied to a substance insoluble in water, and I suppose, with deference to Mr. Yate, that his distinction is unfounded; that the gum means the more recent exudation, which is white, opake, fragrant, and more or less compressible, from the presence of its essential oil; the resin that which has lost by time or exposure the essential oil and a little moisture, and thus become hard and transparent. A piece which was given to me twelve months since in the former state, had become yellow, hard, transparent, and almost inodorous before I repeated my examination of it on the present occasion; and in looking over several cwts. now in this port, I find it in every stage of the difference.

In Berzelius's *Traité de Chimie* (v. 501) is described a *Resine Dammara* lately introduced, as transparent, colourless or yellowish, insipid, inodorous, sp. gr. 1·097 to 1·123, very fusible, without any odour, dissolving partially in alcohol, almost entirely in æther, and completely in oil of turpentine and fat oils. Brandes found in it traces of gum and succinic acid, and two resins; one soluble in cold alcohol, amounting to 83·1 per cent., the remainder insoluble in that menstruum cold, but dissolving in it hot and precipitating in the form of a voluminous snow-like white powder. It will appear that this is not the same with kauri resin, although possessing considerable analogy with it.

Kauri resin (known here as Cowdee gum) is in pieces of various magnitude, from that of a nutmeg to a block of two or three cwts. Generally they are of irregular shape, with rough powdery surface, and often pieces of bark or occasionally even earthy matter attached; whilst some are shining, with a vitreous fracture. In colour they vary from milk-white to deep amber and even brown; the white having occasional transparent lines and patches; the yellow being generally transparent; and the brown sub-opake, apparently from impurity, and perhaps extractive matter. But bits may be found transparent and colourless, and every shade affords abundant examples of milky opacity. Its hardness is intermediate between that of copal and of resin, so that it cannot be scratched by the nail; the milky pieces more or less tough and elastic;

It is black, not quite hard, but friable, breaking like pitch, tasteless and inodorous. On subjecting it to chemical examination, I found it chiefly to consist of asphaltum, containing no kauri resin, as it dissolved entirely in cold oil of turpentine.

fracture bright vitreo-resinous. The white and milky pieces odorous, rather fragrant, somewhat resembling fine elemi; taste like the smell, and sweetish. One piece, indeed, had crevices, dividing it into laminæ, between which was compressed a subsaccharine pasty matter. Sp. gr. 1·04 to 1·06.

It is very inflammable, burns away with a clear bright flame, but does not drop.

By gentle heat it froths and swells, giving out water and aromatic oil, and becoming transparent; and on increasing the heat runs into a clammy fusion, but does not liquefy. After cooling it is transparent, and nearly as hard and tough as shell lac. This change may be effected without heating it enough to impair its whiteness, even by powdering and heating gently over the sand-bath, when it agglutinates as it gives off its essential oil, but becomes hard again on cooling.

To try its solubility, 20 grains, dried as above, were treated with the following liquids, each six times the quantity of the resin.

a. Cold water for twenty-four hours dissolved very little, but acquired its odour, taste, and sweetness; lost its transparency, and rendered the resin also opake.

b. Boiling water four hours on the sand-bath dissolved about 1 grain and became milky, but retained little of the odour and taste, the essential oil having passed off in the steam.

c. Rectified spirit of wine, twenty-four hours, cold, dissolved nearly one half, leaving the residue soft and elastic, like birdlime.

d. Alcohol (tartarised), four hours, at about 90°, dissolved the whole, but on cooling deposited the elastic matter as left by *c*; this dried on the sand-bath, till crisp and beginning to discolour, weighed 8½ grains.

e. Pyroacetic spirit acted little upon it in the cold, leaving the residue partly pulverulent, partly glutinous and elastic.

f. Pyroacetic spirit tartarised (rectified over carbonate of potass) digested four hours in a warm-water bath, dissolved it very partially, acquiring no consistence, becoming milky, but not coagulating when dropped into water, and burning with the same bluish transparent smokeless flame as the pure spirit. No other instance occurs to my recollection in which the distinction between alcohol and pyroacetic spirit is so marked; the latter leaves the kauri resin even when combined with shell lac or common resin.

g. Volatile oil of turpentine, twenty-four hours in the cold, rendered the resin voluminous, soft, and opake, but dissolved very little.

h. The same oil, in the water-bath four hours, dissolved nearly 9 grains; the residue not elastic, (as that of *c* and *d* from alcohol,) and when dry was partially soluble in cold anhydrous alcohol, the undissolved part then acquiring some elasticity.

i. The elastic deposit from alcohol (*c*, *d*) was digested in oil of turpentine on the water-bath, by which almost the whole was dissolved; a small portion separating when cold in a soft bulky jelly.

k. To another alcoholic portion, as *d*, was added one third of its volume of turpentine solution, as *h*, with the residuum, both being warm; the solutions mixed freely and clear, and the heat being continued other four hours, the residue completely dissolved. The solution retained its transparency and deposited nothing on cooling. Thus the kauri resin is completely soluble in four times and a half its weight of alcohol with one and a half its weight of oil of turpentine, and acts as a medium to combine the menstrua.

l. Pyroacetic spirit was tried with oil of turpentine in the same manner, but refused either to combine with the turpentine, or to act effectively on the resin.

m. Coal-tar naphtha dissolved the greater part readily in the cold, leaving a soft gelatinous residue, like caoutchouc digested in that liquid.

n. Linseed oil did not dissolve it by digestion, nor by gently heating together in an iron ladle. On increasing the heat, combination takes place, but not till the hardness and elasticity of the resin are destroyed (*v*). The case is not amended by combining either the oil or the resin previously with oil of turpentine.

It was stated above that this resin wants *fusibility*; for although it easily softens and agglutinates, it refuses to liquefy, and even burns away without dropping.

To try how far this property could be induced:

r. A portion of the powder was mixed with half its weight of volatile oil of turpentine and gradually heated in an iron ladle. The resin softened as usual; the oil acquired consistence; but the combination was only partial, though kept constantly stirring. The oil gradually evaporated, leaving the resin in its original clammy state, from which it did not change at any period of the experiment.

s. Another portion was mixed with half its weight of oil of turpentine, left in the cold for a night, and then digested six hours in a very gentle sand-bath. There were, as before, partial combinations between the mass of the resin and a part of the oil, and between the bulk of the oil and a part of the resin, but they always separated into a tough solid and a thickish liquid, however carefully mixed at every stage of the experiment; and the result in the iron ladle was as (*r*).

t. Coal-tar naphtha entered fully into combination with kauri resin in the cold; but on applying heat no fluidity could be produced, the naphtha exhaled gradually, leaving the resin tough and clammy all through the experiment.

v. Mixed with one-eighth its weight of linseed oil, and gradually heated in the ladle, combination took place, with frothing just as the resin began to discolour. The whole became liquid, and poured easily out of the ladle. When cold it dropped readily on being kindled, and melted liquid at a moderate heat, but its valuable properties were destroyed. It had become more tender and crisp than common resin. And the same result followed when the combination was very slowly effected, and with a smaller proportion of oil.

w. Tallow, which forms a remarkably clammy compound with common resin, was substituted for linseed oil, with the kauri. But making the composition with whatever care, and in whatever proportions, the results were equally unfavourable.

x. Wax answered no better than tallow or linseed oil.

Uses of Kauri Resin in the Arts.

1. From its hardness, fragrancy, and brilliancy the white parts seem well suited for varnish making, for which its solubility in alcohol gives it great advantage. Harder and more free from colour than mastic, quite as soluble, and at perhaps less than one-tenth the price, it seems to be an important addition to our materials for alcoholic varnishes. It may indeed come to be placed quite at their head. The alcoholic solution *d*, with one-fourth its measure of the turpentine solution *h*, is a very excellent spirit varnish, quite colourless, quick-drying, clear, and hard. The solution *k* requires more care in application, being liable to precipitation as the alcohol dries away in the cold, but in a warm dry place it lies well, and gives a fine surface. Its insolubility in pyroacetic spirit is, however, an unfortunate limit to its utility as a material for varnish.

2. Its hardness, fragrancy, and inflammability pointed it out as suitable for sealing-wax, for which purpose the experiments on its fusibility were instituted. But they have not been successful in adapting it, *per se*, to that purpose. Combined, however, with lac and turpentine it answers much better, and the manufacturer will soon ascertain the best proportions. After many small experiments, the most successful with me has been: Kauri, lac, each one ounce; resin three

quarters of an ounce, oil of turpentine half an ounce, vermilion one ounce. Powder together the lac, kauri, and resin; add the vermilion, and then the turpentine. Let them remain a few days in a well-covered vessel, then melt them together in a very gentle heat. The kauri will liquefy in this composition; it burns well, drops freely, and takes a fine impression. But it does not always adhere firmly to the paper; a very serious defect, of which the cause or remedy has not hitherto occurred to me. The same thing sometimes happens with sealing-wax made of the usual ingredients, but less frequently than with kauri.

Another purpose for which its brilliant inflammability and comparative infusibility qualify it, if it come in largely and cheaply, is gas light. A modification of the oil gas apparatus would work it, and the material being supposed at one-fourth the price of oil, whilst the original outlay in laying pipes, &c. would be in the same small proportion as for oil gas, it would stand a fair chance in competition with coal gas, and be much less disagreeable in dwelling-houses than any gas hitherto employed.

As to its officinal employment in medicine and surgery, time and experience only can indicate them. External application seems most suited for it; its masticatory employment is not very likely to be adopted in Europe.

The older transparent pieces do best for sealing-wax, for which their colour is not an objection. The recent white are most suitable for varnish, being first deprived of moisture by drying over a sand- or water-bath, when they become transparent, still remaining colourless.

XXXVIII. *On the Variation of the Arbitrary Constants in Mechanical Problems.* *By* J. W. LUBBOCK, *Esq., F.R.S.*

[Continued from vol. xi. p. 495.]

THE methods I employed with reference to the equations of motion expressed in terms of rectangular coordinates x, y, z, are very easily extended to the more general case when the equations of motion are in the well-known form

$$\frac{ds}{dt} + \frac{dU}{d\phi} + \frac{dR}{d\phi} = 0 \qquad T = \frac{x'^2 + y'^2 + z'^2}{2}$$

$$\frac{du}{dt} + \frac{dU}{d\psi} + \frac{dR}{d\psi} = 0 \qquad U = V - T$$

$$\frac{dv}{dt} + \frac{dU}{d\theta} + \frac{dR}{d\theta} = 0$$

$$s=\frac{d\,T}{d\,\phi'}=-\frac{d\,U}{d\,\phi'},\quad u=\frac{d\,T}{d\,\psi'}=-\frac{d\,U}{d\,\psi'},\quad v=\frac{d\,T}{d\,\theta'}=-\frac{d\,U}{d\,\theta'}$$

$$\phi'=\frac{d\,\phi}{d\,t},\quad \psi'=\frac{d\,\psi}{d\,t},\quad \theta'=\frac{d\,\theta}{d\,t}.$$

By similar steps to those which I employed in vol. xi. p. 494, it is easy to show that

$$\frac{d\,R}{d\,a}\,d\,t=(b,a)\,d\,b+(c,a)\,d\,c+(e,a)\,d\,e+\&c. \qquad \text{(I.)}$$

R not containing ϕ', ψ', or θ' explicitly, so that

$$\frac{d\,R}{d\,\phi'}=0 \qquad \frac{d\,R}{d\,\psi'}=0 \qquad \frac{d\,R}{d\,\theta'}=0$$

$$\frac{d\,R}{d\,a}\,d\,t=\frac{d\,R}{d\,\phi}\,\frac{d\,\phi}{d\,a}\,d\,t+\frac{d\,R}{d\,\psi}\,\frac{d\,\psi}{d\,a}\,d\,t+\frac{d\,R}{d\,\theta}\,\frac{d\,\theta}{d\,a}\,d\,t$$

(b, a) denotes the quantity

$$\frac{d\,\phi}{d\,b}\,\frac{d\,s}{d\,a}-\frac{d\,s}{d\,b}\,\frac{d\,\phi}{d\,a}+\&c. \qquad \text{Again,}$$

$$\frac{d\,U}{d\,a}=\frac{d\,U}{d\,\phi}\,\frac{d\,\phi}{d\,a}+\frac{d\,U}{d\,\phi'}\,\frac{d\,\phi'}{d\,a}+\&c.$$

$$=-\frac{d\,s}{d\,t}\,\frac{d\,\phi}{d\,a}+\frac{d\,U}{d\,\phi'}\,\frac{d\,\phi'}{d\,a}+\&c.$$

where I have omitted to write down the similar terms with respect to ψ and θ.

$$\frac{d^2\,U}{d\,a\,d\,b}=-\frac{d.\frac{d\,s}{d\,t}}{d\,b}\,\frac{d\,\phi}{d\,a}-\frac{d\,s}{d\,t}\,\frac{d^2\,\phi}{d\,a\,d\,b}+\frac{d.\frac{d\,U}{a\,\phi'}}{d\,b}\,\frac{d\,\phi'}{d\,a}.$$

$$+\frac{d\,U}{d\,\phi'}\,\frac{d^2\phi'}{d\,a\,d\,b}+\&c.$$

Similarly, by interchanging the letters a and b,

$$\frac{d}{d\,b\,d\,a}=-\frac{d\cdot\frac{d\,s}{d\,t}}{d\,a}\,\frac{d\,\phi}{d\,b}-\frac{d\,s}{d\,t}\,\frac{d^2\phi}{d\,b\,d\,a}+\frac{d\cdot\frac{d\,U}{d\,\phi'}}{d\,a}\,\frac{d\,\phi'}{d\,b}$$

$$+\frac{d\,U}{d\,\phi'}\,\frac{d^2\,\phi'}{d\,b\,d\,a}+\&c.$$

Subtracting this equation from the last,

$$0 = \frac{d\phi}{db}\,\frac{d\cdot\frac{d}{dt}}{da} - \frac{d\phi}{da}\,\frac{d\,\frac{d\,s}{dt}}{db} - \frac{d.\frac{dU}{d\phi'}}{da}\,\frac{d\phi'}{db}$$

$$+ \frac{d.\frac{dU}{d\phi'}}{db}\,\frac{d\phi'}{da} + \&c.$$

$$\frac{d\,(b,a)}{dt} = \frac{d\phi}{db}\,\frac{d.\frac{ds}{dt}}{da} - \frac{d\phi}{da}\,\frac{d\cdot\frac{ds}{dt}}{db} + \frac{ds}{da}\,\frac{d\phi'}{db}$$

$$- \frac{ds}{db}\,\frac{d\phi'}{da} + \&c.$$

$$= \frac{d\phi}{db}\,\frac{d.\frac{ds}{dt}}{da} - \frac{d\phi}{da}\,\frac{d.\frac{ds}{dt}}{db} - \frac{d.\frac{dU}{d\phi'}}{da}\,\frac{d\phi'}{db}$$

$$+ \frac{d.\frac{dU}{d\phi'}}{db}\,\frac{d\phi'}{da} + \&c.$$

Hence $\frac{d\,(b,a)}{dt} = 0, \quad (b,a) = \text{constant},$

as before, vol. xi. p. 495, when the more limited case was considered, the system being referred to rectangular coordinates x, y, z.

XXXIX. *Chemical Analysis of the Substance of the Electrical Apparatus of the Torpedo.* *By* M. CH. MATTEUCCI.*

OUR readers will find in the last Number an account of M. Matteucci's experiments on the Torpedo, translated from an extract by M. Becquerel in the *Comptes Rendus*; we have since then received the November number of the *Bibliothèque Universelle*, which contains the entire memoir, and for the completion of our former notice subjoin the following extract.

M. Matteucci states:—I analysed the substance of the organ of a moderate-sized torpedo after having removed all the membranes, muscles, and great nervous trunks which are attached to it. I commenced by determining the quantity of water it contained, and proceeded in the ordinary manner. In a first experiment I obtained from 1120 parts of the substance,

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* From the *Bibliothèque Universelle de Génève*, No. 23, Novembre 1837, translated by Mr. Francis.

104 of dried product; in a second experiment, from 1307 I obtained 136 dried parts. The mean quantity of water thus amounts to 903·4 parts in 1000 of the substance of the organ. The analysis of the dried product was made by treating it with alcohol at 36° and renewing this solution three times at intervals of twenty-four hours. The residue was then subjected to the same alcohol, but in a boiling state; this process was repeated twice. The remaining residue was then treated with boiling water and afterwards with concentrated acetic acid. The result is as follows: 6·65 grs. of the dried product gave

3·171 grs.	substance dissolved in cold alcohol	(A.)
0·893	——————— in boiling water	(B.)
2·587	substances insoluble in alcohol......	(C.)

The products A and B are composed of muriate of soda, of lactate of potash, of lactic acid, of Berzelius's extract of flesh, of phocenine, of a fatty substance analogous to the elaïne of the brain, and lastly, of a fatty substance solid at common temperature. The product C is formed almost entirely of albumen and some traces of gelatine.

When the solution obtained with cold alcohol is evaporated several crystalline layers are at first formed, subsequently some drops of a yellow oil; these sink to the bottom of the liquid. This liquid is very acid, and forms a precipitate with tincture of galls. On evaporating the whole of the solution there remains a yellowish-green mass, oily, very acid, and deliquescent. It dissolves almost entirely in water, forming a kind of emulsion. It disengages an odour of oil of rank fish. Potash dissolves the fatty substance, destroys the odour, and neutralizes the liquid; if tartaric acid be added the fatty acid is again formed, and gives by evaporation and distillation lactic acid and phocenic acid. The product of boiling alcohol also gives lactic acid and a solid fatty substance, which treated with nitric acid gives traces of sulphur and phosphorus. The substance insoluble in alcohol, boiled in distilled water, gives a dirty white solution, which becomes rather opake by the bichloride of mercury; tincture of galls produces a flocculent precipitate, which is partly dissolved on heating the liquid. Lastly, the residue is soluble, especially in the hot process, in acids, and in acid alkaline solutions. It is nothing but pure albumen*.

The albuminous substance which surrounds the brain differs from the substance of the electrical organ only by its greater quantity of water.

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* When the dried substance of the organ is treated three times with cold æther, and the solution evaporated, a green fatty matter of a pearly appearance is obtained, which dissolves sparingly in æther and cold alcohol;

It would be impossible for me not to point out the analogy which exists between the composition of the cerebral matter and that of the electrical organ of the torpedo which we have analysed.*

XL. *On a new Property of the Iodide of Silver.* *By* H. F. Talbot, *Esq., F.R.S.*†

It is well known that certain metallic oxides and salts have the property of changing their colour when heated, and recovering it again when cold.

The iodide of silver affords an extremely remarkable instance of this, and yet I believe the fact is not mentioned by any chemical author. I have no doubt, therefore, that a short notice of it may possess some interest.

Let a sheet of white paper be washed over with a solution of nitrate of silver, and afterwards with a *rather dilute* solution of hydriodate of potash. It will immediately assume a pale yellow tint, owing to the formation of iodide of silver. The paper may then be dried and laid aside for use.

When the property in question is to be exhibited the paper is held for some moments before a hot fire, and its colour changes from a pale primrose tint to a rich gaudy yellow emulating the sunflower.

Removed from the fire this bright colour gradually fades away, and in three or four seconds it is entirely gone. It may then be reproduced, and again destroyed as before, and so on for any number of times, for the heat causes no alteration in the substance experimented upon.

When the paper is warm and very yellow, if the finger is pressed upon it and quickly removed, it leaves a print or impression of its shape, which is nearly white. The cause of this is, that the finger is a much better conductor of caloric than the atmospheric air, and therefore cools the paper in an instant of time. Any cold substance may be substituted for the finger; and the effect can be produced at a little distance, without actually touching the paper, merely by the radiation

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it is void of taste, of a faint (*fade*) smell, and saponifies with potash; if burnt and calcined in a platina crucible, it leaves an acid cinder, and treated with boiling nitric acid it yields traces of the sulphuric and phosphoric acids. It is therefore cerebral stearine.

[* A partial chemical examination of the electrical organs of the Torpedo was made by Dr. J. Davy, and recorded in his paper in Phil. Trans. for 1832, p. 267, an abstract of which was given in Lond. and Edinb. Phil. Mag., vol. i. p. 67.—Edit.]

† Communicated by the Author

of the cold body. It appears therefore that this substance, from the peculiar suddenness with which it changes colour, is well adapted for experiments on the radiation and conduction of heat.

If now we throw some drops of ammonia on the paper it turns white, and if we hold it to the fire we find that it has lost the power of changing colour. Gradually however the ammonia evaporates, and then the alternations of colour occur as before. This seems to prove that the alkali enters into chemical combination with the iodide, and possibly the white substance is the double iodide of silver and ammonia. This opinion is confirmed by observing that potash and soda act in a similar manner, giving rise to *permanent* white compounds unchangeable by heat, which are probably the double iodides of silver and potassium, and silver and sodium.

This is the reason why the paper was directed to be washed with a *rather dilute* solution of hydriodate of potash; for if we use a concentrated solution the resulting tint is white, and the colour of the paper is not changeable by heat.

I have kept some pieces of this prepared paper for a year or two, and find that it still remains as sensitive to heat as ever.

XLI. *On Professor Sylvester's Analytical Development of Fresnel's Optical Theory of Crystals. By* John Tovey, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

As Mr. Sylvester's valuable analytical development of Fresnel's optical theory of crystals, published in your last and present volumes, is based upon those ideas of Fresnel which I conceive to be erroneous, I think I ought to show how it may be founded upon the laws which I have deduced in your 9th volume, (third series) p. 420, *et seq.*

For this purpose, then, let x', y', z' be the coordinates of a point in the line Oy (fig. 3, p. 428, vol. ix.), the distance of this point from O being unity; then $\cos x' O y = x'$, $\cos y' O y = y'$, $\cos z' O z = z'$.

$$v_{\prime\prime}^2 = c^2 z'^2 + c'^2 y'^2 + c''^2 x'^2,$$

$$x'^2 + y'^2 + z'^2 = 1.$$

Let $$l x' + m y' + n z' = 0 \qquad (a.)$$

be the equation to the wave-surface, Oy and Oz being the lines of vibration therein; and suppose the axes of x', y', z' to be the *axes of elasticity*. Now, since the expression for

$v_{\prime\prime}^2$ must, as I have shown, be a maximum or minimum, these equations give

$$c''^2 x' dx' + c'^2 y' dy' + c^2 z' dz' = 0,$$
$$x' dx' + y' dy' + z' dz' = 0,$$
$$l dx' + m dy' + n dz' = 0.$$

From the first and second of these equations we find

$$(c''^2 - c^2)\, x' dx' + (c'^2 - c^2)\, y' dy' = 0;$$

and from the second and third

$$(n x' - l z')\, dx' + (n y' - m z')\, dy' = 0.$$

Combining these two, we have

$$(c'^2 - c^2) . (n x' - l z')\, y' - (c''^2 - c^2) . (n y' - m z')\, x' = 0,$$

which, by reduction, becomes

$$\left(c'^2 - c''^2\right) \frac{n}{z'} + \left(c^2 - c'^2\right) \frac{l}{x'} + \left(c''^2 - c^2\right) \frac{m}{y'} = 0. \quad (b.)$$

The equations (*a*) and (*b*) are virtually the same as those which Mr. Sylvester has so denominated, and from them all his other equations are derived.

Between the results of my investigation and those of Fresnel's there is a difference sufficient to afford a criterion for deciding which of the two is correct. (See L. & E. Phil. Mag., vol. ix. p. 429.) If Fresnel's be correct the vibrations of rectilinearly polarized light must be perpendicular to the *plane of polarization*, so that in the undulation constituting the ordinary ray of an uniaxal crystal, the direction of the vibrations must be perpendicular to its *principal plane.* (See Airy's Tract on the Undulatory Theory, art. 100.) Whereas if my investigation be correct, the vibrations constituting the ordinary ray are parallel to the *principal plane* (L. & E. Phil. Mag., vol. ix. p. 424,) and consequently the vibrations in rectilinearly polarized light are parallel to the plane of polarization. Now as my theory not only agrees in this result with M. Cauchy's, (as I have remarked at p. 425, vol. ix.) but is also confirmed by Professor MacCullagh's theory of crystalline reflection, (L. & E. Phil. Mag., vol. x. p. 422,) I feel persuaded that it is the true one.

I am, Gentlemen, yours, &c.

Littlemoor, Clitheroe, Feb. 6, 1838. JOHN TOVEY.

P.S. In my paper in your January Number, p. 12, equations (7.), for n in both places, read $n_{\prime\prime}$; p. 13, line 21, for $(1 - \sin(n_{\prime} t - k x))$ read $(1 - \sin^2(n_{\prime} t - k x))$; and line 23, for $\frac{\eta}{a^2}$ read $\frac{\eta^2}{a_{\prime}^2}$.

A mathematical friend having suggested to me that the mode of arriving at the equations (7.), just mentioned, wants a little more explanation, I have reconsidered this part of the investigation, and find that I have left it obscure. The previous equations give for n^2 two values, denoted by $n_{\prime}^2$ and $n_{\prime\prime}^2$, and for ρ two corresponding values denoted by $\rho_{\prime}$ and $\rho_{\prime\prime}$. It follows, therefore, that either of the values of n, and the corresponding value of ρ, may be substituted for n and ρ in the expression (2.). But since the equations (1.) are of the first degree, they may be satisfied not only by the values of η and ζ corresponding to each value of n, but by taking for η and ζ the sums of these particular values, in which we may change the value of a as n changes. Hence the equations (1.) may be satisfied by the equations (7.). Compare Poisson, *Traité de Mécanique*, No. 546.

I hope Mr. Archibald Smith will communicate to your Journal that step of the analysis to which Mr. Sylvester alludes, at p. 78, as being wanted to make the development complete.

XLII. *On the Composition of certain Mineral Substances of Organic Origin. By* James F. W. Johnston, *M.A., F.R.SS. L. & E., F.G.S., Professor of Chemistry and Mineralogy, Durham.*[*]

1. *Middletonite.*

THE substance for which I propose the name of Middletonite, occurs about the middle of the Main coal or Haigh Moor seam, at the Middleton Collieries near Leeds. It presents itself sometimes in little round masses, seldom larger than a pea, but generally forms thin layers, rarely thicker than the sixteenth of an inch, interposed between the layers of coal. These layers vary in extent from two or three to probably twelve inches, and lie over each other at irregular intervals near the centre of the coal seam, which is here about five feet in thickness.

It is hard, brittle, easily scraped to powder by a knife, in small fragments is transparent, by reflected light of a reddish brown, by transmitted of a deep red colour, and gives a light-brown powder. It has a specific gravity of about 1·6, a resinous lustre, and is void of taste or smell. By exposure to the air for a length of time it blackens, and is then distinguishable from the mass of coal only by a slight peculiarity in the lustre, which it still retains. It is unaffected by heat

[*] Communicated by the Author.

of 400° Fahr. Thrown on a red cinder it burns like resin with much smoke, cakes, and leaves a bulky charcoal, which afterwards disappears without residue.

Alcohol, æther, and oil of turpentine boiled on the mineral in the state of powder, acquire a yellow tint, but on distillation the coloured solutions leave a mere trace of a dark-coloured friable resinous matter.

Heated in a close tube over the flame of a lamp it melts, blackens, and gives empyreumatic products. The residual charcoal burns away in the air with extreme slowness, leaving an almost inappreciable quantity of a white ash; 3·02 grs. left 0·005.

In boiling nitric acid it softens, melts, causes an emission of red fumes, and slowly disappears, giving a brown solution. From this solution on cooling a brown flocky matter falls, which is more fully precipitated by the addition of water. The yellow supernatant acid liquid gives no precipitate with acetate of lead or nitrate of mercury.

Concentrated sulphuric acid dissolves it in the cold, giving a dark-brown solution and evolving sulphurous acid.

Burned with oxide of copper,

4·35 grs. gave 13·6 grs. of carbonic acid, and 3·135 of water.
4·56 grs. gave 14·09 grs. of carbonic acid, and 3·295 of water.
5·18 grs. gave 16·265 grs. of carbonic acid, and 3·755 of water.

These results give the following for the constitution of the mineral:—

		1.	2.	3.
Carbon...	=	86·437	85·440	86·738
Hydrogen	=	8·007	8·029	8·046
Oxygen...	=	5·563	6·531	5·215
		100·007	100·	10

This agrees with

20 Carbon	=	15·2875	86·506
11 Hydrogen.........	=	1·3727	7·780
1 Oxygen............	=	1·0000	5·714
		17·6602	100·

and it may be represented by the rational formula

$$C_{20}\,H_{10} + HO.$$

This formula is analogous to that of Dumas and Peligot, $C_{10}\,H_8 + 3\,HO$, for the supposed hydrate of oil of turpentine.[*] Whether the rational formula above given, however, be the true one, the nature of the substance itself renders it very difficult to determine, while the want of a sufficient supply has prevented me from obtaining any proof that it is really a hydrate.

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[*] See Lond. and Edinb. Phil. Mag. vol. vii. p. 537.—Edit.

Durham, Feb. 5, 1838.

XLIII. *Notices respecting New Books.*

The Flora of Jamaica, by James Macfadyen. *Vol.* I., *containing* Ranunculaceæ—Leguminosæ. London, 1837. 8vo.

A Systematic account of the plants of this interesting island has long been wanting, and we are happy to find that the author has so well supplied this deficiency. All we formerly knew of the plants of this fertile region was scattered through the various and voluminous works of Sloane, Browne, Plumier, Swartz, and many others, and the only work of easy access, which could be of any comparative use to those interested in the Flora of this island, was the *Hortus Jamaicensis*, of Mr. Lunan, which according to Mr. Macfadyen's statement "scarcely comprised one half of the plants at present known to be indigenous to the island." A long residence has enabled the author to visit a considerable portion of the island, and he has thus had the opportunity of carefully studying the floras of the several districts, and of paying great attention to most of the plants in their various states of growth. The arrangement adopted is the natural system, which is undoubtedly the best suited, as such natural families combine individuals not only related to one another by coincident peculiarities of form and structure, but also by their medicinal properties. This, however, being not so well adapted for the tyro in botany, the author intends to give at the end of the natural system an arrangement of the genera according to the artificial system of Linnæus, in order to facilitate the progress of the young student. In the arrangement of the orders, the author has followed that of De Candolle as laid down in his *Prodromus Systematis Universalis Regni Vegetabilis*, as also for the definition of the genera. For the descriptions of the orders the works of Lindley and Richard have carefully been consulted. The author has also greatly added to the value of the work by his details of the history, medicinal properties, and general uses to which the various vegetable products of Jamaica are applied. We have selected one passage, the space to which we are limited not allowing of more, which will no doubt be of some interest to our readers.

"In the cultivation of the Indigo plant (*Indigofera tinctoria*), the best time, for ploughing or preparing the land, is immediately after the October rains. It has been found that sowing broad-cast succeeds better than in drills. A bushel of seed will plant from six to eight acres. In the course of a few days the young plants come up; soon after which they ought to be cleaned and moulded. As the plant grows wild in river courses and in dry gravelly situations, a soil of a similar character is found the best adapted for its cultivation. The rains ought also to be light and seasonable, and it is of importance that they should fall immediately after the young plants show themselves above ground, in order that they may be invigorated, and enabled to resist the attacks of the numerous insects to which they are, at this period of their growth, exposed. From this time little rain is required, except immediately after the branches have been cut; at these periods a shower is of great service, enabling the plants to send out new and vigorous shoots. A wet climate indeed is not at all suited to the cultivation of the Indigo. It is true that the plant may grow luxuriantly, but the juices are watery, and the produce obtained is small in quantity, and inferior in quality. Besides, as Indigo contains an immense proportion of carbon, and, as it is a well-established fact, in Vegetable Physiology, that it is not secreted by plants in the shade, but only when they are exposed to the direct influence of the sun's rays; it is evident, that Indigo requires much and continued sunshine to render its juices rich in this principle.

"The proper period for cutting the plant is previous to flowering. The leaves at this time change from a light to a dark green, and, according to the French Indigo planters, they crack when they are squeezed. It is of importance to determine the exact time when the plant comes to this state, since the branches, if they are prematurely cut, would be deficient in the quantity of the produce, and the quality would be inferior.

"The Indigo plant is retained in cultivation for a year, during which period it yields three or four cuttings. The Indigo obtained from the first cutting is the greatest in quantity, and is of the finest quality. The succeeding cuttings become gradually less productive, so that one part of the first yields as much as two parts of the second cutting.

"There are several methods employed in the manufacturing of Indigo. The 1st is styled the *fermenting process*, and is that which was formerly practised in this country, when Indigo was generally cultivated. The branches having been cut by means of a sickle, are placed, with the stalk upwards, in the steeping vat, till it is nearly three parts full. This vat is a large cistern of mason work or wood, about 16 feet square. It is then filled with water, and to prevent the branches from floating, they are kept down by means of rails loaded with planks. Soon after, the fermentation commences, and goes on till, in 24 hours, the contents of the vat are so hot, that the hand cannot be retained in it. The water gradually becomes opaque, and assumes a green colour; bubbles of carbonic acid gas are emitted, and a smell, resembling that of volatile alkali, is exhaled. When the fermentation has gone on sufficiently far, the liquor must be immediately let into the second cistern: for were it to be allowed to remain after a certain time in the fermenting vat, the pigment would be spoiled; and if, on the other hand, it were drawn off too soon, much of the Indigo would be lost. This second vat, which is lower than the first, is called the battery, and is commonly in size about 12 feet square, and 4½ feet deep. Here the liquor is agitated and beaten up, to perform which a variety of machines have been invented. The best adapted for the purpose is one with paddles, resembling those of a steam-boat, put in motion by means of a horse or mule. The effect of this agitation is, that the liquor will become as if curdled, and the indigo will be observed to separate into flakes. The manufacturer ascertains when the agitation is carried sufficiently far, by examining from time to time a small portion on a white soup plate. A quantity of lime water is now added, and the blue floccules are allowed to subside. The clear water is then drawn off by plugs placed at different heights in the cistern, and the sediment is drained in sieves made of horse-hair. It is after this put into coarse linen bags, and having remained for some time suspended in the shade, is subjected to pressure in order to get rid of as much of the moisture as possible. Lastly, the Indigo, having been converted into a stiff consistent mass, is cut into small squares, and allowed to dry in the shade.

"The 2nd method of manufacturing Indigo is known by the name of the *scalding process*. It appears to be a revival of the ancient Indian mode, as practised at Ambore, and described by Col. Martine in the third volume of the Asiatic Researches. He there mentions, that the natives boil the plant in earthen pots of 18 inches diameter, till the colouring matter has been extracted : it is then removed into larger jars, and agitated by means of a bamboo, until a granulation of the fecula takes place. A precipitant of red earth and water is then added, and the fecula is allowed to subside. The clear liquid is lastly drawn off, and the Indigo is dried in small bags suspended in the shade.

"The modern process is conducted on similar principles. Large coppers are about two-thirds filled with the branches of the Indigo, which are not to be pressed down. Cold water is then added to within a few inches of the brim, and the fire is lighted and kept up rather briskly, till the liquor acquires a deep green colour. During this part of the process, the mass must be constantly stirred, otherwise the bottom will be overscalded before the surface is ready. The fire is now to be withdrawn, and the liquor passed through a hair-cloth into the beating vat, where it must, while still hot, be agitated in the common way for half an hour. Lime water is now to be added, and after standing for about two hours and a half, the supernatant liquor, which is of a Madeira wine colour, is to be drawn off. The rest of the process is similar to that followed in preparing common fermented Indigo.

"The advantages of the scalding over the fermenting process, are, according to Dr. Roxburgh, that:—1. The produce is larger. 2. The health of the labourers is not endangered by the noxious effluvia, as is the case in the fermenting process. 3. Much less agitation, and very little precipitant is necessary. 4. The operation may be performed several times in the course of the day. 5. The Indigo dries quickly, without acquiring a bad smell. 6. Indigo so prepared has not the flinty appearance common to fermented Indigo, but in softness and levity is equal to Spanish *flora*.

"The 3d manner of manufacturing Indigo is called the *dry process*, and is that at present followed in the large factories in the southern provinces of India. It is described at great length by Charles H.

Weston, Esq., in the Quarterly Journal. According to this writer, the branches are cut early in the morning, and spread out in the sun. In the afternoon, the leaves are so dry, that they are easily separated from the branches by simply beating them with a stick. After this they are collected and closely packed in warehouses, and trodden down. As they are not immediately used, but are kept for some time, it is of importance that there be no dampness, as otherwise fermentation would ensue, and their value be destroyed. When the leaves have been kept about a month, their colour is found to have changed to a pale lead colour, which afterwards passes into black. It has been ascertained, that the maximum quantity of indigo is obtained when the leaves have acquired the lead colour, and that the colouring matter is only sparingly given by the fresh green leaves, or when they have passed to the opposite extreme, and acquired the black colour.

"After the leaves have been kept a sufficient time, they are transferred to the steeping vat, which is an uncovered reservoir, built of brick work, and lined with Roman cement, or stucco prepared from burnt shells, and filled with water. They remain there for two hours, and are every now and then turned; after which, the water having acquired a fine green colour, is run off, and passed through strainers into the beating vat. Two hours may appear to be a very short time for infusing the leaves. It has been found, however, that when the process is prolonged beyond this, a partial precipitation of the Indigo takes place.

"The liquor, when in the beating vat, is agitated by paddles for about two hours, during which the fine green colour gradually darkens, and acquires a blackish blue. As soon as this last hue appears, and the froth thrown up in beating becomes more or less white, and the incipient separation of the particles of Indigo can be detected, a certain proportion of lime water is well mixed with the liquor, and the whole is allowed to settle. In the course of three hours the indigo will have fallen to the bottom, and the supernatant liquid, which ought to be of a fine Madeira colour, is allowed to run off by means of cocks, placed at different heights. The indigo is, after this, conveyed into the covered part of the laboratory, where it is spread on strained cloth, and allowed to drain.

"On the following morning, the Indigo is put into a copper, with a quantity of hot water, and fire is applied. As the mass heatens, a quantity of scum rises, which is immediately removed, and, as soon as the whole is brought to the boiling point, the fire is withdrawn. The Indigo is then again taken to the strainers, and having been again drained, it is well worked with the hands, and afterwards subjected to pressure in square boxes, in order to get rid of as much moisture as possible. In this manner large square cakes, about 2½ inches in thickness, are formed, which are subsequently divided into smaller cakes, and allowed to dry gradually in the shade.

"The boiling process, although not generally adopted, is said to improve very considerably the quality, and enhance the value of the produce.

"A beautiful yellow precipitate may be obtained, by means of acetate of lead, from the Madeira-coloured liquid, drawn off in the beating vat. This is said, by Mr. Weston, to promise to supply a great desideratum—a permanent yellow dye. Experiments are, however, wanting to confirm this.

"Indigo in the prepared state is of a rich blue colour, which varies, however, in its shade in different specimens. When pure it is light and friable; tasteless, and almost devoid of smell; of a smooth fracture; insoluble in water or alcohol, but soluble in sulphuric and nitric acids. Some varieties, such as that known among the Spaniards by the name of *flora*, is lighter than water; and the lightest is generally the purest. The analysis of M. Chevreul gives, as the composition of Indigo, a blue colouring principle called *Indigotine*, a red resin, a greenish-red matter, united to the sub-carbonate of lime, alum, silica, oxyd of iron, and some other salts. According to Dr. Ure, the ultimate constituents of pure *Indigo-blue*, are—

Carbon,	71·37
Oxygen,	14·25
Azote,	10·00
Hydrogen,	4·38
	100·00

Indigo is frequently adulterated, by gummy, resinous, and earthy substances being added to it; and its weight and purity are also affected by using lime in excess as a precipitant. Dr. Bancroft proposed, as a test to ascertain the relative values of different specimens of Indigo, to dissolve equal portions of each in sulphuric acid, so as to form the mixture known by the name of *liquid blue*, and after diluting with a certain quantity of water, to compare the shades of colour possessed by the several mixtures.

"Indigo is the most valuable and permanent of all the dye-stuffs. It is also made use of by painters in water-colours.

"The method of preparing Indigo, and of applying it to the purposes of dyeing, appear to have been very early known in India. Dr. Bancroft* has shown that the *indicum* of Pliny (lib. xxxv. c. 6.) possessed similar properties with the modern Indigo. It would appear, by a passage in Caneparius, quoted by the same author, that, in the 15th century, the Venetians were in the habit of receiving Indigo from the East by the way of Alexandria. After the discovery of the passage to India by the Cape of Good Hope, the Dutch are supposed to have been the first, about the middle of the 16th century, to import it direct into Europe. It was long, however, ere it came into general use as a dye, and there appears to have existed against it a very unaccountable prejudice. It was considered to be a kind of stone, and was prohibited in England during the reign of Queen Elizabeth, and also in Saxony by the Elector, who described it in his edict as a corrosive substance, and fit food only for the devil. Soon after this its importance came to be understood, and the cultivation of the plants which yield it was introduced into the West Indies, and into Mexico, and followed up with such success, that the market of Europe was for a long time principally supplied from these countries. A large proportion was furnished by Jamaica, and the remains of Indigo works may now be met with in different parts of the country. In 1672, according to Edwards, there were 60 Indigo works, producing 50,000 lbs. annually. A tax, however, of 3*s*. 6*d*. per lb. having been imposed by the British parliament, the cultivation was soon after, in a great measure, abandoned; and although the duty was soon after removed, and a bounty of sixpence per lb. offered, if imported directly into Great Britain, still it never again became general, and at present, I am not aware that it is produced in any quantity, or that there is a single Indigo work, deserving the name, in the Island. In the East Indies, on the contrary, the cultivation of late years has rapidly increased, so as to supply 3-4ths of the Indigo for the European market.

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* Philosophy of Permanent Colours, vol. i. p. 242.

"It is to be hoped, as few articles give a more profitable return for the capital embarked, that its cultivation among us may be resumed, especially as, from the improvements in the manufacture, the unhealthy fermenting process, which was found so fatal to the labourers employed, may now be dispensed with. An attempt was made, some years ago, by the late Mr. Robert Gray, of St. George's, to introduce the cultivation on his own property in that parish; but he did not succeed, owing to the excessive rains which fall in that district during almost every period of the year. The like ill success, and from a similar cause, has attended an attempt lately made on Greenwich Hill estate, in Manchioneal. The result would be different were a proper choice of climate and soil observed, such as the plains of Vere or Liguanea, where the rains are occasional, and seldom heavy, and the soil light and open.

" The medicinal uses of the Indigo are few. A decoction of the root, used as a lotion, effectually destroys vermin, and is very generally employed for that purpose in the country. The juice of the young branches, mixed with honey, is recommended as an applica-for aphthæ of the mouth in children: and the Indigo, in powder, sprinkled over foul ulcers, is said to cleanse them. The disease in poultry, known by the name of *yaws*, is cured by the application of a solution of Indigo by means of a rag."

Mr. Macfadyen's work is executed with great industry and care, and contains every requisite which may justly be expected from a local flora, and certainly merits a place by the side of the best works of this kind.

The next volume will appear early this year (1838), and on the completion of the work it is proposed to commence a series of illustrations of such plants as are new or may not have been previously figured. We may remark, that a good map of the island, for illustrating the geographical distribution of the species, might advantageously be added.

XLIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 211.]

Nov. 30, 1837. AT the Anniversary Meeting of the Royal Society, Francis Baily, Esq., Vice-President and Treasurer, in the chair,

The Address of His Royal Highness the President to the Meeting, was read from the Chair; from which the following are extracts.

I proceed to notice some of the more important events connected with the administration of the Royal Society during the last year.

One of the Royal Medals has been adjudged to Mr. Whewell for his very valuable series of Researches on the Tides, which have been published in our Transactions, chiefly during the last three years. I must refer you, Gentlemen, for a statement of the grounds upon which this decision has been founded to the more detailed reports of the Council, which will be read to you by your Secretary Dr. Roget; but I gladly avail myself of this opportunity of expressing my respect for the great talents and varied attainments of the distinguished philosopher upon whom this mark of honour has been conferred. If I regard him as occupied with the highest and most important practical duties connected with our system of academical education, and in providing and arranging the materials by which it is conducted, or the principles upon which it should be based, he will be found in the foremost rank of those whose labours do not deserve the less honour because they commonly absorb the entire time and attention of those who are engaged in them, and thus close up the avenue to those distinctions which are almost exclusively confined to great discoveries in science, or to important productions in literature. When I read his essays on the architecture of the middle ages, on subjects of general literature, or on moral and metaphysical philosophy, exhibiting powers of mind so various in their application and so refined and cultivated in their character, I feel inclined to forget the profound historian of science in the accomplished man ot letters, or the learned amateur of art; but it is in his last and highest vocation, whilst tracing the causes which have advanced or checked the progress of the inductive sciences from the first dawn of philosophy in Greece to their mature development in the nineteenth century, or in pointing out the marks of design of an all-wise and all-powerful Providence in the greatest of those works and operations of nature which our senses or our knowledge can comprehend or explain, that I recognise the productions of one of those superior minds which are accustomed to exercise a powerful and lasting influence upon the intellectual character and speculations of the age in which they flourish.

It is now three years since the Royal Medal was adjudged to Mr. Lubbock for his Researches on the Tides; and the Council have availed themselves of the first opportunity which was presented by the recurrence of the cycle of the subjects which are successively entitled to the Royal Medals, to make a similar award to his colleague and fellow-labourer in this very interesting and important series of investigations. It is not for me to attempt to balance the relative claims and merits, in connection with this subject, of these two very eminent philosophers; it is quite sufficient to remark that the first who ventured to approach this difficult and long-neglected inquiry was the first also who was selected for honour: but I have long noticed with equal pride and satisfaction the perfect harmony with which they have carried on their co-ordinate labours, apparently indifferent to every object but the attainment of truth, and altogether superior to those jealousies which too frequently present themselves amongst rival and contemporaneous labourers in the same departments of science.

I regret to observe that the second Royal Medal for the present year has not been awarded, and that it has consequently lapsed to the Executors of his late Majesty. It was proposed that it should be given to the best Memoir presented to the Royal Society between the years 1834 and 1837, containing "Contributions towards a System of Geological Chronology, founded upon an examination of Fossil Remains and their attendant Phænomena;" a subject of the greatest interest, and also of the greatest delicacy, from its connexion with those agitating topics which the speculations of philosophers are compelled to approach, though they may not always venture to decide. I should have rejoiced to have seen in the Transactions of the Royal Society a record of the opinions of a Buckland or a Sedgwick upon a theme which is so worthy of the application of their highest powers; and I trust that, though its announcement as a Prize Question has failed to secure, within the prescribed period, the accomplishment of the object proposed by it, it will still have done some service to the cause of science by exciting the attention of geologists in such a manner as may sooner or later lead to a definite and philosophical exposition of their views on a subject of so much importance.

Those who have attended to the Tidal researches of Mr. Whewell must be aware how much light has been thrown upon the character and course of the phænomena of the tides by the simultaneous observations, under his instructions, which were made in the month of June, 1834 and 1835, at nearly five hundred stations of the Coast Guard Service in Great Britain and Ireland, and simultaneously with the latter also at more than one hundred stations in America, Spain, Portugal, France, Belgium, Holland, Denmark, and Norway. These observations were undertaken by the authority or through the influence of the Government of this country, which likewise most promptly and liberally furnished the requisite funds and assistance for reducing the observations in such a manner as was requisite for deducing general conclusions from them, a labour much too extensive and costly to be undertaken by any single individual. I gladly seize this opportunity of bearing testimony, occupying as I do the highest scientific station in this country, to the readiness which the Lords of the Treasury and the Admiralty have shown on this and on every other occasion to forward scientific inquiries, and particularly such as are connected with the advancement of astronomy and navigation. They have granted funds for reducing and publishing the Planetary Observations at Greenwich, the valuable and extensive series of observations of the late Mr. Groombridge, for repeating upon an adequate scale the very important experiments of Mr. Cavendish, and for many other subjects of great scientific interest and value; and I feel satisfied that every application for assistance towards the accomplishment of any important object in science, will receive from them the most willing attention and support, if it comes before them with the recommendation and authority of those persons who are most competent to judge of its usefulness or necessity, and in such a form as may justify them in appealing to Parliament for its sanction of the requisite expenditure. I rejoice, Gentlemen, in such manifestations of the sympathy of the Government of this great country for the progress of science, and I trust that its influence will be felt in the cordial union and co-operation of philosophers in planning and in executing those great systems of observations, whether simultaneous or not, which are still requisite to fill up some of those blank spaces which occupy so large a portion in the map of human knowledge.

In the course of last year the celebrated Baron de Humboldt addressed a letter to me, as President of the Royal Society, expressing a wish that Magnetical Observatories, upon a uniform plan, might be established in this country and its colonies, with a view of making simultaneous observations with those which are now making, or which are in progress to be made, in different parts of the continent of Europe and of Northern Asia.* I felt it to be due to the illustrious author of this communication to make it generally known to the Fellows of the Royal Society, and to beg that a committee of the Council might be appointed to consider the best mode of carrying its recommendations into effect. A very elaborate Report was consequently made by the Astronomer Royal and Mr. Christie in November last, enumerating many important consequences which might result from such a system of observations, and pointing out a series of stations where they might most efficiently be made. I am happy to inform you, Gentlemen, that measures are in progress for the accomplishment of all these objects: a Magnetical Observatory, which was long contemplated and earnestly recommended by the Board of Visitors of the Royal Observatory, has been established at Greenwich, in a situation so remote from all other buildings as to be altogether free even from the suspicion of external disturbances. The Corps of Royal Engineers, which has always been distinguished for the zeal and scientific acquirements of many of its Members, has spontaneously offered to conduct the requisite observations, in whatever quarter of the globe they may be stationed; the Astronomer Royal has determined the species of observations to be made, and the character and construction of the instruments to be used; and the Lords of the Treasury have placed at the disposal of the Royal Society the requisite funds for their purchase. I have felt it my duty, Gentlemen, to bring these circumstances under your notice, not merely as forming an important part of the proceedings of the Council of the Royal Society during the last year, but as an encouraging and instructive example of the facility with which extensive co-operation and assistance may be obtained in the execution of any scientific object, however extensive it may be, when the practical means for performing it are distinctly and clearly defined.

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* A translation of Baron de Humboldt's letter to the President will be found in Lond. and Edinb. Phil. Mag., vol. ix, p. 42.

The Society has lost during the last year twenty-nine Members on the Home, and two on the Foreign List, and I shall now proceed to notice some of the most distinguished names which appear amongst them.

Henry Thomas Colebrooke was the son of Sir George Colebrooke, an eminent Director of the East India Company, under whose auspices he proceeded to India, as a writer, in 1782. Though a severe student in youth, and strongly disposed to follow a learned profession at home, he gave no indications for many years after his arrival in India of those tastes for severe and abstract studies for which he was afterwards so celebrated; and we consequently find that, whilst resident at Purneah, he devoted much of his time to the wild and animating field-sports of the East, for which he long retained a passionate fondness. He made his first appearance as an author in 1792, in a Treatise on the Agriculture and Commerce of Bengal; and it was about this period that he began, with all the ardour and energy which distinguished his character, the study of the Sanscrit language, chiefly with a view to acquire a knowledge of the Lilawati and other Sanscrit treatises on Algebra and Astronomy, which the somewhat extravagant speculations of Bailly and others had begun to bring into notice. He subsequently undertook the translation of the Digest of the Hindu Laws of Contracts and Successions, which had been compiled under the direction of Sir William Jones, a most laborious and difficult task, which he completed in less than two years. It was during his engagement on this work that he was appointed to a judicial situation at Mirzapore, a position singularly suited to his tastes and pursuits, from its vicinity to Benares, the great repository of the ancient treasures of the literature of Hindostan, and the place of residence of its most learned expounders.

In the year 1800 he was removed to Calcutta, and raised to the highest judicial situation in the native courts of India, at the same time that he was made President of the Board of Revenue, Member of the Supreme Council, and Honorary Professor of Sanscrit in the College of Fort William. But the important official duties which he was thus called upon to discharge seem rather to have stimulated, than to have checked, his labours and investigations in oriental literature and oriental science. In the course of a few years there appeared from his pen many profound dissertations in the Asiatic Researches, on the Vedanta System of Philosophy, on Sanscrit and Pracrit Poetry and Grammar, on the Indian Classes, on the Origin and Tenets of the Mahometan Sects, on the Jains, on the Indian and Arabian Division of the Signs of the Zodiac, and on the Notions of the Hindu Astronomers on the Precession of the Equinoxes and the Motions of the Planets; to which must be added the first volume of a very elaborate Sanscrit Grammar, the translation of the Peostra, a Sanscrit Dictionary, and two extensive Treatises on the Hindu Law of Inheritance, together with editions of the Amera Cosha, a Sanscrit Vocabulary, and of the Hitópadésá, or "Salutary Instruction", which had been translated by Mr. Wilkins, and which is more commonly known under the name of the "Fables of Pilpay".

It was some time after Mr. Colebrooke's return to this country that he published, in 1817, a translation of the Lilawati and Vija-Ganita, Sanscrit treatises on arithmetic, algebra and mensuration, to which was prefixed a dissertation on the early history of algebra and arithmetic in India, Arabia and Italy, which is equally remarkable for its profound knowledge of Hindu and Arabian literature and its correct views of the relations of oriental and ancient and modern European science. He was also the first person who maintained, from his own observations on the plains of Hindostan, the superior elevation of the Himalayan mountains above the Andes of America, in opposition to the opinions generally entertained at that period, and which had been sanctioned by the great authority of Humboldt's theory of the range of the curve of perpetual congelation. The complete confirmation which his opinion afterwards received, from accurate barometrical and trigonometrical measurements, was always referred to, in his later years, with particular satisfaction and triumph.

Mr. Colebrooke continued the steady pursuit of his oriental and scientific studies until nearly the close of his life, and even when the progress of his infirmities confined him almost entirely to his bed. He was one of the founders of the Asiatic and Astronomical Societies, and a short time before his death he gave to the library of the India House his incomparable collection of Sanscrit and Asiatic manuscripts, which had been collected at an expense of nearly 10,000*l.*, with the noble view of preserving them for ever from the danger of dispersion by the fluctuating accidents of inheritance.

Mr. Colebrooke was probably, with one single exception, the greatest Sanscrit scholar of his age; and when we take into account his great acquirements in mathematics and philosophy, and in almost every branch of literature, combined with the most accurate and severe judgement, and also his great public services in situations of the highest trust and responsibility, we shall not hesitate to pronounce him one of the most illustrious of that extraordinary succession of great men who have adorned the annals of our Indian empire, the deaths of so many of whom it has been my misfortune to record in my recent addresses from this chair.

Dr. John Latham reached the extraordinary age of ninety-seven years, having enjoyed the full possession of his faculties and almost unbroken health until within a few days of his death: he was the father of the Royal and Antiquarian Societies, and it is sixty-seven years since his first paper, on a medical subject, was published in our Transactions. He was the author of many papers on antiquarian subjects; but his favourite study throughout life was natural history, and particularly ornithology. He published, in 1781, his General Synopsis of Birds, in six volumes quarto, and afterwards two supplementary volumes. In 1792 he published his Index Ornithologicus, a complete system of ornithology, arranged in classes, orders, genera and species, in two volumes quarto. At the age of 82, he commenced his General History of Birds, a magnificent work in eleven volumes quarto. He was a man of very systematic habits and most amiable character, the tranquil course of whose long life was neither disturbed by scientific or professional jealousies, nor embittered by the want of those enjoyments which competence and domestic happiness and virtue alone can confer.

Dr. Tiarks was born at Jever in Oldenburg, and came to England in 1810, when he was appointed Assistant-Librarian to Sir Joseph Banks, through whose influence he was nominated Astronomer to the Commission for settling the North American Boundary, under the authority of the Treaty of Ghent. After his return to England, in 1822, he was commissioned by the Admiralty, at the request of the Board of Longitude, to ascertain, by means of a great number of chronometers, the difference of the longitudes of Falmouth and Madeira, and subsequently of Falmouth and Dover, the results of which were detailed in a very able paper in our Transactions for 1824, in which he pointed out and explained the origin of an error of nearly 4″ of time in the longitudes of all the stations of the Trigonometrical Survey*. He was afterwards sent on a similar mission to Heligoland and various stations in the North Seas, and on the last occasion he was accompanied by Sir Humphry Davy, who wished to try the effect of his protectors on the corrosion of the copper sheathing of ships. In 1825 he was recalled from Germany to resume his astronomical surveys in America, where he was employed to ascertain the position and extent of the north-western boundary of the Lake of the Woods, an operation in the execution of which both he and the party who assisted him suffered the greatest hardships and privations. He published various reports of his surveys, and was necessarily much employed and consulted in the difficult and embarrassing negotiations which have attended, and unhappily still attend, the settlement of the important question of the North American boundaries. Dr. Tiarks died in the forty-eighth year of his age, at his native place, in consequence of a fever which attacked a constitution already shattered and broken by the severe labours and privations which he had endured. He was a mathematician of no inconsiderable attainments, a very careful and efficient practical astronomer, and admirably qualified for the very important and responsible duties which he was appointed to discharge.

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* See Phil. Mag., First Series, vol. lxiii. p. 66, 378.

Dr. Edward Turner was a native of Jamaica, and studied medicine at Edinburgh, and chemistry at Göttingen under the instructions of the celebrated analytic chemist Stromeyer. He became a lecturer on chemistry at Edinburgh in 1824, and his first publication was a short introduction to the study of the laws of chemical combination and the atomic theory. He obtained the Professorship of Chemistry in the London University at its first establishment in 1828, a situation which he continued to hold to the end of his life. His Elements of Chemistry have enjoyed an uncommon degree of popularity, and are remarkable for clearness and precision both in the description of his experiments and in the deduction of his theory*. He was the author of two papers in our Transactions; the first "On the Composition of the Chloride of Barium," and the second containing "Researches on Atomic Weights," both written with a view of impugning the theory which had been promulgated by some English chemists of high authority, "that all atomic weights are simple multiples of that of hydrogen†." In the year 1835 Dr. Turner was compelled by the declining state of his health to suspend all original researches, confining himself simply to the duties of his professorship; and he died in February last, in the fortieth year of his age, to the deep regret of every friend of the progress of chemical science. He was a person of most engaging manners and appearance and of most amiable character; and his body was followed to the grave, with every manifestation of respect and affectionate attachment, by the whole body of the pupils and professors of the institution of which he had so long been a principal ornament.

Dr. William Ritchie was originally Rector of the Royal Academy of Tain in Inverness-shire, where he contrived, by extreme frugality, to save a sufficient sum from his very small annual stipend to attend a course of the lectures of Thenard, Gay-Lussac, and Biot at Paris, and also to provide a substitute for the performance of his duties during his temporary absence from Scotland. His skill and originality in devising and performing experiments with the most simple materials, in illustration of various disputed points of natural philosophy, attracted the attention of the distinguished philosophers whose occasional pupil he had become: he had also communicated, through Sir John Herschel, who took a strong interest in his fortunes, to the Royal Society, papers "On a new Photometer," "On a new form of the Differential Thermometer," and "On the Permeability of transparent Screens of extreme tenuity by Radiant Heat," which led to his appointment, through the recommendation of Major Sabine, to the Professorship of Natural Philosophy at the Royal Institution, where he delivered a course of probationary lectures in the spring of 1829: he became, from this time, a permanent resident in London, and was appointed to the Professorship of Natural Philosophy at the London University in 1832. He subsequently communicated to the Royal Society, papers "On the Elasticity of Threads of Glass, and the application of this property to Torsion Balances;" and also various experimental researches on the electric and chemical theories of galvanism, on electro-magnetism and voltaic electricity, which are more remarkable for the practical ingenuity manifested in the contrivance and execution of the experiments, than for the influence of the views which they display on the progress of their theory, which was so fully and so happily developed by the contemporary labours of another illustrious chemist and philosopher. Dr. Ritchie was subsequently engaged in experiments, on an extensive scale, on the manufacture of glass for optical purposes, for the examination of the results of which a Commission was appointed by the Government, with a view to their further prosecution by a public grant of money, or by affording increased facilities of experiment by a relaxation of the regulations of the Excise. A telescope of 8 inches aperture was made by Mr. Dollond from Dr. Ritchie's glass, at the recommendation of this commission; but it is generally understood that its performance was not so satisfactory as to sanction a further expenditure in the extension of these experiments. Dr. Ritchie died in the autumn of the present year, of a fever caught in Scotland; and though the traces of an imperfect and irregular education are but too manifest in most of his theoretical researches, yet he must always be regarded as an experimenter of great ingenuity and merit, and as a remarkable example of the acquisition of a very extensive knowledge of philosophy under difficulties and privations which would have arrested the progress of any person of less ardour and determination of character*.

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* This work was reviewed in Phil. Mag. and Annals, N.S., vol. i. p. 379.

† Dr. Turner's paper on the chloride of barium was given in Phil. Mag. and Annals, N.S., vol. viii. p. 180; and abstracts of his Researches on atomic weights in L. & E. Phil. Mag., vol. i. p. 109; iii. p. 448.

Mr. Joseph Sabine was educated in the University of Dublin, and devoted himself, from a very early period of life, to the study of botany, ornithology, and other branches of natural history, to the neglect of those professional studies which his friends designed him to pursue. One of his earliest labours was the formation of a collection of British birds of almost unrivalled extent and completeness. He became secretary to the Horticultural Society at the period of its first establishment; and though his connection with it was afterwards very abruptly and perhaps very harshly terminated, he must always be considered as the chief author of its successful and complete development. To the Horticultural Transactions he contributed 64 papers, the most important of which are those on the genera *Crocus*, *Dahlia*, and *Chrysanthemum*; and he was also required to re-write the greatest part of the communications which were addressed to the Society by gardeners and practical men, which were rarely sent in a fit state for publication, but which frequently embodied very important information on the various processes of horticulture.

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* Abstracts of Dr. Ritchie's papers read before the Royal Society will be found in Phil. Mag. and Annals, N.S., vol. vi. p. 52; viii. 58; x. 226; xi. 448; and L. & E. Phil. Mag., vol. iii. pp. 37, 145; x. 220; xi. 192. Papers communicated by him to the Philosophical Magazine have appeared in nearly every volume of the present series.

Mr. Sabine was likewise an active and valuable member of the Zoological Society, whose gardens are chiefly indebted to his taste and knowledge for the introduction and systematic arrangement of those splendid flowers and shrubs which have added so greatly to their beauty and interest.

Mr. Sabine held, for the greatest part of his life, the situation of Inspector-General of Taxes, and was called upon by his official duties to make periodical visits to almost every part of the kingdom; he never omitted any opportunity which his various journeys afforded him, of acquiring or of communicating practical knowledge of horticulture and of botany; and few persons have contributed so much, by their personal exertions, to add to the decorations of the cottage and the park, to increase and improve the produce of our gardens, and thus greatly to extend the sphere of the innocent enjoyments and luxuries of all classes of society.

The Rev. Dr. Joseph Hallett Batten was a native of Penzance in Cornwall, and was elected a Fellow of Trinity College, Cambridge, in 1801, after attaining very high academical honours. He was appointed Classical Professor at the East India College at Hayleybury at the period of its first establishment, and became Principal of the college upon the retirement of Dr. Henley, a situation which he continued to retain until within a month of his death. He was a man of cultivated taste and of very extensive attainments, both in theology and general literature; and in every way worthy, by his intellectual powers and character, of presiding over an establishment which has been so justly distinguished by the very eminent men who have been, and now are, connected with it.

Dr. John Johnstone was the sixth son of the celebrated Dr. James Johnstone of Worcester, and received his education at Merton College, Oxford. He was for upwards of forty years a very distinguished physician at Birmingham and its neighbourhood, and made his first appearance as an author in a defence of his father's claim to the first discovery of the disinfecting powers of muriatic acid gas, which had been claimed by Dr. Carmichael Smyth. Though earnestly attached to the study and practice of his profession, he retained throughout life a fondness for classical literature, and lived on the most intimate terms with some of the most distinguished scholars of the age, including amongst their number the justly celebrated Dr. Parr, whose life and voluminous correspondence he published, a work full of interesting literary anecdote and classical research; and his Harveian oration, pronounced in 1819, and which has been recently published, with a short memoir of his life, by his friend the Bishop of Lichfield, is a model of spirited and correct Latinity. Dr. Johnstone was a man of very warm affections and of great independence of character, and he was universally respected in the great manufacturing city in which he resided, for his great professional skill and services, and for the active support which he gave to every benevolent and useful institution.

Sir John Soane received his early architectural education under Mr. Dance and Mr. D. Holland, and was afterwards sent, by the especial bounty of King George the Third, as a student of the Royal Academy, to pursue his professional studies at Rome. After his return he gradually obtained extensive employment, both as an architect and a surveyor, and finally succeeded in securing almost every important and honourable appointment which is connected with the exercise of his profession in this country. In later life, when in possession of an ample fortune and public honours, he became a most munificent patron of public institutions, and more particularly of those which are connected with the advancement of the fine arts; and in 1835 he bequeathed his house in Lincoln's Inn Fields, and the magnificent collection of works of art which it contained, to the nation, and secured the accomplishment of this noble project by an Act of Parliament; he continued to pursue his usual course of public munificence until his death, which took place on the 20th of January last, in the 84th year of his age.

Sir John Soane was profoundly acquainted with the great principles of his art, and many of the interiors as well as exteriors of his buildings are remarkable for skilful construction and for rich and harmonious effects; but he was unfortunately disposed, in some cases, to seek for novelty rather in new forms and decorations of architectural members, than for originality in the combination of those which have been sanctioned by the concurrent voice of the most cultivated of ancient nations and the greatest masters of modern art; it is for this reason that many of his works appear somewhat capricious and extravagant, and fail to produce that undefinable feeling of pleasure and satisfaction which always attends the contemplation of those great productions of architecture which have been celebrated for correct proportions, or for beautiful and appropriate decoration.

In connexion with this distinguished professor and patron of art, I feel myself called upon to allude to the name of the venerable Earl of Egremont, whose very recent loss we have to deplore. He was a nobleman distinguished by his active yet discriminating benevolence, and by his princely use of a princely fortune; but it is as a judge and patron of art that his loss will be most severely felt beyond the precincts of his own family and the numerous poor who were the immediate partakers of his bounty. He was equally judicious in the selection of subjects for artists to execute, and liberal in rewarding them when done.

Mr. J. D. Broughton, Surgeon of the Life Guards, had served with great distinction as a medical officer during a great part of the Peninsular war and at Waterloo. He was an eminent physiologist, and devoted a great portion of his time and attention to the study and improvement of the science of medical jurisprudence, and more particularly to experiments on the effects of poisons, and to the best and most unerring tests for detecting their presence after death. His death, which followed a serious operation, rendered necessary by a long-neglected accident, was deeply lamented by a large circle of friends, by whom he was equally respected and beloved for his great professional talents and for his honourable character.

Mr. John Davidson, the last known victim to the cause of African discovery, was formerly a partner in the house of Messrs. Savory and Moore, the well-known chemists, but was induced to quit it in 1826, partly with a view to gratify his passion for foreign travel, and partly from other causes. He afterwards visited North and South America, India, Palestine, Turkey, Greece, Italy, Germany, and France; and the lectures which he gave at the Royal Institution and elsewhere, after his return, on the pyramids of Memphis and Mexico, on Thebes and the temples of Egypt and Jerusalem, afforded a sufficient proof both of his activity and of his accurate observation. The spirit of enterprise and travels, when once excited, is not easily allayed, and Mr. Davidson devoted himself, almost from the period of his return to this country, to a course of preparation for a journey to Timbuctoo, which had already proved fatal to so many adventurers. He was accompanied on this journey by Abu-Bekr, an enfranchised African slave, who had been a prince in his own country when young, and was well acquainted with the Arabic language. He had penetrated from Wadnoon to within twenty-five days' journey of Timbuctoo, when he was murdered by the El Hareb tribe, who were suspected to have been hired for that purpose by Moorish merchants, who, from not being able to understand or conceive the real motives of such an undertaking, conceived that its success would be injurious to their interests. Mr. Davidson was a man of great activity and strength, in the full vigour of life and health, and able to endure the severest labours and privations; but personal accomplishments the most calculated to secure success in ordinary attempts of this nature, serve only to augment the suspicion and to stimulate the cruelty of those savage tribes, who tyrannize over these inhospitable and almost impenetrable regions, and who are described by his companion, Abu-Bekr, "as full of envy at a stranger's goods; they lie in wait to plunder him of every thing, as a lion lieth in wait for the cattle; they have no mercy on the stranger; even if a stranger were to strip off his skin and to give it to them, they would seize upon it."

The only Foreign Members whom the Society has lost during the last year are Dr. Adam Afzelius, of Upsala, and Professor Morichini, of Rome.

Dr. Adam Afzelius was born at Larg in West Gothland in 1750, and was one of the last surviving pupils of Linnæus. In 1777 he was appointed Reader of Oriental Literature, and in 1785 Demonstrator of Botany in the University of Upsala, and he made his first appearance as an author by the publication of a short supplement to the *Flora Suecica* of his master, in the Transactions of the Academy of Stockholm for 1787. In the years 1792 and 1794, he made botanical expeditions to Guinea and Sierra Leone, and a considerable part of the collections which he formed in those countries passed subsequently into the herbariums of Sir Joseph Banks and Sir James Edward Smith. In 1797 he was made Secretary of Legation to the Swedish Embassy in this country, and in the following year he was elected a Foreign Member of the Royal Society on the ground of his great knowledge of botany and zoology. Upon his return to his own country, he became Professor of Materia Medica and Diætetics, at Upsala, situations which he retained for the remainder of his life. He was the author of a learned paper in the Linnean Transactions for 1791 on the genus Trifolium*, and also of two works entitled *Remedia Guinensia* and *Stirpium in Guinea medicinalium species*: he edited likewise the botanical Correspondence of Linnæus. He was a botanist of great learning and acquirements, and highly esteemed by the leading founders of the Linnean Society; but I am unable to connect his name with any considerable advancement in natural knowledge.

Professor Morichini, of Rome, was elected a Foreign Member of the Royal Society in 1827, and is chiefly known for his experiment on the magnetizing influence of the violet rays in the solar spectrum. His experiment was repeated by Configliachi at Pavia, and by Berard at Montpellier, without success, and in consequence doubts were expressed of the accuracy of his results, which appeared to be finally removed by the successful repetition of it by our justly celebrated countrywoman Mrs. Somerville, in the summer of 1825. I am not aware however that any other philosopher has succeeded in a similar attempt†.

[To be continued.]

ROYAL ASTRONOMICAL SOCIETY.

Nov. 10, 1837.—Among the numerous presents to the Library, laid on the table this evening, was the magnificent work of Struve, on the micrometrical measures of double and multiple stars, made at the Dorpat Observatory, from 1824 to 1837, with Fraunhofer's great telescope; accompanied with a Report to the President of the Imperial Academy of Sciences at St. Petersburg, detailing the nature of the work, and the principal results which had been deduced.

The following communications were read:—

On the Parallax of α Lyræ. By the Astronomer Royal.

The author commences with stating, that after the discussions as to the sensible annual parallax of α Lyræ, which have been conducted with so much ability and ardour‡, and in which the opposite opinions have been founded on so many well-chosen observations, it would be useless now to express an opinion, except it were based on more numerous and more excellent observations, reduced with greater attention to accuracy than in former instances. He states, therefore, that the whole number of observations employed was 184, made entirely in the year 1836, and, in general, distributed uniformly over the year (with the exception of the month of February, in which no observations could be obtained): that the observations were divided equally between the two circles, and that nearly half of them were by reflection: that the telescopes have been in the same position on the circle during the whole year, with the exception of a few days at its commencement: that the zenith points have been determined independently every day; and that the six microscopes of each circle have been read for every one of these observations, as well as for every observation assisting to determine the zenith point. He then states, as an innovation on the practice of Mr. Pond, that a *correction for runs* has been introduced, determined from examinations made every week; the necessity for which arises from the circumstance that it is practically impossible to adjust the microscopes, so that five turns of their micrometers shall correspond exactly to the interval between two divisions on the limb, and whose importance in this investigation depends on its periodic character varying with the temperature, and (with some regularity) with the season of the year. The author states his belief that, with this correction, the only appreciable defect of the mural circle is removed; and that it is thus superior to any other instrument which has been employed in this investigation. The author then gives the results in the form of equations, founded each on the mean of a group of observations; each set of observations (Troughton, by direct vision; Troughton, by reflection; Jones, by direct vision; Jones, by reflection) being divided into four groups, and an equation being obtained from each group, expressing the polar distance in terms of the correction of the coefficient of aberration, and the coefficient of parallax. Taking the mean of the results, with each circle, by direct and reflected vision, the coefficient of parallax from Troughton's circle appears to be $+ 0''\cdot 2$; and that from Jones's circle $- 0''\cdot 1$. The author concludes from this, that the annual parallax is too small to be sensible to our best instruments. The coefficient of aberration ($20''\cdot 36$) appears, from the observations with Troughton's circle, to require no sensible alteration; from the observations with Jones's circle, it appears to require the increase $0''\cdot 4$. The north polar distance of α Lyræ, for Jan. 1, 1836, deduced from the whole of the observations, is $51^\circ\ 21'\ 53''\cdot 73$.

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* Prof. Afzelius was the author of another paper in the Linnean Transactions, vol. iv., entitled "*Observations on the genus Pausus, and Description of a new Species.*"—Edit.

† See Phil. Mag., First Series, vol. liii. p. 269: Mrs. Somerville's paper was given in vol. lxviii., p. 168.

‡ Mr. Pond's paper on the Parallax of α Lyræ was inserted in Phil. Mag., First Series, vol. lxii. p. 292.

The constant quantity of the Moon's Equatorial Horizontal Parallax, deduced from observations made at Greenwich, Cambridge, and the Cape of Good Hope, in 1832 and 1833. By Professor Henderson, Astronomer Royal for Scotland.

An abstract of this paper will be found in the Society's "Monthly Notices"; but it is proper to state here, that the most probable value of the constant of parallax deduced from Mr. Henderson's observations is $57'\ 1''{\cdot}8$; the corresponding value of the moon's mass is $\frac{1}{78{\cdot}9}$; and that of the coefficient of lunar notation $9''{\cdot}28$.

List of Moon-culminating Stars observed at the Royal Observatories of Greenwich and Cambridge, in the month of June, 1837.

This list also will be found in the Monthly Notices.

December 8, 1837.—The following communications were read:

Observations of the Solar Eclipse of May 15, 1836, at the Observatory of San Fernando. By M. Cerquero.

Moon Culminating Stars observed at San Fernando in 1835 and 1836. By M. Cerquero.

List of 285 Stars of the Society's Catalogue which are Double. By Mr. Holehouse.

On a very ancient Solar Eclipse observed in China. By R. W. Rothman, Esq.

This is the famous eclipse which has been so much discussed by the Jesuit Missionaries, De Mailla and Gaubil, which has been vaunted as an irrefragable proof of the antiquity of the Chinese empire and science; and for failing to predict which, the unluckly astronomers, Ho and Hi, were punished with death.

The particulars are given in the *Histoire Générale de la Chine*, translated from the Chinese, by Moyriac de Mailla, and the *Observations Mathematiques*, &c., published by Souciet. It seems that a certain Chinese history, the *Chou King*, said to be of the highest antiquity, but without date, contains a statement to the effect that, towards the beginning of the reign of Tchong-Kang, on the first day of the third moon of autumn, there was an eclipse of the sun in the constellation *Fang* (Scorpio). Another chronicle, less ancient, but of which the date is still anterior to 460 B.C., states that the eclipse took place in the fifth year of Tchong-Kang, on the first day of the ninth month, and adds cyclic characters for the day and year, corresponding, according to some of the chronologers, to October 13, 2128 B.C. The chronologers, however, are not agreed with respect to the year of the eclipse. Some refer it to 2159 B.C.; others, as just stated, to 2128; and Gaubil, by whom the eclipse was calculated, and who cites the calculations of three other Jesuits in proof of the accuracy of his own, says it took place on the 12th of October, in the year 2155. Freret, on the authority of the calculations of Cassini, refers it to the 23rd of September, 2007. On account of these discrepancies, and the uncertainty occasioned by the imperfections of the tables employed in the former calculations, Mr. Rothman undertook to calculate the eclipse anew, from the more accurate tables now existing.

He states, that for the sun he employed Delambre's tables, and for the moon the elements of Damoiseau; and that he has found the eclipse took place on the 13th of October, 2128 B.C., the instant of the greatest phase being $12^h\ 8^m\ 47^s$ mean time from midnight at the place of observation, and the magnitude 10·5 digits;—a result agreeing entirely with the indications of the Chinese chronicle.

The reader may see the arguments for and against the authenticity of this observation, in Delambre, *Histoire de l'Astronomie Ancienne,* tom. i. p. 350, *et seq.*

On the Repetition of the Cavendish Experiment, for determining the Mean Density of the Earth. By the President.

It is well known to most of the Members of the Society, that the Council has long had it in comtemplation to repeat the celebrated and interesting experiment of the late Mr. Cavendish for determining the Mean Density of the Earth, and that a Committee was appointed more than two years ago to consider of its practicability. The object is now in a fair way of being accomplished, Her Majesty's government having been pleased to grant the sum of 500*l.* towards defraying the requisite expenses. The apparatus is, at this moment, in the course of being erected, and as soon as it is completed, the experiments will be commenced.

During the time, however, that the subject has been in agitation in this country, it appears that the same experiment has been undertaken by M. F. Reich, Professor of Natural Philosophy in the *Académie des Mines,* at Freyberg, in Saxony. The details of the experiment are given in a memoir, read by him at the German Scientific Association, which met at Prague in the month of September last. Whether this memoir has yet been published, the Author has been unable to ascertain; but an abstract of it has appeared in some of the foreign journals, from which the following particulars are collected.

The method followed by M. Reich appears to have been exactly the same as that of Cavendish. The apparatus was erected in a large room under the buildings of the *Académie,* the windows of which were carefully closed up, and other precautions taken to preserve a uniform temperature. The torsion-balance, carrying two small leaden balls at the extremities of its arms, was encased in a wooden box, of dimensions just sufficient to allow room for the oscillations. To avoid currents of air, the oscillations were observed by means of a telescope fixed outside the door of the room in which the apparatus was placed, and directed on a mirror attached to the extremity of the arm, and illuminated by a lamp, also placed outside the room. The masses, whose attraction was to be measured, were spheres of lead, weighing 45 kilogrammes, or 695,061 grains. They were suspended by brass rods to a beam, moveable about a vertical axis; and which by means of cords and pullies, the observer, without entering the room, could bring into any required position, with reference to the direction of the arms of the torsion-balance. It was found, however, most convenient, to use only one of the spheres. The principal correction required is for the *moment* of the arm of the balânce. This was computed by a method similar to that which was employed by Gauss for determining the *moment of inertia* of his magnetic bars.

Nearly two years were consumed in the necessary preparations; but when completed, M. Reich was enabled to perform the experiments during the three months of June, July and August, 1837. Each observation required the determination of three quantities:—

the distance of the centres of the large and small spheres, the time of the oscillations, and the deviation of the arm of the balance. The distance varied from 6·62 to 7·49 inches; the duration of the oscillations, from 6^m 41^s, to 6^m 50^s; and the deviation from ·236 to ·315 of an inch. The greatest source of error is in the determination of the deviation; the position of the arm being subject to some anomalous variations caused, probably, by slight currents of air in the interior of the wooden case. This source of error could only be eliminated by increasing the number of observations; but the differences of the partial results actually obtained were so small that the mean result may be regarded as sufficiently approximate.

The number of observations was 57. The mean of the whole gives the density equal to 5·44, a result which is almost identical with that of Cavendish. M. Reich also used for the attracting mass a sphere of cast-iron, of the same diameter as the leaden one, and weighing 30 kilogrammes, or 463,373 grains. Five observations with this sphere gave the density = 5·43.

The public will look with much interest for further particulars respecting these experiments, in order that they may be examined more in detail. The only innovations on the method of Cavendish appear to have consisted in using only one of the great spheres in the same experiment, and in the mode of observing the deviation of the arm of the balance. The arm itself appears to have been nearly of the same length as that used by Cavendish, but we are not informed of its weight, nor of the weight of the small balls. The large spheres, however, were much inferior to those of Cavendish, their diameters being only 7¾ inches, and weight less than 2-7ths of those used by Cavendish. The employment of the cast-iron sphere is a new feature in the experiment, but it does not appear that the small balls were changed.

Mr. Baily concludes with remarking that, though these experiments are, on the whole, confirmatory of the general result obtained by Mr. Cavendish, they do not interfere with the plan the council of the Society had in contemplation, which was not merely to repeat the original experiment in a precisely similar manner, but also to extend the investigation by varying the magnitude and substance of the attracting masses, by trying their effect under considerable differences of temperature, and by other variations that may be suggested during the progress of the inquiry.

A Catalogue of Moon-culminating Stars observed at Greenwich, Cambridge, and Edinburgh, in the months of July, August, and September, 1837. This catalogue appears in the Monthly Notice of the Society, No. 10. of vol. iv.

GEOLOGICAL SOCIETY.

Nov. 15, 1837.—A Letter from Walter Calverly Trevelyan, Esq., F.G.S., to Dr. Buckland, V.P.G.S., on "Indications of Recent Elevations in the Islands of Guernsey and Jersey and on the coast of Jutland, and on some Tertiary Beds near Porto d'Anzio" was first read.

On the shore near the point where the road descends towards the rock or islet of Lihou, on the east of Guernsey, may be seen a section, in which, above the present high-water mark, the granite rock bears evident marks of having been worn by the action of the waves, previously to the deposition on it of a bed of gravel, which now covers the granite and fills up the inequalities of its surface. The gravel, which is firmly bound together by a ferruginous sand, consists of pebbles of the neighbouring rocks, also of chalk flints, some not much rounded, and extends to about 8 feet above the present high-water mark, and apparently ranges a little inland. On the gravel is a bed, about 3 feet thick, of disintegrated granite, mixed with angular fragments of that rock, and covered by the surface soil.

On the N.W. of the island, near Fort Doyle, there occurs, near the shore, a similar bed of gravel, about 8 feet above high-water mark, resting on the surface of the syenitic rocks of a low cliff, which bears evident signs of disturbance from subterranean agency. Here the gravel, previously lying upon the rock, has fallen into and filled up the fissures which have been created, and has even been forced under some parts of the rock, which seem still to be in connexion with their original masses.

In St. Catherine's Bay, Jersey, the author also observed a section which affords evidence, though to a small extent, of an elevation of the old beach.

The author then calls attention to a fact regarding the elevation of land bordering on the Baltic, which he conceives has not before been noticed. On the coast of Jutland, near Frederickshavn (not far from the Scaw), the author observed, that the country abounded with sepulchral tumuli, except on a low and extensive tract bordering the sea, where none occurred. He therefore supposes, that the latter has been elevated since the period when the above mode of burial was disused in that country, which he believes was about the eighth or ninth century*.

The author concludes his letter with some account of a visit paid to Porto d'Anzio (the ancient Antium), and of some extensive tertiary (pliocene) beds found there. These deposits form cliffs about 50 feet high, and contain numerous shells, but little altered, and apparently of the same species as those now inhabiting the neighbouring sea. *Pecten Jacobæus* and *P. opercularis*, not at all water-worn, are the most numerous, often forming considerable beds in a loose or indurated calcareous sand. The dip of the strata is considerable and to the south-east. The deposit may be traced some way into the interior, and to elevations of 200 or 300 feet above the sea, where there are quarries worked in ancient times; and on the east to Nettuno, about a mile and a half from Antium, at which point the upper beds rise to the surface. Passing thence to the west, and beyond Antium, lower beds successively crop out for about a mile, when the lowest, a sandy clay, appears, and continues for some distance, nearly horizontal. It is overlaid in part by a bed of sand, containing a layer of gravel in the lower part, and the two strata form together a cliff 30 or 40 feet high. In the clay the fossils are not so abundant, but are apparently of the same species as in the upper beds.

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* A similar inference was drawn by Dr. Forchhammer with respect to Bornholm in a paper "On some changes of level during the historical period in Denmark", read May 31, 1837. See Proceedings of Geological Society, vol. ii. p. 555, or L. & E. Phil. Mag., vol. xi. p. 310.

About two miles from Antium, a thin bed of tertiary sandstone, containing abundance of the same fossils, begins to make its appearance resting on the clay, and it gradually increases in thickness to about 20 feet; but then gradually thins out again to the west, extending altogether between a quarter and half a mile. Below it, 20 feet of clay are exposed, and above it, a ferruginous sand, about 15 feet thick, through the lower 6 feet of which, some fine siliceous gravel is interspersed, like the flint gravel of the plastic clay of England, and agreeing in character with that of the present beach. The masses which have fallen on the shore, from above, look, when washed by the waves, exactly like a rock abandoned by the sea for merely a few tides, in consequence of the fresh appearance of the shells and corals with which they are covered.

In the tertiary rock of the neighbourhood, specimens were met with, in which the calcareous matter of the shells had been replaced by sulphur, and the author conceives the change to have been effected through the percolation of water, of which a stream exists, containing a strong solution of sulphate of iron with excess of acid. Near this spot, called Solfarata, are some pits, apparently in the upper sand, and from which sulphur is dug in winter.

A letter from Sir Robert Smirke was then read, forwarding another from Mr. Edge to himself, in which the latter states, that when engaged in erecting some works in the neighbourhood of St. Peter's, Guernsey, he found it necessary to have a well dug. At the depth of 45 feet from the surface, the workmen came to a block of granite, which they were forced to blast, and ascertained to be 6 feet in thickness. A few feet beneath the granite, they were surprised at finding a small quantity of peat, with several pieces of fossil timber (specimens of which have been sent to the Society) in the state of bog-wood, and conceived to be oak.

The reading of a paper was afterwards commenced "On the Geology of the Eastern Portion of the Great Basaltic district of India," by J. G. Malcolmson, Esq., F.G.S., of the Madras Medical Establishment.

Dec. 16, 1837.—Mr. Malcolmson's paper on the eastern portion of the Great Basaltic district of India, begun on the 15th of November, was concluded.

The principal objects of this paper are to describe the eastern boundary of the great basaltic formation of India, with its associated stratified deposits, and to arrive at a proximate conclusion respecting the age of the basalt.

Extent of Country.—The region noticed generally in the paper, is included between the 14th and 21st degrees of north latitude, and the 75th and 82nd degrees of east longitude; but the districts more particularly described, are those watered by the Pennar river (lat. 14°), the pass of the Sichel hills, near Neermul (lat. 19° 18′, long. 79° 33′), and the plains extending from the northern base of that chain to Nagpoor.

Physical Features of the Country.—The region forms part of the great, elevated plateau which includes all the countries to the south of the Nerbudda (lat. about 22° N.), and connects the provinces watered by the southern branches of the Ganges with the Deccan. It is traversed on the north by the Sichel or Shesha hills, locally called the Neermul range, which extends from the junction of the Wurdah and Godavery rivers (lat. about 18°·48′, long. 80°), till lost in the gradual rise of the country near Lonar (lat. 20°, long. 76° 30′). The principal rivers which traverse the region are the Wurdah, the Godavery, the Kistnah and the Pennar. The first flows north and west of the Sichels, the second south of that chain, till its waters unite with those of the Wurdah, when it takes a south-easterly direction to the Indian ocean. The Kistnah flows nearly W. and E., between the parallels of 16° and 17°; and the Pennar traverses the southern portion of the region (lat. 14° 30′). In the part watered by the last river, a marked feature is presented in the horizontal summits of many of the ranges of hills, which appear to have been once connected, though they are now separated by extensive plains.

Geological Structure.—The formations consist of granite, gneiss, mica and hornblende slates, trap, argillaceous limestone, red sandstone, with diamond breccia, and tertiary freshwater strata. The granite forms apparently the base of the country, and the trap penetrates all the formations, including the granite and the freshwater beds. In addition to these regular deposits are considerable accumulations of travertine and kunkur, which are scattered over the whole surface of the country.

Granite.—This rock is frequently displayed in all the rivers of southern India, and is occasionally visible as the substratum of the other formations. In the table-land of the Mysore it attains an elevation of 3000 feet above the sea. In the Deccan, between the Kistnah and the Godavery, it is traversed by greenstone dykes, sometimes porphyritic, and ranging, for the greater part, from S. by E. to N. by W., a direction not very different from that of several of the basaltic mountains in the northern part of the region; but on approaching the Godavery, from the south, the granite is penetrated by dykes, which strike N. and S. Beyond Nagpoor the granite has burst through the red sandstone, which is converted into quartz rock; and, still further north, granite veins intersect the argillaceous limestone, which has lost its stratified structure. Granite veins also penetrate the neighbouring hills of gneiss and mica slate.

Gneiss, Mica and Hornblende Slates.—These formations appear to be of limited extent. Hornblende slate was noticed by the author only in the neighbourhood of Deemdoortee, 20 miles E. of Neermul, where it contains the magnetic iron ore used in the manufacture of Damask steel. Gneiss and mica slate are mentioned only at the locality alluded to above, a few miles N. of Nagpoor.

Trap.—Mr. Malcolmson distinguishes the trap of the dykes from that which constitutes the great basaltic ranges, by the absence of olivine in the former, though it is common in the latter. The great masses of basalt are also distinguished by being amygdaloidal and more crystalline.

When *en masse* the trap overlies the granite, as well as the stratified deposits. In the form of veins it traverses the granite, limestone, and sandstone, and the freshwater strata are often imbedded or entangled in it.

In sinking a well near Hutnoor, (lat. 19° 38′ N., long. 78° 30′ E.) seams of pure white, pulverulent limestone were found beneath layers of basalt, and calcareous depositions appear to accompany the formation almost universally. With respect to the minerals contained in the amygdaloids, Mr. Malcolmson is of opinion, that they have not been produced either by infiltration or sublimation, but by molecular attraction, because calcareous spar is much more rare than siliceous minerals, though carbonate of lime abounds throughout the basalt.

Argillaceous Limestone.—Organic remains have not been noticed in this rock. It consists, in the lower part, of thin strata of compact blue or white limestone, and generally, in the upper, of blue, red, green and white schists, or slaty clay. Siliceous matter occurs in both the limestone and schist. Where the formation is in contact with the trap, the limestone is sometimes crystalline, and loses its stratified structure; and at the Pindee Ghàt, in the Sichel Hills, the argillaceous and siliceous ingredients appear to have separated, and the latter to have collected in bands, having partly the aspect of chalcedony, and in black chert. In some districts the limestone is cavernous, and it is often penetrated by circular cavities, which, the author conceives, were formed by the extrication of gaseous fluids, in the same manner as similar cavities are now produced in the mud by the escape of carbonic acid gas.

A jointed structure, dividing the beds into rhombs, prevails in the limestone, the schist, and the overlying sandstone. The strata are often inclined, apparently the result of dislocation.

At Jumulmudagur (lat. 14° 50′, long. 78° 30′) the limestone contains layers of muriate of soda; and Mr. Malcolmson is of opinion, that the salt which is found in the alluvial matter, is obtained solely from this formation, as he did not discover a trace of it in the sandstone.

The limestone and shale are well displayed in the Pennar district, also between the northern foot of the Sichels and Nagpoor; and the author has no doubt that they belong to the same system of strata as the limestone of Bundelcund, described by Major Franklin*, though the red sandstone of that country is stated to underlie the limestone, while in the region examined by Mr. Malcolmson it overlies.

Red Sandstone.—This formation is distinguished by containing the breccia in which are situated the diamond mines of Golconda, on the banks of the Kistnah, and those on the banks of the Pennar. Where the sandstone rests upon the limestone schist, a gradual passage occurs. The rock is more or less compact, and its prevailing colours are red and white. The diamond breccia is considered by the author, as only a variety of the sandstone in which fragments of older rocks have been imbedded. Ninety miles S.W. of Nagpoor traces of coal were noticed, and in the hill of Won (lat. 20° 6′, long. nearly 79°) Mr. Malcolmson found the cast of apparently a hollow vegetable, the only trace of an organic body observed by him. The sandstone, as already noticed, partakes of the same jointed structure as the subjacent limestone. It is penetrated as well as overlaid by trap, and near Nagpoor veins of granite have converted it into quartz rock. In the district drained by the Pennar, the sandstone attains the height of 3000 feet, forming the horizontal or flat summit of the mountains; but in the same district, and at no great distance, it occurs on a level with the plain.

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* Geol. Trans., 2nd Series, vol. iii., part i., p. 191 *et seq.*, also Asiatic Researches, vol. xviii. p. 24, *et seq.* An abstract of Major Franklin's paper appeared in Phil. Mag. and Annals, N.S., vol. iv. p. 294.

Tertiary Strata.—Masses and fragments of differently coloured chert, a tough, white, argillaceous stone, and a greyish blue crystalline rock, all containing freshwater shells, either project from the trap in which they are entangled, or are scattered over its surface for considerable areas in the Sichel hills. In a precipitous descent, on the northern flank, the author also noticed a horizontal bed of white limestone, 12 ft. thick, containing freshwater shells and resting on granite, but covered by basaltic debris. The organic remains, brought to Europe by the author, have been examined by Mr. James De Carle Sowerby, and ascertained to belong to two species of Gyrogonites, two of Cypris, two of Unio, with numerous specimens of Paludinæ, Physæ, and Limneæ. The greater part are siliceous casts, but some retain their original calcareous matter. Silicified portions of palm woods, and fragments of vegetables, in a charred or carbonized state, also occur. In accounting for the different state of preservation of the shells, Mr. Malcolmson suggests, that the lime being in some instances retained, may be explained on the supposition, that the shells were perfectly dry at the time they were acted upon by the basalt.

With respect to the origin of this singular rock, the author is of opinion that the basalt, when it was irrupted, changed the features of the country, and, destroying pre-existing lakes, entangled in its substance the debris and shells which had accumulated at the bottom of the bodies of water, and converted the loose sand into chert or siliceous rock. Of the age of the formation, he does not pretend to offer a precise opinion. None of the shells have been identified with those now inhabiting the rivers of India; and he is, therefore, inclined to consider them as extinct, and to refer them to the tertiary æra.

This fossiliferous chert was noticed by the author over a surface extending 140 miles N. and S.; but shells considered to be identical with those collected by him, were found by Dr. Spilsbury, 18 miles E. from Jubalpore (lat. 23° 45′ N., long. 78° 53′ E.), in a block of indurated clay, resting on basalt*; by Dr. Voysey, in the Gawilghur range (near the table-land of Jillan)*; by Dr. Spry, in a bed of limestone, overlaid by trap, near Saugor† (lat. 24° 15′ N., long. 79° E.); by Dr. Voysey, in a siliceous rock in the hills of Medcondah and Swalpigapah south of the Godavery‡; also at Jirpah, N. of the sources of the Taptee (about lat. 22° N., long. 78° E.)§.

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* Journal of the Asiatic Society of Bengal, vol. ii. pp. 205 and 583.

Comparative Age of the Formations.—On this head few observations are necessary. North of Nagpoor the granite has been shown to be more recent than the sandstone. The trap in the form of veins penetrates the granite, and affects, *en masse*, the limestone and sandstone, and entangles in its substance the fossiliferous chert. If therefore, the last belongs to the tertiary period, part at least of the basalt of the Sichel hills, forming the eastern boundary of the great basaltic region of India, cannot be assigned to one more ancient. Of the age of the limestone and sandstone Mr. Malcolmson offers no positive opinion, but he objects to their being considered the equivalents of the new red sandstone and lias of England‖, because their order of superposition in the district examined by himself, is inverted, the limestone underlying the sandstone; and he only ventures to suggest, that they may belong to the older secondary, or younger transition systems.

Travertine and Kunkur.—Springs charged with carbonate of lime prevail throughout the country, and in the bed of some of the rivers, calcareous matter is so abundantly deposited as to cement the pebbles into a hard rock. "It is impossible," says the author, "to examine these accumulations without immediately perceiving the origin of the nodular limestone or kunkur, which is so extensively distributed in India."

Thermal Springs and Mineral Waters.—At Kair (lat. 19° 55′, long. 78° 56′) and Urjunah, springs having a temperature of 87°, and charged with carbonic acid gas, issue through the limestone; and one, at the former locality, contains also a little muriate of soda, a minute quantity of sulphate of lime, and much carbonate of lime. At Byorah (lat. 17° 57′, long. 80° 20′) is a spring, the temperature of which is 110°; and at Badrachellum, (lat. 17° 43′, long. 80° 79′) one possessing a temperature of 140°, and containing sulphuretted hydrogen, and sulphates and muriates of soda and lime.

A minute description is given of the mineral waters of the Lonar Lake, (lat. 20°, long. 76° 30′) and of the natron which is deposited in a layer beneath its muddy bottom. The water of the lake is clear, its specific gravity is 1027·65, and it has no unpleasant smell; but the mud at its bottom is highly charged with sulphuretted hydrogen. The salt under the mud accumulates slowly, and is extracted only once in several years. It consists of carbonic acid 38·, soda 40·9, water 20·6, insoluble matter ·5, and a trace of a sulphate; and thus corresponds in composition with the Trona, or striated soda from the Lakes of Fezzan, analyzed by Mr. R. Phillips*, and approaches somewhat nearer to the equivalent numbers of the sesquicarbonate established by that analysis. The water of the Lonar lake contains, besides a little potash, muriate of soda 29 grains, sesquicarbonate of soda 4·2 nearly, and sulphate of soda ·1, in 1,000 grains of water. No lime was detected in it, nor any magnesia. The absence of the former Mr. Malcolmson says, is easily accounted for, as the sesquicarbonate of soda and the water itself precipitated the sulphate and muriate of lime, notwithstanding the mutual decomposition they undergo when in a semifluid state. In accounting for the production of the natron, he adopts the theory of Berthollet for the formation of that salt in the lakes of Egypt, viz., a mutual decomposition of the muriate of soda and carbonate of lime, when in a pasty state; but as the natron of Fezzan and the Lonar lake contains half an equivalent more of carbonic acid than can be furnished by carbonate of lime, he proposes a modification of that theory, and suggests that the carbonic acid by which the lime is held in solution in the mud, furnishes the acid, and perhaps indicates the existence of an unstable sesquicarbonate of that substance. "Where-ever," adds the author, "I have met with natron, or obtained detailed accounts of its occurrence, muriate of soda and carbonate of lime existed in the soil, and the natron was found on the surface of the moist earth or mud."

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* Asiatic Researches, vol. xviii. p. 192.

† Journal of the Asiatic Society of Bengal, vol. ii. pp. 376, 639.

‡ Asiatic Researches, vol. xviii. p. 193, and Journal of the Asiatic Society of Bengal, vol. ii. p. 304.

§ Journal of the Asiatic Society of Bengal, vol. i. p. 247.

‖ See Major Franklin's memoir on Bundelcund, &c., in the Geol. Trans., 2nd ser., vol. iii. p. 191, *et seq.*, also Asiatic Researches, vol. xviii. p. 24, *et seq.*; and Phil. Mag. and Annals, N.S., vol. iv. p. 294.

METEOROLOGICAL SOCIETY.

Jan. 9, 1838.—A paper on the meteorology of the Island of Teneriffe, lat. 28° 37′ N., long. 16° 15′ W., by Lieut. Grey (Associate Member of the Society) in charge of the Australian expedition, was read.

"The island of Teneriffe," says Lieut. Grey, "is of volcanic origin; and as its famed peak is at the present moment, and has been since the island was known to Europeans, in a state of solfatara, I entertained a hope that if a series of observations had been made they might possibly throw some light upon the influence that volcanic action exercises upon atmospheric phænomena."

Lieut. Grey ascertained that the late Dr. Saviñon of Laguna, a town on the island of Teneriffe, about 1800 feet above the level of the sea, and three miles from the shore, had made a series of thermometrical observations during a period of eight years (from 1811 to 1818 inclusive) without an interruption of a single day, from which observations a mean of the whole year was obtained of 62°·75 Fahr., and 68°·75 Fahr. the mean of July, the month of Lieut. Grey's visit to the island.

Lieut. Grey immediately caused two sets of observations to be made on board H. M. ship Beagle at Santa Cruz, the vessel lying at the time a quarter of a mile from the shore. One set was made in the cabin of Captain J. Wickham, R.N., and the other upon the quarter deck in the shade. During the stay of the vessel, which was only five days, the mean temperature (from these two sets of observations, taken three times a day, and in the unusually warm month of July,) was found to be 76° Fahr., a difference of less than eight degrees, which serves fully to corroborate Dr. Saviñon's observations, if we consider that one series of observations was made at an altitude of 1800 feet, and through an uninterrupted period of eight years, and the other at the level of the sea, and during a period of only five days.

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* Journal of the Royal Institution, vol. vii. p. 294.

The quantity of rain which fell at Teneriffe in 1812 was 19·33 inches, of which 5·24 inches fell in January in twenty-four hours. In 1813, 25·22 inches.

In Dr. Saviñon's MSS. the following particulars are recorded of a tempest of wind and rain which visited the Canary Islands in the night between the 7th and 8th of November 1826. In the Island of Teneriffe alone (though much damage was done in all the islands) there were 311 houses destroyed and 114 houses ruined, 243 persons killed, and 1042 animals destroyed. Thus may be seen the fury with which these storms sometimes rage*.

XLV. *Intelligence and Miscellaneous Articles.*

NOTE OMITTED IN DR. SCHLEIDEN'S PAPER.

"In order to meet any objections which may be made by those who have had no opportunity of examining these subjects more closely, I will just remark, *en passant*, that Corda's history of the development of the *Coniferæ*, (*Acta Leop. Carol.*, xvii. pars 11,) coincides with Nature in the fewest points possible. It has caused me much pain, on account of some eminent men, who, having neither opportunity nor time to examine into the case, and judging of others by their own conscientiousness, have allowed themselves to be led into a precipitate admiration. It is however, in this case, impossible to absolve them from all blame, since the memoir exhibits its character openly enough. For in the first page it is stated: 'Since the appearance of Robert Brown's writings, and his journey through Germany, every one is acquainted with the general results of his observations, so that I consider it superfluous to give, in this place, an accurate account of them.' Now it is well known that Mr. Brown had already published, in 1832, his discovery of the entrance of one or more pollen tubes into the micropyle; and those persons who had the good fortune to meet with Mr. Brown, during his journey through Germany (in 1833), will remember that he carried with him impregnated ovaria in spirits, and with his accustomed kindness showed the entrance of the pollen tubes into the ovulum to every one who took any interest in the subject. Notwithstanding this, Corda, a few lines lower down, affects an unpardonable ignorance of this fact, in order to arrogate to himself a discovery which Amici (already in 1830) and Mr. Brown had made long before him. . . . . . Inconsistencies are also evident in the figures. Fig. 14, for instance; the embryonal sac is termed *nucula,* (should be *nucleus,*) and the pollen tubes enter it in order to produce, by their emissions, heaven knows what kind of an imaginary figure. Fig. 22: here the embryonal sac is even called embryo (E), and the pollen tubes run around it. But it would be an herculean task to follow, step by step, this memoir. It will here suffice to observe that, with the exception of a few points of minor importance, everything almost surpasses the limits of possible error, and does not in the least represent nature. I refer every one, who has even but little practice in such examinations, to nature herself, as the observations are not of the most difficult kind."

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* Further particulars of this tempest will be found in Mr. Alison's paper on the Peak of Teneriffe, in Phil. Mag. and Annals, N.S. vol. viii. p. 26; and tables of meteorological observations made in the island at p. 439—441 of the same volume; in which also Mr. Alison discusses the decrement of temperature as observed in his ascent of the Peak, p. 248.—Edit.

ON THE DEVELOPMENT OF AN ELECTRIC CURRENT ACCOMPANYING THE CONTRACTION OF THE MUSCULAR FIBRE. BY DR. J. L. PREVOST.

About fourteen years ago we published, together with M. Dumas, a memoir on the muscular fibre, in which we determined that the contraction of the muscles was owing to the sinuous flection of the fibres; we attributed the flection to the attraction of the nervous reticula, which placed at short distances from one another and perpendicularly to the direction of the muscular fibres, approached each other when an electric current, emanating from the cerebro-spinal system, passed through them. Our observations having been made with a microscope inferior in goodness to those constructed by Amici, the true disposition of the motive apparatus escaped us, and our assertion was considered an ingenious hypothesis deficient in the investigations requisite for its confirmation. Last summer I again resumed this subject with better means, and the following is one of the results which I obtained. If we observe the muscles of a frog with a magnifying power of four hundred, we perceive that they are composed of small cylinders, the diameter of which varies from five to twenty hundredths of a millimetre: these cylinders are connected with each other by the cellular tissue, through which pass from one cylinder to the other the nerves and vessels.

The fibres arranged thus parallel to one another, fix themselves, without separating, either to the tendons or to the aponeuroses which correspond to their extremities, the latter becoming round and disposing themselves in a small cavity placed on the tendon to receive them.

The muscular cylinders, which we shall call fibres, are themselves composed of fibrillæ, the diameter of which amounts to about $\frac{1}{300}$

of a millimetre. They are placed in juxtaposition in the cylinder and united so closely that they appear to a common observer to form a homogeneous mass.

At the surface of the muscular fibres just described, we perceive some rings, which surround their entire circumference like small ribands; they are about $\frac{1}{100}$ of a millimetre apart from each other upon the fibre when it has lost all its irritability, closer on the living fibre: these rings belong to the enveloping membrane. If this latter becomes fissured longitudinally, which at times occurs, we observe the longitudinal fibrillæ which form its mass, project in the fissure. The torn portions of the rings enable us to observe the ends of the reticula of which they are composed, and which cannot be seen in the normal state.

On illuminating the muscular fibres by means of a mirror which reflects the light upon their upper surface, we observe that the nervous reticula which ramify on the muscle enter the linings of the fibres; they thus appear to surround them similar to a series of circularly curved handles (ansæ). The fibres in their quiescent state are not straight but slightly curved. When they act, every portion of the broken line which they present gravitates the one against the other, and the muscular contraction results from the shortening produced by this action. Such are the facts which every one may observe with a good microscope.

Now let us apply to this highly remarkable anatomical arrangement, the doctrine of electric currents along the nervous reticula. It is evident that in this case each fibre becomes a little magnet with a flexible joint, the various parts of which would tend reciprocally to attract each other, and would produce the effect which we observe in the muscular contraction. But how detect these currents? Hitherto they have been sought for only with the electrical multiplier; and we could not expect to find anything, as we had to do with closed circuits, and knowing at the same time that a divided nerve does not transmit any action. There was nothing left therefore but the magnet that could point them out to us. To employ the magnetic needle was rather a difficult affair: I had recourse to a different means.

If a needle is placed in contact with some finely divided filings, such as we obtain from a file and soft iron, be it ever so slightly magnetic, it is perceptible from the arrangement which the particles of iron take at its surface: they plant themselves as little needles, which arrangement is easily perceptible with a magnifier. We must not confound this action with the attraction by which minute bodies remain attached to a bar with which they are touched. I ran a very fine needle not magnetized into the thigh of a frog, following the direction of the fibres; the point projected and dipped into the filings. At the moment when I excited a violent contraction by wounding the spinal marrow, I saw the small particles of iron arrange themselves at the point of the needle as they do when it is magnetic; they disappeared with the irritation of the muscle.

By further investigating this phænomenon I hope to render it very evident, and I should have deferred the publication of it until then, had not Professor de la Rive advised me to join it to the preceding observation, [Matteucci's Researches on the Torpedo, vide last and present Numbers of Phil. Mag.] and to record its date in our Society.—*Bibliothèque Universelle*, November 1837.

ON THERMO-ELECTRIC PHÆNOMENA. BY CH. MATTEUCCI.

Every time that a copper wire attached to a galvanometer and well-brightened (*décapé*) is brought into contact with a second copper wire equally well cleaned, but heated by a lamp, we obtain an electric current, which passes from the hot end to the cold end. If we repeat this experiment with well-cleansed iron wires we obtain a contrary current which proceeds from the cold end to the hot one; the same takes place also with zinc and antimony. This difference is observed whatever be the temperature to which one of the wires is heated. Now if instead of touching the wires we immerse them in pure mercury contained in two capsules united by a siphon full of mercury, one of which is hot, the other at the common temperature, we have still a current, but which proceeds in the same direction with copper, iron, zinc, and antimony. The mercury has here no influence by the thermo-electric currents which it might develop, for the same results are obtained, if the two wires, always well-brightened, are immersed one after the other in the same heated capsule. It is therefore the action of the heat and of the air which produces an alteration at the surface of the metal: and in fact, if a copper wire is heated exposed to the air in the flame of an alcohol lamp, and afterwards immersed in the mercury where the other wire is, the same difference is still observed as when the unequally heated wires were placed one on the other.

I endeavoured to obtain thermo-electric currents with mercury by employing three capsules united by means of two siphons. In the outer capsules were plunged the wires of the galvanometer. I take away one of the siphons, heat the middle capsule and replace the siphon. I thus bring the hot mercury into contact with the cold. In this manner, however, I obtained but very feeble and doubtful deviations. Although the wire of the galvanometer was rather long, yet I doubt of there having been any development of thermo-electric currents on the mercury.

An amalgam of bismuth (1 of bismuth, ¼ of mercury) which is very crystalline, is endowed with a very considerable thermo-electrical power.

If a heated plate of bismuth is touched with the two extremities of a galvanometer, very powerful currents are obtained. If the bismuth be melted and we still retain the two ends immersed in the fluid metal, the currents cease; at times there are still some, but we at once easily perceive that either some of the bismuth has solidified, or that the two ends of the wire are unequally heated. With a larger melted mass in any sort of a vessel these currents entirely cease, If we then discontinue the heating, and allow the mass to become cold, at the instant when it solidifies the needle indicates deviations. The amalgam above-mentioned produces this phænomenon exceedingly well. If the amalgam, although still capable of becoming solid, loses its property of crystallization from the presence of a larger proportion of mercury, the phænomenon no longer takes place.

We obtain the same phænomenon with antimony. One might be inclined from this to conclude that thermo-electric currents take place only on solid metals, especially since it appears to be well demonstrated that it is by the effect of a chemical action that the contact of hot water with cold develops electric currents.—*Bibliothèque Universelle,* November 1837.

ON CETRARIN. BY M. HERBERGER.

This name of cetrarin is given to the bitter principle of the cetraria or liverwort. M. Herberger first procured this product; the process by which he obtained it is as follows: boil coarsely-powdered liverwort in four times its weight of alcohol, of specific gravity ·883 for half an hour, and let it remain; in order that no alcohol may be lost, it is strained and pressed; to this liquor there is to be added, for each pound of liverwort employed, three drachms of hydrochloric acid, which is to be diluted with four times its bulk of water; the mixture is to be allowed to remain for twelve hours in a stopped glass vessel; the supernatant liquor, of a deep yellow colour, is then to be poured off from the abundant deposit formed; this is impure cetrarin, and is of a green colour of greater or less intensity. It is to be collected on a strainer, and when it ceases to drop, it is to be pressed.

Cetrarin is to be purified by separating it into small portions, and washing it while moist with alcohol or with æther, which deprive it of its colouring matter; it is afterwards to be treated with twice its weight of boiling alcohol, which dissolves it, and allows it to precipitate on cooling. A further quantity may be obtained by evaporating the alcoholic solution.

Cetrarin is sometimes a white powder resembling magnesia, and at other times it is in small globules united in the form of arborizations, which exhibit no appearance of crystallization, even when examined by the microscope. When pressed it has a silky lustre; it is light, unalterable in the air, inodorous, with a decidedly bitter taste; it is not entirely fusible; it begins to become brown at 257° Fahr. At a higher temperature it yields a reddish-yellow acid oil, which becomes solid at 320° Fahr. It then blackens and leaves a great quantity of light charcoal, which burns readily in the air. It is soluble in boiling absolute alcohol, 100 parts of which take up 1·70; at 60° it dissolves only 0·28.

The use of cetrarin in medicine is but recent, and it is impossible to say how far it may be employed; it has, however, been exhibited as a febrifuge. The formula being,

Cetrarin........	2 grains,
Gum arabic	2 grains,
Sugar	12 grains,

for a dose, and it is recommended that it should be repeated every hour.—*Journal de Chimie Médicale*, November 1837.

ACTION OF CHLORINE ON ÆTHERS.

M. Malaguti finds that dry chlorine, while acting in the dark upon oxacid æthers, always attacks, and in a uniform manner, the sulphuric æther which is the base of them. If the acid of the compound æther be represented by $\overline{X}$, the formula, after the action of the chlorine, will always be $\overline{X}$, $C^8\ H^6\ Cl^4\ O$, that is, 4 atoms of hydrogen replaced by 4 atoms of chlorine.

The action of potash on the compound chloridized æthers is also constant and uniform: the results are always chloride of potassium, acetate of potash, and an organic salt with a base of potash, the acid of which is that which existed in the compound chloridized æther.

Although the action of the chlorine is constant and uniform, the phænomena which accompany it are not always the same. Thus the camphoric and œnanthic ethers, during the action of chlorine, give out only hydrochloric acid. The acetic and formic æthers, during the same action, disengage hydrochloric acid, and acetic or formic and hydrochloric æther. It sometimes happens that the acid of the compound æther is attacked by the chlorine, and presents, in its turn, the phænomena of substitutions; but the action of chlorine upon the sulphuric æther, which serves it as a base, is not modified, and remains independent. Sulphuric æther, subjected to the action of chlorine under the same circumstances as the compound oxacid æthers, among the numerous products which may be foreseen, yields a liquid, the elementary composition of which is $C^8\ H^6\ C^4\ O$. This liquid is changed by the action of potash into chloride of potassium and acetate of potash.

The æthers which M. Malaguti subjected to the action of chlorine are the camphoric, œnanthic, acetic, formic and benzoic. Some other compound æthers, as the mucic and pyrotartaric, appeared to him not to be attacked; but he is going to resume these experiments, and to extend them to other æthers. At present, the agreement which the fore-mentioned facts prove to exist between the action of chlorine and the compound oxacid æthers, and the action of the same agent upon sulphuric æther, induce him to think that it is very probable, when any other compound oxacid æther may be acted upon by chlorine, four volumes of hydrogen will be replaced by four volumes of chlorine in the base, allowing for modifications which the acid may undergo.—*Journal de Chimie Médicale*, November 1837.

CAMPHORIC ACID.

According to M. Malaguti, the formula for camphoric acid is thus written:

Anhydrous acid............ $C^{20}\ H^{14}\ O^3$,
Hydrated acid .... $C^{20}\ H^{14}\ O^3,\ H^2\ O$,
Camphorate of silver, &c..... $C^{20}\ H^{14}\ O^3,\ AgO$.

The acid, yielded by the action of nitric acid upon camphor, is the hydrated acid; when it is sublimed, as first shown by M. Guibourt, it becomes anhydrous. M. Malaguti obtained the anhydrous acid in a different manner; by decomposing camphovinic acid by distillation, he converted it into anhydrous camphoric acid and camphoric æther. He has also verified the composition of camphoric acid by analysing camphovinic acid, camphoric æther, camphorate of ammonia, silver, &c. He has observed that hydrous and anhydrous camphoric acid produce salts sensibly different from each other, although dissolved in water, in the same manner as phosphoric and pyrophosphoric acid. He has also observed that camphoric acid in solution does not form a neutral ammoniacal salt; that, in order to obtain this salt, it is requisite to expose the crystallized hydrated acid to ammoniacal gas until absorption ceases, in the same manner as performed by M. Robiquet with gallic acid.

M. Laurent has also obtained results perfectly similar to those of Malaguti, respecting the composition of camphoric acid; but they were not published so soon.—*Journal de Chimie Médicale*, November 1837.

ON THE COLOURS OF METALS. BY MONS. R. BOTTIGER.

It has long been known, that when very fine copper filings are put into a vial, and they are covered with a saturated solution of hydrochlorate of ammonia, in 24 hours a colourless solution is obtained, provided only one-third of the vial contains air, and that it is well stopped and frequently shaken; this solution, when exposed to the air, instantly becomes of a fine sky-blue colour, and is again rendered colourless by being again strongly shaken in the vial with the copper filings.

This liquid is a solution of ammoniacal chloride of copper; and if a slip of polished platina be put into it, no change is perceptible; but if, at the same time, it be touched with a piece of zinc, its surface, whatever may be its extent, becomes then completely covered with a very thin stratum of copper, and this disappears quickly, as soon as the platina is separated from the zinc. This fact is readily explained when it is remembered that the liquor contains a considerable quantity of free ammonia. If also, instead of exposing the coated platina to the action of the ammoniacal liquid, it be immersed in a vessel of water, as soon as the coating of copper appears, it remains fixed on the platina, notwithstanding it may be agitated in the water.

If, instead of removing the platina from the contact of the zinc as soon as the copper appears, this action is suffered to continue for a longer time, for example, during one or two minutes, bubbles of gas are given out, and copper is deposited in spite of its electro-positive state, the colour of which is no longer red, but, on the contrary, appears black. At the same time, the red colour of the slip of platina, derived from the deposit of copper, which was at first fixed upon it, disappears, and in its place there arise various shades of all colours possessing remarkable beauty. Some are yellow, others green, others again red or brown, but the greatest number are black. When it is desired to fix these colours on the metal employed, it is sufficient to withdraw it from the liquid as soon as the last colours begin to predominate, and to allow them to dry spontaneously in the air.

By employing this simple and easy process, M. Bottiger succeeded in fixing the most incompatible tints on the same metallic surface, the aspect of which was, nevertheless, as soft as possible.—*L'Institut*, September 1837.

ON THE ACTION OF PROTOXIDE OF IRON ON PROTOXIDE OF COPPER.

M. Levol remarks that it is well known that the persalts of iron are reduced to protosalts by the protochloride of copper; whilst the protosulphate of iron produces no such effect with the salts of copper, at least with the sulphate; this fact being known, superior affinity would, from analogy, be assigned to dioxide of copper over protoxide of iron; but the facts which M. Levol states prove that this is not the case under all circumstances.

If a mixture of sulphate of copper and protosulphate of iron be dissolved in water, no chemical action, as is well known, takes place, and the two oxides will remain mixed in solution, that is, under the most favourable circumstances for reaction without any occurring; if, however, the oxides be precipitated by an alkali, the case is no longer the same, and experiment shows that peroxide of iron and dioxide of copper are obtained, instead of the oxides which existed in the two salts; the iron, therefore, is peroxidized at the expense of the oxide of copper.

The affinity of the two protoxides for sulphuric acid, the impossibility of forming a sulphate of dioxide of copper, and even of the existence of dioxide of copper in the presence of sulphuric acid, are causes which unquestionably oppose the reaction of the oxides in the two sulphates; it appears, therefore, that half an equivalent of oxygen, separated from the protoxide of copper, leaves it in the state of dioxide; and this, added to the equivalent of oxygen in the protoxide of iron, converts that into sesqui- or peroxide. That this is the case, is also shown by dissolving an equivalent of each sulphate in water, and adding ammonia to the mixed solutions. When kept from the contact of air, a colourless solution of ammoniuret of dioxide of copper will be formed, and peroxide of iron precipitated, containing scarcely a trace of copper.

If an equivalent and a half of sulphate of copper and one equivalent of sulphate of iron be similarly treated, peroxide of iron will also be precipitated; but blue ammoniuret of protoxide of copper will also be formed; when, on the contrary, the proportions were reversed, a colourless solution was obtained which contained protoxide, both of iron and copper; and by exposure to the air it consequently became of a blue colour, and precipitated peroxide of iron. Potash produces the same precipitation of dioxide of copper and peroxide of iron.

From the foregoing experiments M. Levol concludes:

First. That when ammonia is employed as a reagent, it may happen that the presence of copper, even in large quantity, may escape detection, if the solution also contains protoxide of iron, especially if it be supposed that this oxide is either partially or totally in the state of peroxide. It is not even requisite for this that the operation should be conducted in a close vessel, if the iron preponderates; because, when once solidified, the pellicle of oxide formed at the upper part of the solution preserves the rest from further oxidizement. Thus, in an analysis, or to discover by ammonia copper mixed with iron, it is requisite first to peroxidize the iron entirely.

Second. As no suboxide of nickel exists, it cannot occasion the same reaction; and therefore a new method of distinguishing it from copper results, when in the state of a double ammoniacal salt. This is effected by pouring an excess of solution of a protosalt of iron into the ammoniacal solution, which immediately decolorates it if copper, and not nickel, be held in solution, operating out of the contact of the air.—*Annales de Chimie et de Physique*, July 1837.

ON THE GASES CONTAINED IN THE BLOOD, AND ON RESPIRATION. BY M. G. MAGNUS.

M. Magnus remarks that it remains a question whether carbonic acid is formed in the lungs by the oxidizement of a part of the carbon in the blood by the action of the air, or whether venous blood, when it reaches the organs of respiration, contains carbonic acid ready formed, which is merely separated from it.

M. Magnus passed hydrogen gas through a solution of potash to deprive the gas of any carbonic acid which it might contain, and when it gave no precipitate with lime wate he passed it into the blood of a healthy man; the gas afterwards made to go through lime water gave a plentiful precipitate of carbonate of lime. Azotic gas similarly employed produced a like effect; and M. Magnus concludes, from these experiments, that carbonic acid exists ready formed in the blood, and consequently that it is not formed in the lungs. Carbonic acid was also separated from blood by means of the air-pump.

By using Liebig's apparatus M. Magnus found that blood contained about one-fifth of its volume of carbonic acid gas, and when it had been kept 24 hours, without emitting any bad smell, the quantity was larger. The results were confirmed by employing atmospheric air instead of hydrogen gas.

M. Magnus then ascertained the nature and proportions of all the gaseous contents of the blood. He found that 100 volumes of the arterial blood of a horse yielded

Carbonic acid gas..........	4·32 vols.
Oxygen	1·52 ,,
Azote	2· ,,
Total	7·84 vols.

The venous blood of the same horse, drawn 4 days afterwards, gave

Carbonic acid gas		4·29 vols.
Oxygen		1·12 ,,
Azote		·54 ,,
	Total	5·95 vols.

The arterial blood of the calf contains more, and the venous blood less oxygen, than that of the horse.

M. Magnus observes that these experiments, and others which we have not copied, appear to show that the gases contained in the blood of the animals, amount to about one-eighth or one-tenth of the quantity employed. He admits however that the experiments are not absolutely precise, because they were not all continued the same length of time, &c. But he observes, that as the proportions between the oxygen and carbonic acid are invariably the same, these results may be regarded as satisfactory.

With regard to the theory of respiration all experimentalists agree as to the reciprocal proportions between the carbonic acid expired and of the oxygen absorbed; while however some of them are of opinion that those quantities are always equal, as must happen if the oxygen gas were employed merely in the formation of carbonic acid in the lungs, there are chemists whose results show that more oxygen is inspired than carbonic acid expired. Messrs. Allen and Pepys observed that this was constantly the case when the same air was repeatedly respired.

M. Magnus adds, that this fact, so inexplicable by other theories, is an immediate consequence of the hypothesis founded on the law, that a liquid holding a gas in solution parts with it when it comes in contact with another gas.

Another circumstance noticed by Messrs. Allen and Pepys is as inexplicable as the preceding, namely, that by the respiration of oxygen, or by a mixture of oxygen and hydrogen, azotic gas is constantly expired, the volume of which is proportional to the bulk of the animal; this proves that it cannot at all be attributed to the air.

It now remains to be shown that the carbonic acid extracted from the blood is in sufficient quantity to account for the whole of that which the lungs expire. The results obtained on this subject are discordant; those of Messrs. Allen and Pepys evidently exceed what they should be; for Berzelius has shown that, if correct, it would require six pounds and a quarter of solid nourishment in 24 hours to produce the quantity of carbon consumed.

Taking then the results obtained by Davy as a mean of those of Lavoisier, Allen and Pepys, although perhaps a little too high, we shall have 13 cubic inches as the quantity of carbonic acid gas expired by a man. If it be further admitted, that at each pulsation of the heart an ounce of blood arrives at the lungs 75 pulsations in a minute would convey five pounds of blood in the same time. This is the minimum quantity which can be admitted; for it is very probable that five pounds of blood pass through these organs every minute: these five pounds produce 13 cubic inches. It has been already mentioned that the blood contains at least one-fifth of its volume of carbonic acid; and as a pound is equal to 25 cubic inches, each pound of blood would contain at least 5 cubic inches of carbonic acid. It will be observed that no circumstance opposes the proposed theory, hence the experiments prove, that the quantity of carbonic acid contained in venous blood, is more than sufficient to furnish the quantity expired.—*Journal de Chimie Médicale,* November 1837.

ON THE LOW TEMPERATURE OF JANUARY 1838, BY MR. F. WATKINS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

As my own residence is so unfavourable for meteorological observations, I have had for many years, in the Blackheath road, a series of instruments under the charge of a gentleman of high scientific acquirements, and devoted to the study of the various phænomena which occur in the aërial regions. From his observations a diagram has been formed, showing the maximum and minimum thermometric curves for the month of January as compared with the mean.

The beginning of the month was mild, both curves being above the mean temperature of Greenwich, deduced from twenty years' observations. Towards the 8th both curves descended far below the mean, and continued so till, on the 20th, just at sun-rise, the thermometer stood at 4° below 0 or zero, or 40° below the mean of the period.

This low degree of temperature lasted some hours, for at 9 a.m. it was −2°, at 10 a.m, +1½° at 11 a.m. +4°, and at noon only +7°, after which it rose many degrees, and the wind veered from the east to south.

On the 22nd both curves ascended *above* the mean, and on the 23rd descended as abruptly *below*, accompanied by a strong easterly gale, which continued until the end of the month.

Two things may be here remarked, as being unprecedented in the annals of meteorology in this country: 1st, the thermometer below zero for some hours; and 2ndly, followed, almost immediately after, by a variation of nearly 50 degrees. It should be noticed however from general observation that thaws commonly succeed very unusual low degrees of temperature.

My friend informs me, that before last month the lowest degree of temperature he has ever registered during thirty years, is +4°, and that only once, and it was very transient.

The temperature of the month had been gradually preparing us for an extreme of cold, for on the 12th the minimum was 12°·2, on the 13th 9°·6, and on the 15th 6°·5. From the 18th to the night of the 19th, on which day there was only a maximum of 21°, the approach of an unusual degree of severity was indicated; the radiator on the snow at 6 p.m. marked 9°: the evening proved *cloudy* with a variable temperature from 15° to 12°. The lowest mean temperature for the month (for twenty years) falls about the 16th. Finally, the average degree of cold on our very severe night is 16°; therefore the thermometer on the night of the 19th departed 20° lower than this, a circumstance, happily for us, of rare occurrence in this climate. I remain, Gentlemen, yours, &c.,

5, Charing Cross. FRANCIS WATKINS.

Just published, No. I. *New Series,*

Annals of Natural History, or Magazine of Zoology, Botany, and Geology. (Being a Continuation of the 'Magazine of Zoology and Botany,' and of Sir W. J. Hooker's 'Companion to the Botanical Magazine.') Conducted by Sir W. Jardine, Bart., P. J. Selby, Esq., Dr. Johnston, Sir W. J. Hooker, Regius Professor of Botany, Glasgow, and Richard Taylor, F.L.S.

METEOROLOGICAL OBSERVATIONS FOR JANUARY 1838.

Chiswick.—Jan. 1. Fine: slight rain: very fine. 2. Cloudy and fine. 3, 4. Very fine. 5, 6. Dense fog. 7. Bleak and cold. 8, 9. Frosty: slight snow. 10—13. Frosty. 14. Snowing. 15—18. Continued severe frost. 19, 20. Most intense frost. 21. Overcast: thawing. 22. Fine. 23. Hazy and cold. 24—27. Frosty: bleak and cold. 28—30. Fine. 31. Hazy and cold.

On the night of the 19th and morning of the 20th, the frost was more intense than has been the case for at least the period since the commencement of the present century. The registering thermometer in the Arboretum of the Horticultural Society's garden was 4 degrees below zero of Fahrenheit's scale; and some thermometers in the neighbourhood, in more exposed situations, indicated 6 degrees below zero. The destruction of subjects of the vegetable kingdom has in consequence been unprecedented in this country, within the memory of any now alive.—R. T.

Boston.—Jan. 1. Fine: beautiful morning. 2. Cloudy. 3. Cloudy: rain early A.M. 4, 5. Fine. 6. Foggy. 7. Cloudy. 8, 9. Snow 10, 11. Cloudy: snow A.M. and P.M. 12. Cloudy. 13. Snow. 14. Fine 15, 16. Cloudy. 17. Fine: snow early A.M. 18. Fine: large fall of snow early A.M. 19. Cloudy. 20. Cloudy: Ther. 12, 5 P.M. 21, 22. Cloudy. 23, 24. Stormy. 25—28. Cloudy. 29. Rain: snow early A.M. rain P.M. 30. Foggy: rain P.M. 31. Cloudy.

Penzance.—Jan. 1. Fair and clear. 2. Very stormy, rain, fair at night. 3. Cloudy, showers. 4. Fair and clear. 5. Fair with clouds. 6, 7. Cloudy, a shower. 8. Cloudy. 9. Clear and fair. 10. Snow. 11. Sleet, fair at night. 12. Misty, fair and clear at night. 13. Stormy, sleet. 14. Cloudy, clear at night. 15. Rain, evening fair. 16. Sleet, fair at night. 17. Fair and clear, snow shower. 18. Fair and clear. 19. Cloudy, snow. 20. Cloudy and stormy. 21. Misty. 22. Fair and clear, evening a shower. 23. Fair with clouds, evening fair and clear. 24. Very stormy. 25. Cloudy, showers, stormy. 26. Cloudy, heavy showers at night. 27. Showers. 28. Fair with clouds, showers in the evening. 29. Fair and clear. 30. Cloudy. 31. Fair with clouds.

The early part of the month was mild though stormy; the thermometer reaching as high as 49°. Cold commenced on the 8th, and continued to increase in severity till the morning of the 19th, when the thermometer stood at 23°, the wind blowing from E.S.E. On the 20th the temperature had risen to 45°, the wind still continuing to come from the eastern quarter; the mercury in the instrument kept advancing till the 29th, when it arrived at its maximum, 50°; the wind suddenly veering to the W., but proceeding from that direction for only twenty-four hours, when it again moved round to the E., which produced a consequent depression of temperature. Though the barometer gradually fell in the space of ten days (from the 18th to the 28th) more than an inch, yet the rain in the gauge did not amount to an inch. The anemometer during this period marked E. to E.S.E. This month must be considered a remarkably fine one, though cold for this part of England.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. VEALL *at Boston; and by* Mr. R. HOCKING, *Penzance.*

Days of Month. 1838. Jan.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8½ A.M.	Penzance.		London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	Penzance.		London: Roy. Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	Penzance.	London: Roy. So. 9 A.M.	Chisw.	Boston.	Pens.	Lond.: Roy. Soc. 9 A.M.
		Max.	Min.		Max.	Min.	Fahr. 9 A.M.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·978	29·982	29·937	29·55	29·769	29·598	45·7	50·2	43·8	48	32	40	49	42	SSE.	S.	calm	SW.	...	·01	...	...	43
2.	29·888	29·894	29·648	29·43	29·429	29·301	45·8	48·7	43·0	48	41	40	49	42	SE.	S.	calm	WSW.	...	·02	...	0·380	43
☽ 3.	29·642	29·821	29·563	29·15	29·641	29·407	44·3	48·8	43·2	48	27	43	46	41	SSE.	S.	calm	NW.	...	·04	·06	0·065	43
4.	29·976	30·384	29·992	29·53	30·020	29·771	40·2	48·3	39·4	48	28	34	46	41	S.	SW.	calm	NW.	...	...	...	...	39
5.	30·270	30·302	30·283	29·85	30·040	30·026	37·4	47·4	35·8	34	30	30·5	49	43	W.	NW.	calm	W.	...	...	...	...	38
6.	30·292	30·326	30·252	29·95	30·008	29·963	35·5	38·7	32·3	36	33	28·5	46	42	SW.	NE.	calm	S.	...	...	...	...	34
7.	30·304	30·341	30·316	30·	30·090	30·074	36·7	40·4	35·0	35	27	35	44	36	N.	NE.	calm	ESE.	...	...	...	0·030	34
8.	30·398	30·447	30·270	30·07	30·066	30·000	30·6	37·3	28·8	30	22	30	37	28	NE.	E.	calm	E.	...	...	...	...	25
9.	30·212	30·234	30·151	30·	30·065	30·040	24·0	32·8	23·0	25	22	28	30	27	N.	NE.	E.	ENE.	...	...	...	...	23
○ 10.	30·050	30·109	29·930	29·83	30·040	29·977	25·3	25·6	23·2	26	13	29·5	34	30	N.	NE.	E.	NE.	...	...	...	...	23
11.	29·960	30·041	30·024	29·66	30·004	29·667	22·4	23·2	19·8	27	11	26	38	32	NW.	N.	calm	NE.	...	...	...	0·070	21
12.	30·324	30·408	30·041	30·05	30·124	30·090	23·3	24·0	19·7	24	9	25	39	31	NNW.	E.	calm	ESE.	...	...	...	...	20
13.	30·314	30·376	30·194	30·05	30·020	29·930	21·8	27·7	20·8	25	20	27·5	34	32	SE.	NE.	E.	ESE.	...	...	...	...	21
14.	29·988	30·018	29·959	29·80	29·810	29·665	24·4	26·2	22·0	26	4	25	38	33	NW.	N.	calm	S.	...	...	...	...	20
15.	29·824	29·887	29·859	29·56	29·582	29·470	15·7	26·3	14·9	26	15	20	38	34	SW.	NW.	calm	SE.	...	...	...	0·160	12
16.	29·850	30·100	29·905	29·64	30·050	29·797	27·0	26·3	11·4	29	21	26	37	29	NW.	NE.	calm	E.	...	...	...	...	20
17.	30·216	30·291	30·276	29·98	30·180	30·100	29·0	32·0	24·2	30	20	27	37	29	NW.	N.	calm	ESE.	...	...	...	...	21
18.	30·016	30·084	29·938	29·84	29·851	29·667	23·3	31·5	23·4	23	19	26	31	23	NE.	NE.	E.	E.	...	...	...	...	19
☾ 19.	29·786	29·930	29·826	29·58	29·545	29·525	21·0	25·6	21·2	22	−4	23	37	30	NNE.	N.	calm	ESE.	...	...	...	...	11
20.	29·934	30·018	29·994	29·70	29·536	29·510	12·8	21·0	11·5	12	7	15	45	37	NW.	N.	calm	SE.	...	...	...	...	14
21.	29·892	29·941	29·757	29·72	29·460	29·376	24·5	25·0	13·8	38	28	23	48	43	ENE.	SE.	calm	S.	...	...	...	0·220	16
22.	29·680	29·786	29·740	29·45	29·326	29·313	39·8	40·3	24·7	46	29	34	50	44	ESE.	SE.	calm	S.	...	...	...	...	24
23.	29·706	29·755	29·713	29·54	29·320	29·288	34·4	43·3	32·7	37	24	33·5	48	38	NE.	E.	E.	ESE.	...	...	...	...	33
24.	29·752	29·816	29·769	29·70	29·337	29·316	26·7	37·4	26·4	28	23	27	39	32	NE. var.	NE.	E.	ESE.	...	...	...	...	23
25.	29·554	29·604	29·494	29·36	29·290	29·130	26·7	27·0	24·8	29	26	27	42	36	ENE.	NE.	calm	SE.	...	...	...	0·100	21
● 26.	29·362	29·414	29·389	29·20	29·110	29·087	28·6	29·2	26·4	31	27	29	43	35	ENE.	NE.	calm	SE.	...	...	...	0·360	24
27.	29·284	29·410	29·328	29·13	29·091	29·082	29·8	30·4	28·4	32	27	31	46	36	ENE.	E.	calm	E.	...	...	...	...	23
28.	29·454	29·531	29·490	29·20	29·089	29·080	28·7	31·0	28·5	36	28	32	49	43	E.	E.	calm	E.	...	...	...	0·230	24
29.	29·464	29·698	29·529	29·20	29·339	29·230	37·8	38·4	28·8	49	32	33	50	41	E.	S.	calm	WSW.	...	...	...	...	29
30.	29·640	29·729	29·479	29·30	29·436	29·387	35·8	44·3	35·8	37	32	34	47	40	E.	SE.	calm	SE.	·072*	·20	·90	...	32
31.	29·784	29·993	29·833	29·50	29·734	29·487	35·2	36·8	33·8	35	32	33	46	36	NE.	NE.	calm	SE.	...	...	·04	...	31
Mean.	29·897	29·986	29·867	29·62	29·810	29·592	30·1	34·4	27·1	32·99	20·74	29·5	42	36					Sum.	·27	1·00	1·615	Mean

THE
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[THIRD SERIES.]

APRIL 1838.

XLVI. *On the Action of Nitric Acid upon Bismuth and other Metals. By* Thomas Andrews, *M.D., Professor of Chemistry in the Royal Belfast Institution.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I AM happy to find that the observations which I communicated to the British Association, on some singular modifications of the ordinary action of nitric acid upon certain metals, have attracted the attention of so distinguished a philosopher as M. Schœnbein, whose opinions upon this subject must be considered to be of peculiar value. As, however, the results of some of my experiments are at variance with those which M. Schœnbein has obtained, and would tend perhaps to modify his conclusions, and as the published notice of my paper is very brief and imperfect, I shall now endeavour to give as complete an account of this subject as my investigations will enable me to do.

In the following extract from the manuscript read at Liverpool will be found a complete description of the phænomena to which M. Schœnbein alludes, and which he supposes that I may not perhaps have remarked.

"Having introduced a small fragment of bismuth into a large excess of nitric acid of sp. gr. 1·4, and afterwards brought a plate of platina exposing an extensive surface to the liquid, into contact with the bismuth, the solution of the latter almost entirely ceased, while at the same time its surface assumed a peculiarly brilliant lustre. On removing the platina, the bismuth sometimes began to dissolve in its ordinary manner; at other times, a dark film appeared upon its surface, which soon afterwards dissolved away and the metallic surface again became visible. But the action of the acid on the bismuth was now no longer perceptible, and, although not altogether arrested, yet it had become so feeble that a piece of bismuth weighing scarcely half a grain was not completely dissolved by a large excess of acid at the end of two days. Yet during this period the acid was removed, and replaced by a fresh portion. Indeed, the more frequently the solvent was changed, the more slowly did the action proceed,—a result apparently paradoxical, but arising from the circumstance, that the metal in this peculiar state is less able to resist the action of an acid having a metallic salt in solution than that of a pure acid.

"If the bismuth in this peculiar state was touched by a platina wire, the only effect apparently produced was that of increasing, perhaps, the brilliancy of its lustre; but when the contact with the platina was broken, the surface of the bismuth became at first covered with a dark film, then recovered its metallic aspect as before; and this series of phænomena always occurred, when connection with the platina was made and broken.

"Copper gave very similar results to bismuth. The contact of platina checked its solution in the same acid and maintained its surface bright. When the platina was removed its surface became covered with a black coat of oxide, which was afterwards very slowly dissolved by the acid, leaving the copper in the peculiar or slowly soluble state. But if the copper, while covered with the oxide, was raised from the liquid, the acid adhering to its surface instantly dissolved away the crust: the copper was now however left in its ordinary state."

It is obvious that the bismuth and copper, in the preceding experiments, are brought into the peculiar or slowly soluble state, by being made the positive surfaces in a simple voltaic arrangement. I was therefore much surprised to observe that M. Schœnbein should have failed in producing the same effect by making the bismuth act as the positive pole of a pile, while it is well known that iron can be rendered inactive in both ways; and that from this difference in the bearing of the two metals, he should have concluded, that the peculiar condition of iron is not brought about by the same cause which occasions the inactivity of bismuth. The following experiments will, however, show that in this respect there is the most perfect similarity in the behaviour of iron and the other metals.

When a small bar of bismuth connected, as the positive pole, with a battery composed of two pairs of amalgamated zinc and platina plates, was introduced into nitric acid of sp. gr. 1·4 at the temperature of 75° Fahr., its solution was instantly checked, and on breaking contact with the battery, the bismuth was found to be in the peculiar state. The acid in this experiment was contained in a platina capsule, which was connected with the other end of the battery and formed the negative pole. But on substituting for this feeble arrangement a battery of 20 pairs of double plates, in moderate action, the bismuth continued to dissolve at a sensible rate when the circuit was completed (although much more slowly and in a different manner than when alone), and the peculiar state was afterwards rarely developed.

So far are these experiments from establishing a distinction in the manner of development of the peculiar states of iron and bismuth, that they will appear from what follows to show more clearly the identity of the two cases.

The inactive state of iron is more readily produced by simply bringing it into contact with a platina surface than by making it the positive pole of a *couronne des tasses*; for in the former case, the action of the acid may be arrested after it has already commenced [*], while, in the latter case, it is essential that the iron should be connected with the battery before it is introduced into the acid [†]. If a more powerful battery is employed, the iron acting as positive pole has been shown by Dr. Faraday to be actually oxidized and dissolved while the current is passing [‡]; but as M. Schœnbein attributes the trace of iron which he has himself sometimes discovered in the liquid, to the action of the acid vapours upon the part of the iron above the acid, and the conduction of the nitrate thus formed down into the acid by capillary attraction [§], I thought it necessary to repeat the experiment in such a manner as to obviate this objection. This was easily effected by attaching a small piece of iron wire to a fine wire of platina, and immersing the former completely beneath the surface of the liquid, or by coating an iron wire with glass and exposing simply a section of the wire to the acid. When the iron thus adjusted was made the positive pole of a battery of 20 pairs of plates in moderate action, it began to dissolve at a very perceptible rate (oxygen gas being, however, at the same time evolved from its surface), and a black crust of insoluble oxide was usually formed, when the connection with the battery was broken. This result was obtained with different specimens of nitric acid from the sp. gr. of 1·47 to that of 1·5, yet such acids have no action whatever upon iron alone; so that the passage of an electrical current of sufficient intensity is capable of becoming the cause of the solution of iron when acting as the positive pole. The manner of closing the circuit produced no difference in the result.

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[*] Faraday, Phil. Mag., vol. ix. p. 58.

[†] Schœnbein, *Ibid.*, p. 55.

[‡] *Ibid.*, p. 62—3.

[§] See Phil. Mag., vol. x. p. 173.

It appears therefore, from these observations, that the passage of an electrical current of a certain intensity renders iron and bismuth inactive in acids capable of dissolving them, while the passage of a current of a higher intensity causes their solution in acids which otherwise have scarcely any action upon them. It is true that the required intensities of the currents for these objects are different for each metal, but this necessarily follows from the difference in their chemical relations to nitric acid. But although the peculiar state of the two metals thus appears to be developed by the same cause, it must be carefully observed, that while the chemical action of the acid upon iron is entirely destroyed, its action upon bismuth, and all the other metals which I have examined, except perhaps tin, is only greatly retarded. This distinction, important as it is, does not appear to me to be sufficient to prevent us from referring all the phænomena to the same general principle. As to the circumstance of the peroxide of lead not protecting bismuth while it protects iron, I have only to observe, that this substance has so little tendency to attach itself to the surface of bismuth, that I have never been able to succeed in coating properly that metal with it; and when I endeavoured to employ iron coated with the peroxide to protect bismuth in nitric acid of sp. gr. 1·4, the peroxide generally separated leaving the surface of the iron exposed.

Concentrated nitric acid immediately develops the peculiar state of bismuth, as well as of iron, and when a small portion of bismuth is left in nitric acid of sp. gr. 1·5, its solution will occupy some weeks, just as happens when, in the peculiar state, it is kept in nitric acid of sp. gr. 1·4. But even in the same acid and at the same temperature, it is remarkable what apparently trivial circumstances are capable of determining these two states in bismuth, and the facts which I have now to describe are certainly among the most singular to which this inquiry has led.

If a small fragment of bismuth (half a grain, for example,) is introduced into nitric acid of the sp. gr. 1·4, at the temperature of 40° or 50° Fahr., and allowed to remain at rest, it will usually dissolve in a few seconds, with the disengagement of orange fumes; but sometimes, after the solution has proceeded to a certain extent, it will suddenly cease, and the bismuth will be found in the peculiar state. This latter effect will be more frequently obtained by agitating the liquid, so as to bring a fresh surface of acid into contact with the bismuth. But the peculiar state is never produced in this way till the original surface of the bismuth has been dissolved away, and a fresh surface exposed to the acid. It is easy, however, to procure a surface of bismuth, which will always be inactive, even when first introduced into acid of the above strength, not only at the temperature of 50° Fahr., but even at that of 80°, at which degree unattached fragments of bismuth always dissolve with great rapidity. For this purpose all that is necessary is to fill a glass tube about $\frac{1}{12}$th of an inch in diameter with fused bismuth, and then to file it across, so as to expose a circular section of bismuth to the acid. The surface thus obtained was always found to be in the peculiar state on its first immersion in the acid. The greatest care was taken to render the surfaces of the unattached fragments perfectly similar by filing, and to bring all to the same temperature.

Are we to suppose that in this case the glass acts the part of an electro-negative metal, and induces the peculiar state by developing an electrical current? This supposition appears extremely improbable, and some experiments which I made in reference to this view were unfavourable to it. The influence of the glass is most probably mechanical, as the plane surface of the bismuth alone exposed to the acid opposes its solution, and thus develops the peculiar state. It must be acknowledged, at the same time, that there is some difficulty in supposing that so slight a mechanical difference should have the effect of arresting a powerful chemical action.

The phænomena presented by the other metals agree in their general features with those already described, although they differ slightly in some of the details.

The peculiar state of tin closely resembles that of iron. Nitric acid of sp. gr. 1·5 exerts no action whatever upon tin; at least I have preserved that metal in acid of this strength for several weeks, and its surface still remains untarnished*. If a pencil of tin is dipped into nitric acid, sp. gr. 1·47, at 50° Fahr., it is immediately attacked and its surface becomes densely coated with peroxide; but if the acid is placed in a platina vessel, with which the tin has been connected before immersion, the acid will no longer act upon it, and on breaking contact the tin will be found to be inactive. This metal may also be rendered inactive, by being made the positive pole of a battery of a certain strength. It resists better the solvent action of a current of higher intensity, and opposes a greater obstacle to its passage than iron or bismuth.

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* In Dumas's *Traité* (vol. i.) it is stated that nitric acid of sp. gr. 1·5 acts violently upon tin, which is insoluble in acid of sp. gr. 1·48. This is certainly a mistake, provided pure nitric acid is employed.

To the facts before stated in reference to copper, it may now be added, that nitric acid of sp. gr. 1·5 develops in this metal also the peculiar state. In this state, it is slowly soluble. When alone in acid of sp. gr. 1·47, there is at first violent action, then the copper acquires the peculiar state: when connected with platina it acquires at once that state; and so long as the contact with the platina is maintained, the copper retains a bright lustre; but when it is broken, the surface of the metal becomes covered with a black film, which can only be partially dissolved by the acid or by renewing the connection with the platina.

A permanently peculiar state cannot be produced in zinc; but by connecting it with platina, or making it the positive pole of a pile, its solution may be greatly retarded, so long as the current continues to pass.

In taking a general view of this subject, it is necessary to distinguish the modification which the action of the acid undergoes, while the metal forms part of a voltaic arrangement, from the permanent modification which continues after its connection with the battery has been destroyed. Having extended my inquiries to other oxy-acids, I find that the chemical action of those acids in a concentrated state upon the metals is diminished by voltaic associations. The effect of galvanic combinations upon chemical action may be thus generally stated.

The contact of an electro-negative metal increases the ordinary action of an oxy-acid upon an electro-positive metal, if the acid is so dilute that the latter becomes oxidized from the decomposition of the water; and retards or arrests that action, if the acid is so concentrated that the metal is oxidized from the decomposition of the acid itself[*].

Thus, in the case of the sulphuric acid, if hydrogen gas is disengaged at the surface of the platina, in a voltaic combination of zinc and platina, the ordinary solution of the zinc will be greatly accelerated by contact with the platina; but if sulphurous acid is set free at the platina surface, then the solution of the zinc will be, on the contrary, greatly retarded.

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[*] To the first part of this law an exception, which is perhaps rather apparent than real, occurs in the action of some dilute acids upon iron under certain circumstances; to the second part, which I believe has not been before observed, I have yet met with no exception.

The experiments by which the latter part of this law has been established are contained in a paper which will shortly be presented to the Royal Irish Academy.

In reference to the peculiar state of the metal in nitric acid, after it is separated from the voltaic influence, it may be useful to remark, that the more completely it is developed and the more perfectly inactive the metal becomes, the brighter is the surface which it presents to the liquid; and as Faraday has shown that the remarkable properties of a platina plate, when polarized by acting as the positive pole of a battery, depend upon the absolute cleanness and purity of its surface, is it not probable that, in like manner, the inactive states of these metals depend upon the pure metallic surfaces which the voltaic action develops by dissolving away every trace of oxide, upon which surface, when thus more perfectly freed from all impurity than can be effected by mechanical means, the acid has either no action or a greatly diminished action? This is merely however stated as a simple conjecture, and I can offer no explanation of most of the particular facts which have been described. I am, Gentlemen, yours, &c.

Belfast, Feb. 6, 1838. THOMAS ANDREWS.

XLVII. *Further Experiments on the Current Electricity excited by Chemical Tendencies, independent of ordinary Chemical Action.* By *Professor* SCHŒNBEIN.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

SOME time ago I communicated to Dr. Faraday a letter, in which the results of my late researches on the voltaic relations of some metallic peroxides, inactive iron, and platina were made known to that distinguished philosopher. As I understand that my letter has been, or is about to be published in your valuable Journal*, you will perhaps be kind enough to give a place also to this paper in one of the next Numbers of the Magazine.

After having ascertained that the peroxides of silver and lead, as well as inactive iron and platina, being voltaically associated with one another, are capable of exciting current electricity quite independent of what is commonly called "chemical action," I extended my experiments to a series of other substances, which, according to the assertion of all chemists, do also not chemically act upon each other, for instance, to zinc, cadmium, tin, iron, copper, mercury, silver on the one side, and water on the other. By associating any one of the metals named with platina, and putting such a combination into water entirely deprived of air and chemically pure in every other respect, I always obtained a current which passed from the oxidable metal through the fluid to the platina, and the strength of which seemed, generally speaking, to be proportional to the degree of affinity of that metal for oxygen. The deviation of the needle caused by zinc, for instance, was greater than that occasioned (*cæteris paribus*) by copper. But even the strongest of the said currents proved to be so weak as to require the most delicate galvanometer in order to be made perceptible.

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* Prof. Schœnbein's letter to Mr. Faraday here alluded to appeared in our last Number, p. 225.

The instrument with which I made my experiments is provided with 2000 coils. To ascertain whether a current excited by chemical tendencies is possessed of any electrolysing power, I constructed a pile of substances which, being in contact, do not cause any sensible chemical action, but which have a strong tendency to unite chemically with one another. In my last letter to Prof. Faraday I stated the fact, that inactive iron voltaically associated with platina excites a continuous current when put into nitric acid, and that this current is apparently quite independent of any chemical combination or decomposition, the inactive iron retaining its metallic lustre, and the nitric acid containing no perceptible quantity of nitrate of iron, however long the voltaic pair may have been immersed in the acid fluid. On account of this remarkable property of iron I formed the voltaic arrangement in question, of this metal, platina, and nitric acid of 1·35 sp. gr. Nothing, indeed, can be easier than the construction of such a pile. Having joined one of the ends of a common iron wire to one of the ends of a platina wire, prepared the number desired of such associations, and filled a corresponding set of cups with nitric acid of the above-mentioned strength, I first put the free end of the platina wire of one pair into the exciting fluid, and afterwards immersed in the latter the free end of the iron wire belonging to the same pair. According to my now well-known experiments, iron turns inactive in the circumstances here mentioned. The iron wires of all the pairs being rendered chemically neutral after the manner indicated, and arranged into a "*couronne des tasses*," the pile is ready for use. The one I employed in my experiments consisted of a dozen of pairs. If the extremities of such a pile are connected by the ends of the wire of a very delicate galvanometer, the needle is affected so as to indicate a continuous current passing from iron to platina. Acidulated water being exposed to the action of this current is no more electrolysed than sulphate of copper is. On a mixture of iodide of potassium and gelatinous starch being put in connection with the electrodes, signs of decomposition are certainly exhibited, a very small and slightly coloured spot making its appearance round the positive electrode, which spot, however, does not sensibly increase either as to size or intensity of colour, however long the action of the pile may last. From the facts stated it seems as if a current excited by mere chemical tendency has the power of decomposing at least one electrolyte; but from reasons I shall now indicate I think such an inference inadmissible. Although there is apparently no chemical action going on in the pile described, still such action takes place to a certain degree at those parts of the iron wires which are placed immediately above the level of the acid. That such is really the case appears from the fact, that after some time the parts mentioned are losing their metallic lustre and covering themselves with a brownish film. This chemical action, slow and insignificant as it is, must originate a current, and consequently increase the intensity of that current, which is produced by the tendency of inactive iron to unite with oxygen. Now as the united strength of the two currents is hardly capable of decomposing traces of iodide of potassium, I think from this fact we may safely draw the conclusion, that the current which results only from chemical tendencies cannot of itself cause electrolyzation. Some might, perhaps, be inclined to think the whole current of my iron-platina pile produced by that slow oxidation of the iron wires which takes place at and above the level of the acid, but by most conclusive experiments I have ascertained that such is not the case. But if, on the contrary, we suppose the current of our pile to be wholly due to chemical tendency, it can easily be shown that the electrolyzing power of a current proceeding from an indubitable, (i. e. visible) chemical action, surpasses by far the decomposing force of that current which is produced by my tendency pile. Let one only of the inactive iron wires of the latter be thrown into chemical action, (for instance, by touching it with a piece of any active metal,) and a remarkable change in the action of the pile will take place; whilst the latter, on having all its iron wires inactive, colours but very slightly the starch impregnated with iodide of potassium: no sooner has one of those wires been rendered active, than a large and deeply coloured spot is produced round the positive electrode. It is a matter of course that the electrolyzing action of the pile upon the iodide will be increased by augmenting the number of active wires. Indeed, a pile containing a dozen of active wires readily decomposes, not only iodide of potassium, but also any other electrolyte. Now, if there be any means of demonstrating, as it were *ad oculos*, the intimate connection which exists between the chemical action of a pile and its electro-chemical power, i. e. current, my iron-platina arrangement, I think, must be considered as such; and on this account alone, if on no other, it may, perhaps, be recommended to the attention of scientific men. If the views of Volta on the origin of current electricity were true, it is obvious that the pile in question ought to be, comparatively speaking, a very powerful arrangement, iron being what is called an eminently electro-positive metal, platina an electro-negative one, and nitric acid one of the very best conductors we know of.

As what I term a current of tendency is no doubt in some cases nothing but that electrical state which the voltaists consider to be the effect of their "force electro-motive," or of contact, it appears to me, that from some of the facts above stated a specific and most important conclusion regarding the theory of the pile can be drawn. Even if we grant to the voltaists our current of tendency to be the effect of mere contact, the facts alluded to prove that such a current does not possess a sensible degree of electrolyzing power, consequently that the chemical effects of the common voltaic arrangements have nothing to do with current electricity excited by contact. The important discovery made by Mr. Faraday with regard to the connection which exists between the magnitude of the electrolyzing power of a pile and the quantity of metal oxidized within the voltaic circle, beautifully agrees with the results I have obtained from the experiments above stated. I would not have said so much upon the relation of chemical action and current electricity to one another, this subject having been so beautifully and satisfactorily cleared up by the skill and sagacity of Mr. Faraday; but as I understand that the truth of the chemical principle of galvanism will, before long, be seriously denied by a German philosopher, and that facts will be brought forward which are said to be altogether irreconcilable to the chemical theory and quite in favour of Volta's hypothesis, I thought the preceding remarks not superfluous. I cannot deny my being very curious to learn the novel facts alluded to, for certainly they must be of quite an extraordinary nature; but of whatever kind they may be, I am almost sure the foundation of the chemical system will not be shaken by them.

Before concluding my letter, I must not omit to say some words about the decomposition which peroxide of lead undergoes when voltaically associated with platina and put either into nitric acid or a solution of blüe vitriol. In the first case nitrate of lead is formed, in the second sulphate of lead. The said peroxide and platina being substances each separately quite indifferent to any of the fluids named in a chemical point of view, how are we to account for the deoxidation of the peroxide? No doubt can be entertained that in the circumstances mentioned a decomposition of water takes place, and that the hydrogen of the latter unites with one of the two equivalents of the oxygen contained in the peroxide. But by what means is water decomposed? It seems to be effected by a current. As stated in my last letter to Dr. Faraday, there is, indeed, a current in the case; but in the beginning at least it is a current of tendency, and as such, according to the preceding statements, incapable of causing chemical decomposition. Upon the first view of the matter we can hardly help thinking the case in question to be analogous to that which is offered by distilled or amalgamated zinc, platina, and dilute sulphuric acid, that is to say, we are inclined to suppose that the hydrogen of water bears in one case the same relation to some part of the oxygen of the peroxide, as the zinc does in the other case to the oxygen of water, and that the incipient currents in both instances are due to similar causes. But upon a nearer view, I think, we must give up such an opinion. In the first place, it can hardly be denied that there is some sensible chemical action taking place between distilled zinc and acidulated water, previously to any sort of a circuit being established; such, however, is decidedly not the case with regard to nitric acid and peroxide of lead. In the second place, one is really at a loss to imagine any reason, why the hydrogen of water should tend to unite with the oxygen of the peroxide, in order to form water again. The affinity of hydrogen for oxygen being satisfied, it would be, if I am allowed to speak in a metaphorical manner, on the part of the former a most wanton capriciousness and groundless love of change, should it try to leave one particle of oxygen to combine with another one of precisely the same kind. As to zinc, which is in a single state, its tendency to unite with the oxygen of water is quite natural, and as it were legitimate. According to my opinion, there is only one way left to account for the very remarkable decomposition which peroxide of lead undergoes in the above-mentioned circumstances. By putting a voltaic association of peroxide and platina into nitric acid, a current of tendency is excited, which, as already stated, passes from platina through the fluid to the peroxide. This current, though it be too weak to cause any electrolyzation, nevertheless acts upon the particles of water placed between the electrodes, in such a manner as to turn their hydrogen side towards the cathode (peroxide), the oxygen side towards the anode (platina), and to diminish or destroy the chemical attraction mutually exerted by the oxygen and hydrogen of each atom of water. Now if we admit of such a state of things, it is not difficult to conceive how the hydrogen of that particle of water which is contiguous to a particle of peroxide of lead, can combine with one proportion of the oxygen of the latter (peroxide). The second equivalent of oxygen of this substance being of itself rather loosely united to lead, and having its affinity for the said metal still more weakened by the tendency of the acid to unite with the protoxide of lead, we may consider the second equivalent of oxygen as almost free, and consequently as endowed with a great affinity for hydrogen. Being in such a condition it can easily unite with the hydrogen of that particle of water which is in immediate contact with the peroxide, and the combination of the two elements will take place the more readily, that the hydrogen is, from the reasons above stated, likewise in almost a single state. The oxygen set free from the said particle of water must, from obvious grounds, combine with the hydrogen of the second particle, the oxygen of the latter with the hydrogen of the third, and so on, until the whole row of particles of water placed between the electrodes have, as in common electrolyzation, undergone a similar decomposition and recomposition. Oxygen must, of course, be at last disengaged at the positive electrode. According to the chemical principle of galvanism, the deoxidation of the peroxide, or rather the oxidation of the hydrogen of water, ought to excite a current, being of itself capable of electrolyzing water. There is no doubt that such a current is produced, but from the fact which I am going to state, it appears that its electrolyzing power is in some way or other counterbalanced. Supposing one end of a platina wire to be covered with peroxide of lead and the other end left free from this substance; put both ends of the wire into a solution of blue vitriol, but the first so as to make the fluid rise a little above the peroxide. My experiments having shown that in these circumstances the platina end is to the peroxide end like zinc to copper, it is obvious that if a current were circulating in the arrangement on account of the deoxidation of the peroxide, it would enter both the immersed portions of the negative end of our platina wire, the metallic part as well as that covered with peroxide. Such being the case, hydrogen ought to be set free at the said metallic part, or rather copper (by secondary action) eliminated at it. But no perceptible trace of this metal is deposited there, however long the voltaic pair may act upon the copper solution. Now we ask, by what means is the electrolyzing power of the current, which proceeds from the deoxidation of the peroxide, counterbalanced? No doubt by a second current, which as to direction is opposite, and as to strength equal to, the first current. But whence such a second stream? I do not know any source to which it can be assigned but to the union of the sulphuric or nitric acid with the protoxide of lead, though I am well aware of Mr. Faraday's high authority being against such a supposition, which does not allow this sort of chemical action to be a cause of current electricity. Nobody knows better than I do that the remarkable voltaic action of the peroxides is very far from having been sufficiently cleared up by my researches; I hope, however, that the interesting subject will be taken up by abler hands than mine; and I particularly wish that the British philosophers, who generally take so lively an interest in everything relating to electrical science, would engage themselves in investigating the matter still further.

Your most humble and obedient servant,

Bâle, Feb. 18, 1838. C. F. SCHŒNBEIN.

XLVIII. *Notes on Repulsion by Heat, &c. By the Rev.* BADEN POWELL, *M.A., F.R.S., F.G.S., Savilian Professor of Geometry, Oxford.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the Philosophical Transactions* for 1834, part ii. there is inserted a paper in which I described an experiment for establishing without difficulty or ambiguity the existence of a repulsive power exerted between heated surfaces at small though sensible distances, viz. at those intervals at which the colours of thin plates are formed; the repulsive effect being indicated by the instantaneous descent of the tints in the order of the scale, and rapid contraction and final disappearance of the rings, between lenses or plates of glass. This method I showed was at once decisive and simple; the "warping" or change of figure (if any) in the glasses by heat being readily seen to be such as ought to cause the rings *to enlarge* at the first instant. It seemed to me a method easily applicable to a number of other points of inquiry connected with the subject; and to many such topics I began at that time to turn my attention.

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* See also Reports of British Association, vol. iii. (or Fourth Report) p. 549; and Journal of Franklin Institute, U.S., Feb. 1836. [and L. & E. Phil. Mag., vol. vi. p. 58.]

I regret to observe in a synopsis of the analogies of light, heat, &c. in the British Annual of this year, this property of repulsion is mentioned as doubtful, without any reference to my experiments; which is the more remarkable, as an account of them was given in one of the early Numbers of the Records of General Science, by the same Editor.

The subject is evidently one which may have extensive bearings on a variety of points connected with the study of *molecular forces*. On these it was once my intention to have entered: but various circumstances have led me to discontinue such researches, more especially the superior attractions presented by the study of physical optics, and the observations and calculations connected with the verification of the theory of dispersion, in which I have been, and am likely to be, engaged. This subject at present engrosses all the time and attention I can devote to physical research, more especially in the present stage of its advance: it seems to call for a comprehensive review and systematic methodizing of the principles of the undulatory theory; and in attempting something towards the great work of *a systematic treatise on the* MECHANISM OF LIGHT, I am now likely to find ample employment, probably for some years to come.

Under these circumstances, in looking over and destroying a number of old papers, I thought that perhaps a few rough memoranda of trials of experiments, and hints for others, connected with the subject of *repulsion by heat*, which had been thrown aside formerly in the hope of some day resuming the subject, might possibly be not altogether unworthy of preservation, if they should only have the effect of drawing the attention of any experimenter to the subject, and inducing him to pursue it. With this object I thought the best course would be to request your insertion of the few following notes of this kind in your Journal; trusting that your readers will look upon them in no other light than as suggestions of experiments rather than as definite experimental investigations.

(1.) Most of these experiments were performed by forming the rings simply between two plates of glass and not with lenses. It is very easy in this way to obtain rings of tolerably regular form. The *warping* of the glasses by the heat has been alleged as likely to interfere with the results; but I have observed above (as indeed a very little consideration will sufficiently evince) that this cannot produce any effect. But further, according to the experiments of Sir David Brewster on the polarized tints developed by heated glass, it appears that the heat is transmitted instantaneously through the glass, the system of colours appearing complete at once. This would show that in point of fact *no warping* at all takes place: the two opposite surfaces are simultaneously heated.

(2.) I made some trials for ascertaining the *time* required for the communication of heat through two glasses, when at different small intervals of separation, as marked by the appearance of different tints of the thin plates. A delicate mercurial thermometer was placed with its bulb resting on the upper plate exactly at the point where the central tint was formed. Heat was applied below by the flame of a spirit-lamp playing against the under side of an iron plate on which the glasses rested. The time was noted on which the thermometer rose 1° Centigrade under the different circumstances; the results were as follows:

	Tint at the commencement.	Time of rising 1° Centigr.	
		1st Set.	2nd Set.
Exp. 1.	Black.	40 sec^{s}.	30 sec^{s}.
2.	1st. Yellow.	45	35
3.	No colour.	60	45

(3.) To compare the transmission of heat when the glasses were kept in close contact by forcible compression by means of a wedge, &c., and when loose, the following results were observed with a thermometer touching the upper glass:

	Time of rising 1° Centig.
Glasses compressed	30 sec^{s}.
Glasses loose	40 sec^{s}.

(4.) In these cases part of the heat is employed in separating the glasses by repulsion, part communicated through them. It would form an interesting subject of inquiry to examine the law by which this is regulated. Many questions naturally arise as to the connection of such results with the communication of heat and expansion in general,—and thus with the specific heat of bodies, and their expansion in different directions, especially in crystals. Perhaps some results might be obtained by comparing the transmission of heat through crystals in the direction of cleavage and in that at right angles to it, especially those which split into laminæ, as sulphate of lime, &c.

(5.) When a large central black spot is formed, the attraction is so great that it seems impossible to overcome it by the repulsion of the heat. In one experiment a large black spot was formed, and heat being continued for 3^{m} 30^{s}, no change had taken place, when the upper glass cracked, but not so as to disturb the colours: they continued unaltered for 30 seconds more, when the heat was withdrawn.

A thermometer placed with its bulb on the central black, rose 1° Centigr. in the first 30 seconds of this experiment.

Another experiment was continued to see how long the central black would remain. After about 4 minutes the lower glass cracked, but not so as to disturb the black; at 5^m 30^s it disappeared, and the other colours rapidly vanished: in this case the flame was applied closer to the iron plate than in the former.

(6.) The repulsion between two plates at the small intervals here employed, may be compared with the repulsion between the molecules of the solid body which produces expansion.

In experiments of this kind we have an interval (τ) between the glasses, which is increased to ($\tau + i$); by a given increase of temperature we can easily compare the ratio of this increase with that of the glass itself by expansion for the same increase of temperature.

A thermometer having its bulb kept in contact with the lower glass, (the colours being formed close to the edge of a smaller glass laid upon it, and the bulb of the thermometer being close to the same point,) the increase of temperature was noted which reduced the tints from the bright of the first to that of the fourth order.

In several trials, an increase of only 2° Fahr. was sufficient to produce this effect.

Here $$\tau = \frac{1}{178000} \text{ inch},$$

and $$i = 6\,\tau \quad \text{or} \quad \tau \text{ becomes } \tau\,(1+6).$$

From the experiments of Lavoisier and Laplace the dilatation of plate-glass for a difference of 180° Fahr. is 1·00089, or in this case $\tau_{\prime}$ becomes $(1 + {\cdot}00089)\,\tau$, consequently for a difference of 2° τ becomes $(1 + {\cdot}000098)\,\tau$.

The ratio in the two cases $$\frac{i}{i'} = \frac{6{\cdot}000000}{{\cdot}000098} = \frac{61223}{1} \text{ nearly.}$$

(7.) I made some rough attempts towards ascertaining the *law* by which the repulsive effect of heat between the plates of glass may be regulated, in relation to the interval between the plates. Though these results are hardly of sufficient precision to justify any decisive inferences, yet they may be mentioned, as perhaps the experiments may be thought not unworthy of being repeated. The process consisted in observing the times of the central tint changing to another given tint, commencing from different points in the scale, with a constant heat.

The results of two sets of experiments were as follows:

Change.	Interval. Multiple of $\frac{1}{178000}$ inch.	Time. Mean of two Experiments.
From maximum of 1st order to max. of 4th	6	$11^{s}\cdot5$
———— 2nd to 4th	4	7·5
———— 3rd to 4th	2	3·5
From minimum of 1st order to min. of 4th	6	8·5
———— 2nd to 4th	4	4·75
———— 3rd to 4th	2	3·

In all such experiments the most troublesome part consists in having to wait in each instance until the apparatus has cooled down to the same temperature, before commencing. This and numerous other precautions are not here dwelt upon, as they will soon suggest themselves to those who may be induced to enter upon such experiments.

XLIX. *On the Nature and Properties of Teriodide of Chromium. By Mr.* Herbert Giraud, *F.B.S.E., Mem. Med. Soc., Edin.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

ALTHOUGH the remarkable and beautiful combinations of chromium with chlorine and fluorine are well known to every chemist, yet I am not aware that any iodide of chromium has hitherto been formed; therefore a short account of one which I have recently produced may not be uninteresting.

When we consider the relative electro-chemical condition of iodine compared with that of chlorine or fluorine, we are led to conclude that if any combination can be effected between chromium and iodine, such a combination would be held together by weaker affinities than those which are exerted in the analogous compounds of chromium with chlorine or fluorine; accordingly we find that the compound we are about to describe is subject to decomposition from slight causes. The method adopted for the production of this compound was similar in character to that which is generally made use of for the formation of the terchloride or terfluoride of chromium.

33·5 grains of chromate of potassa were intimately mixed with 165·45 grains of dried iodide of potassium (the quantities being in the proportion of 3 equivalents of iodide of potassium to 1 of chromate of potassa); these materials were then introduced into a tubulated retort, to which a receiver was adapted; about 70 grains of fuming sulphuric acid were then poured upon the materials in the retort; this instantly gave rise to intense chemical action, accompanied by the evolution of much caloric and the production of heavy garnet-coloured fumes, which constitute the iodide of chromium in a state of vapour: the elevated temperature already induced was sustained by means of a spirit-lamp, and the fumes continuing to come over were condensed in the neck of the retort and in the receiver. A small proportion of free iodine and also of sulphuric acid were carried into the receiver: the products remaining in the retort were sulphate of potassa and green sulphate of the oxide of chromium.

In every attempt which I have made to procure this substance, with varied proportions of the materials, I have never been able to obtain it quite independent of a small proportion of free iodine and sulphuric acid; it therefore appears, that it is not essential that the materials should be employed in the exact proportion of their equivalents. The essential changes which occur in the formation of this substance are expressed by the following formula:

$$3\,\mathrm{KI}, (\mathrm{KO}+\mathrm{CrO}^3) \text{ and } 4\ \mathrm{SO}^3 = 4\,(\mathrm{KO}+\mathrm{SO}^3) \text{ and } \mathrm{Cr\,I}^3.$$

The teriodide of chromium, like the other compounds of that metal, is remarkable for the brilliancy of its colour, which is of a deep garnet hue; it is a fluid of an oily consistence, heavier than water, converted into a dense vapour, possessing the same colour as the fluid, at a temperature of about 300° Fahr.; when exposed to the air it attracts moisture, and thence gives rise to watery fumes; by mixture with water it is resolved into chromic and hydriodic acids; it destroys organic substances, gives a black colour to paper and wood, stains the skin of a deep and permanent brownish-red colour, and destroys the cuticle; it is also destructive of animal and vegetable life.

If the teriodide could be obtained free from adhering sulphuric acid, its analysis would be easily effected and its composition determined by means of a soluble salt of lead, a solution of which being added to the teriodide of chromium gives rise to iodide of lead and chromate of the oxide of lead, which are readily separable, the iodide being soluble in boiling water, while the chromate is insoluble; but sulphate of lead is formed, which is also insoluble. It unfortunately happens that the degree of heat required for the formation of the teriodide always causes a small quantity of sulphuric acid to be carried over with it.

The proof, however, which we have that this compound is indeed a teriodide of chromium, is afforded by the fact of its being resolved into the chromic and hydriodic acids by the action of water.

I am, Gentlemen, yours, &c.,
Edinburgh, Feb. 5, 1838. HERBERT GIRAUD.

L. *On the Relation between the Number of Faces, Edges, and Corners in a Solid Polyhedron. By* AUGUSTUS DE MORGAN, *Professor of Mathematics in University College.*[*]

THE remarkable relation which exists between the number of edges, faces, and corners (or solid angles) in a solid figure, namely, that the number of faces and corners together always exceeds the number of edges by two, is usually demonstrated by reference to the celebrated expression for the area of a spherical triangle. The theorem was given by Euler, in the Petersburg Acts for 1758; but not having access to that work, I cannot tell whether he employed the method just alluded to, or not. However, since Legendre has derived the theorem by means of the spherical triangle, as well as every other elementary writer with whom I am acquainted; and since an equally simple relation which exists among the edges, corners, and faces of a *portion* of a solid figure has been little if at all noticed, I conjecture that the following demonstration is new. At any rate, it is more simple, and derived from more elementary principles, than the one commonly given, and is therefore worthy of notice.

Let there be a number of polygons, so placed that each has a common edge with one or more of the others. Let every angular point be called a *corner* (whatever may be the number of lines which meet there); every line joining two corners, an *edge*; every unsubdivided portion of space, a *face*. Then the number of faces and corners together will always exceed the number of edges by *one*. For this is evidently true of a single polygon, while for every polygon which is added, the number of new edges is one more than the number

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[*] Communicated by the Author.

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of new corners. But the additional polygon is one new face; therefore the addition of each new polygon increases the faces and corners together as much as the edges; consequently the initial relation, namely,

$$\text{No. of faces} + \text{No. of corners} = \text{No. of edges} + 1$$

remains undisturbed.

The preceding is equally true if the faces be not all in one plane, and consequently it is true of any portion of a solid figure which is not the whole. It is true then of a solid figure from which one face is omitted, the edges of that face answering to the exterior contour in the preceding theorem. Consequently, including the face just omitted, and thus completing the solid, we have

$$\text{No. of faces} + \text{No. of corners} = \text{No. of edges} + 2.$$

LI. *On the received Equivalents of Potash, Soda, and Silver. By* JAMES F. W. JOHNSTON, *A.M., F.R.S. L.& E., Professor of Chemistry, University of Durham.*[*]

IN the progress of the sciences of observation it is interesting to remark how the establishment of every new principle is opposed by certain apparent exceptions or anomalies, yet how these anomalies all ultimately disappear, and how the removal of each is necessarily preceded either by some correction of received opinions, or by fresh additions to our actual knowledge.

The researches of Dulong and Petit into the specific heats of the metals rendered it extremely probable that the atoms or equivalents of these elementary bodies had the same specific heat. This relation was found to hold with remarkable exactness in the case of a considerable number of metals; but to bring silver, gold, and mercury into conformity with the law, it was found necessary to reduce by *one half* the atomic weights of these metals as generally received among chemists. The result of these beautiful researches therefore, though they pointed to and rendered probable the existence of a very simple relation among the specific heats of the metals as a law of nature, yet being opposed by so many apparent exceptions, have not been generally received with that confidence to which they may ultimately prove to be entitled. Chemists have been unwilling to alter the received atomic weights of so many metals for the sole purpose of bringing them into conformity with a relation still considered hypothetical.

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[*] Communicated by the Author.

In the progress of discovery, however, other facts were observed, which though at first appearing to oppose the law in question, have yet been preparing the way for the removal of the exceptions which opposed its general reception. Among the admirable observations of Mitscherlich on the isomorphism of analogous compounds was the identity of form of the sulphate and seleniate of silver with the anhydrous sulphate and seleniate of soda from which the isomorphism of silver and sodium was deduced; and as the same quantity of sulphuric acid existed in these compounds in union with the received atomic weights of the soda and oxide of silver, the opinion was strengthened that these weights were correct. Professor Gustav Rose more recently established the identity of form of silver and gold; and though his analyses of native gold from different localities did not confirm those of Boussingault, that the two precious metals in native crystals replaced each other in atomic proportions, yet their isomorphism connected those metals by another link, and tended to strengthen the received opinion in regard to the equivalent weights of both. Thus far the progress of discovery was opposed to the results of Dulong and Petit.

When by the sagacity and elaborate researches of Berzelius the doctrine of the sulphur salts was established to the satisfaction, at least, of the German chemists, who understood it best, Professor Henry Rose entered upon the study of the various compound metallic sulphurets which occur so abundantly in the mineral kingdom. The results of this inquiry, while they confirmed the beautiful doctrine of Berzelius, threw an unexpected light on the nature of these sulphurets, and gave a simplicity to their constitution wholly unthought of; and while they made known the existence of many isomorphous relations which were to be anticipated from those already observed among the analogous *oxygen* compounds, they brought to light others also which could not even be suspected to exist. To one of these only, having reference to the atomic weight of silver, it is necessary at present to advert.

The sulphuret, oxide, and chloride of silver are composed respectively of

	Silver.	Sulphur.	Oxygen.	Chlorine.
1 Atom each	13·5	2·0116	1·000	4·4265

The analogous compounds of copper consist of

	Copper.	Sulphur.	Oxygen.	Chlorine.
1 Atom each	7·9	2·0116	1·000	4·4265

and the disulphuret of 15·8 + 2·0116.

Now in certain compound metallic sulphurets the sulphuret of silver is replaced by the disulphuret of copper, and not by the native sulphuret consisting of 7·9 copper + 2·0116 sulphur.

Thus in Fahlerz (gray copper), for which the general formula is

$$\dot{R}^4\,\underline{\dddot{R}} + 2\,\underline{\dot{Cu}^4}\,\underline{\dddot{R}} \text{ or } \dot{R}^4\,\underline{\overset{'''}{R}} + 2\,\dot{Ag}^4\,\underline{\overset{'''}{R}},$$

the first representing the copper and the second the silver Fahlerz, we have identity of crystalline form and identity of chemical formula, if it be admitted that the compound $\dot{Ag}$ is capable of replacing the disulphuret $\underline{\dot{Cu}}$, and that these two minerals are varieties of the same species. It is so far favourable to this view, that though native crystals of disulphuret of copper ($\underline{\dot{Cu}}$) occur in rhomboids of 71° 30′ nearly, yet that by fusion of the artificial as was first observed by Mitscherlich, or of the native sulphuret as observed by G. Rose, this compound can be obtained in octohedrons like native sulphuret of silver ($\dot{Ag}$). The occurrence of two substances in any form belonging to the regular system does not, it is true, prove them to be isomorphous, yet in the present case, the several circumstances connected with the forms and apparent mutual replacement of these two compounds are such as to have induced some distinguished chemists to consider their isomorphism as certain. It has however been placed almost beyond doubt by the discovery at Rüdelstadt of a sulphuret of copper and silver in the rhomboidal form of the sulphuret of copper (Rose, Pogg. xxviii. p. 427), and consisting according to Sander (Ibid. xl. p. 313) of equal atoms of either sulphuret ($\underline{\dot{Cu}} + \dot{Ag}$).*

But the isomorphism of two compounds generally implies an analogy in their atomic constitution,—that they are both sulphurets of the same order. If the compound of copper be a disulphuret $\underline{\dot{Cu}}$, that of silver is most probably a disulphuret $\underline{\dot{Ag}}$, and if so the atomic weight of silver must be reduced one half, or to 6·75. We have here therefore an argument in favour of the old result of Dulong and Petit.

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* In stating that it is placed almost beyond doubt, I take for granted that the crystals from Rüdelstadt, of which the analysis is published by Sander in Poggendorff's *Annalen* for 1837, part ii. p. 313, are different from the imperfect crystals measured by G. Rose, and of which an account is given in his crystallography published in 1833, p. 158.

The introduction of this change, however, would render necessary a like change in the received atomic weight of sodium, with the oxide of which in anhydrous sulphate of soda (Thenardite) that of silver in sulphate of silver is isomorphous. Soda, common salt, and sulphuret of sodium must be represented by $\underline{\dot{\mathrm{Na}}}$, $\underline{\mathrm{Na}}\mathrm{Cl}$, $\underline{\overset{,}{\mathrm{Na}}}$.

Nor can the change stop here. Several years have elapsed since Mitscherlich announced the very interesting fact, that the nitrates of potash and soda were isomorphous respectively with arragonite and calc spar, and that they presented the same cleavages (Pogg. *Ann.* xviii. p. 173.); to which Mar afterwards added that the rhomboids of nitrate of soda possessed the doubly refracting structure in a higher degree even than calc spar (*Jahrbuch der Chim. und Phys.*, xix. p. 165). From these observations it was natural to infer that some relation existed between the two alkaline nitrates analogous to the relation between the two forms of carbonate of lime; that like carbonate of lime the nitrates of potash and soda might each be capable of assuming two forms isomorphous each with each, though in ordinary circumstances of temperature, &c. the class of form preferred by each did not correspond; the nitrate of potash *generally* affecting the right rhombic prism, the nitrate of soda the rhomboidal form. The probability of such a relation was strengthened by a comparison of the analyses of chabasie from different localities and by different chemists, from which there appeared strong reason for believing that potash and soda were capable of replacing each other in equivalent proportions.

The alums however presented an anomaly. Nothing positive in regard to isomorphism could be inferred from potash and soda occurring indifferently, and in equal atomic proportions, in similarly constituted crystals of the regular octohedral and cubical forms; they might occur indifferently in such crystals without being isomorphous: but supposing them really to be isomorphous, there appeared an inconsistency in the alleged existence of 26 equivalents of water in soda alum, while potash alum contained only 24 atoms. This objection has been lately removed by Professor Graham, (London and Edinb. Phil. Mag., vol. ix. p. 26,) who has shown that soda alum when perfectly dry contains only 24 atoms of water, and that the potash and soda alums therefore have the same constitution*.

But all doubt has at length been removed from the relation between the forms of potash and soda by a beautiful observa-

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[* The identity of constitution of the potash and soda alums had been previously shown we believe by Dr. T. Thomson.—Edit.]

tion of Frankenheim (Pogg. xl. p. 447). He has found that when a saturated solution of pure nitrate of potash is left in small quantity to spontaneous evaporation two sets of crystals are formed; one in prisms, the other in rhomboids; the former the common arragonitic form generally assumed by nitrate of potash, the latter that of calc spar, commonly assumed by nitrate of soda. The rhomboidal crystals are microscopic and pass into the prismatic form by friction, by pressure, or by contact with a prismatic crystal, and hence when the salt is crystallized in large masses they entirely disappear[*]. It may therefore be considered as demonstrated that the nitrates of potash and soda are at once isomorphous and dimorphous, or isodimorphous; and since the potash and soda replace each other in certain mineral compounds, the alkalies also, perhaps the metallic radicals themselves, may be considered isodimorphous.

Whatever change then we adopt in regard to the atomic weight of silver the same must be adopted for potash and soda. If we halve the atom of the former, potash and soda must be represented as dioxides, K_2O and Na_2O: and if we consider the united observations of Boussingault on the composition of the native alloys of gold and silver from South America, and of G. Rose on the crystalline forms of the native metals in a state of comparative purity to be sufficient evidence of the isomorphism and replacing powers of gold and silver, we must also halve the received equivalent of the former metal; or for the four metals in question we must adopt the equivalents

Gold	$\frac{24{\cdot}9}{2}$	Potassium ...	$\frac{4{\cdot}894}{2}$
Silver	$\frac{13{\cdot}5}{2}$	Sodium	$\frac{2{\cdot}912}{2}$ [†]

of which the former two are the multiples indicated by the researches of Dulong and Petit.

I leave it to British chemists to judge how far the reasons here stated are sufficient to authorize the introduction of the proposed change. In compiling the second part of my tables of chemical constants it has been necessary to subject many points of this nature to a critical examination; and if in these tables I should find it necessary to adopt the smaller multiples for the equivalents of the metals above mentioned I am anxious that the cultivators of chemical science into whose hands they fall may have an opportunity of estimating the united force of the several reasons by which I may have been influenced in doing so*.

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[*] [See Mr. Talbot's Observations, p. 147 of the present volume.—Edit.]

[†] For easy reference I have copied these weights from Turner's Chemistry.

Durham, September 1837.

Note.—It may be in the recollection of many of the readers of this Journal, that Dr. Clarke of Aberdeen, in a letter to Professor Mitscherlich published in the Records of Science, endeavoured to show that the atoms of one or more of the alkaline metals should be *doubled*; in a subsequent paper I propose to consider the principle on which his reasoning is founded.

LII. *On the Path of the projectile Weapon of the Native Australians called the "Boomarang" or "Kylee." By* A CORRESPONDENT.†

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

A SHORT time since my attention was directed to the "*Boomarang*" or "*Kylee*," known for a considerable time to the scientific world, and at present become a very general source of exercise and amusement. You must be aware that when thrown in a certain direction, it will return to, and sometimes far beyond, the spot from which it was projected. Curiosity induced me to devote some attention to the subject; and unable to see any insufficiency in the following simple causes, by which I have endeavoured to account for this curious property, I am surprised that none of your learned correspondents has favoured the public with any explanation of it. Should the following observations appear worthy of the attention of your readers, I am sure you will not refuse them an early insertion in your excellent Journal.

1. To explain the principles on which the kylee ascends, and afterwards returns to the place from which it was projected, it will be necessary to consider the various forces which act on it, from the time it is projected until its return to the earth. These forces appear to me to be five: first, that of projection; secondly, that of gravity; thirdly, the resistance of the air to its plane or level side; fourthly, that resistence to its curved side; and fifthly, to its edge. The last three forces, though distinct in their effects, are but one in their nature or cause. In enumerating these forces, I have supposed your readers to be perfectly acquainted with the form or shape of the body. Let us now examine the effect

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[* How will this subject be affected by the application of the mode of determining true atomic weights pointed out by Mr. Faraday, in his Seventh Series, par. 851; or L. and E. Phil. Mag., vol. v. p. 430? EDIT.]

† A notice of a paper on the Boomarang will be found in our report of the Royal Irish Academy's Proceedings in a future page.

produced by each of these forces separately, and we shall be better able to estimate with precision their conjoint effects.

2. Before the body is projected, it must be observed, that there are a variety of angles with the horizon, at each of which it might be thrown; but, for the present, we shall confine ourselves to two, I mean the angles of projection and inclination. By the former, I understand the angle which the plane of the horizon makes with the direction in which the body is projected; and by the latter, I mean the angle which the plane side of the body makes with the horizon. These two angles should be accurately attended to, as they are quite distinct.

3. When the body is projected in any direction, and at any angle of inclination, say 40° or 50°, the effect of the first force, viz. projection, will be to impel it forward; that of the second, viz. gravitation, to cause it to descend: but the effect of the third force, I mean that which acts on the plane side, will be, first, to counteract and nullify the fourth force, which acts on its curved side, and by this means, becoming itself *superior for a time* to gravitation, its ultimate effect will be to bear the body upwards in a curvilinear direction. Finally, it will cooperate with the fifth force, (the effect of which, inasmuch as it acts on the edge of the body, is scarcely estimable,) in causing the body to gyrate around its axis. This axis is obviously an imaginary line at right angles to the plane surface of the body, and passing through the point on which the body would balance itself, in the position in which its plane surface would be parallel to the horizon; i. e. if the body be supposed for a moment to rest on the paper on its level side, and if the paper be parallel to the plane of the horizon, the position of the axis is a perpendicular to the horizon passing through the centre of gravity of the body while in this position.

4. It is easy to perceive that the body will ascend in a curvilinear direction, if the five forces above mentioned are capable of producing the different effects which I have attributed to them. That the projectile force will impel the body forward, and that of gravity downward, cannot be questioned. Not so the effects ascribed to the other three. I shall therefore proceed to explain whatever may appear not sufficiently satisfactory in my assertions regarding them. The third force above mentioned is the resistance of the air to the plane or level side of the body. This force, if not twice as great, is at least much greater than the force acting against the curved side. This truth will appear evident, if we consider attentively the following facts.

5. If a globe and cylinder of the same diameter move in an uncompressed fluid in the same direction, viz. that of the axis of the cylinder, the base of the cylinder suffers twice as much

resistance as the globe. See Sir Isaac Newton's *Principia*, lib. ii. prop. 34; also Vince's Hydrostatics, prop. 29. From page 11 of App. to last-mentioned work, it appears that experiments prove the resistance aforesaid to be even in a greater ratio than theory teaches, viz. in the ratio of 2·23 : 1 nearly 2¼ : 1, i. e. 9 : 4. Also, from the same page, we learn that the resistance to the plane side of a semiglobe is greater even than the resistance to the base of its circumscribed cylinder. Hence we may infer that the resistance to a plane surface moving in a fluid is considerably increased by the curvature of the back part of the body.

6. From a careful attention to these facts, we can have no difficulty in admitting that the plane surface of the kylee suffers a resistance, if not twice as great, at least much greater than the curved side; and consequently, since the forces acting on its two sides are in opposite directions, whatever may be the extent of the resistance to the curved side, it is altogether counteracted and nullified by the superior resistance to the level side. And not only this, but we may also see, as I will show immediately, that the excess of the more powerful over the weaker force remains a free agent, unexhausted, and therefore capable of bearing the body upwards for a time in opposition to gravitation.

7. I stated above (3.) that the force which acts on the plane side is for a time superior to gravity; and before I prove the correctness of this assertion, I may remind you, in connection with this point, of the principles by which is explained the cause that makes a boy's kite hover for hours suspended in the air, notwithstanding its greater specific gravity. You are aware that the kite, by constant tension of a cord attached to it, is made to act on the air, the particles of which, since action and reaction are equal and in opposite directions, react on the kite in a contrary direction, and thus support it against gravitation as long as the cord is held tight. 'Tis the same principle which enables a bird to support itself on its wings; and it is worthy of remark, that there is a very striking resemblance in the shape of a bird's wing to the figure of the body in question.

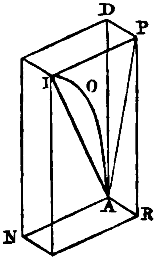
8. I shall now proceed to show that the force acting on the plane surface is for a time superior to gravity, or, which is the same thing, that it acts upwards. When the body is projected in any direction, its plane side making with the horizon an angle of 40° or 50°, and the projector holding the plane side off, and the curved side towards himself, the plane side, by impinging on the first volume of air, generates, as we have seen (5.), a certain resistance to itself, which is in some ratio to the velocity with which it moves (generally admitted to be

as the square). And since both the plane and curved surfaces of the body act simultaneously on the air, and are, therefore, simultaneously reacted on by it; and since this reaction is so much greater (5.) against the plane than the curved side, it follows manifestly that there remains an excess of force acting on the plane side, which will cause it to deviate from the direction of its first motion. And in what direction will it be turned? Evidently in the direction imparted to it by the force acting on it. To ascertain this direction, we must bear in mind, that the body was projected with its plane surface making with the horizon an angle of 40° or 50°; and if we estimate, as we may, (see Wood's Mechanics, Comp. and Res. of Forces,) the total efficient resistance in a direction perpendicular to its plane surface, it is manifest that the direction of the force in question will be a line making with the horizon an angle of 40° or 50°, or in other words its direction in the first instant after projection will be that angle with the plane of the horizon which is the complement of the angle of inclination. Therefore, as this force acts at an angle of 40° or 50° with the horizon, it is an upward force. The same process will be gone through in the second instant, and so on until the velocity of projection is spent; and then the upward impulse, depending for its existence on the continuance of the projectile force, will also cease. Hence this force is, as I said, superior for a time to gravitation.

9. I said that we may estimate the total efficient force which acts on the plane side in a direction perpendicular to that side (8.). Should this be objected to, we may ascertain its real direction. It cannot be forgotten that the pressure on the plane surface is much greater than on the curved; and the particles of air are acting on it in every direction, but certainly not with equal force in every direction. For the particles that impinge on the plane surface with the greatest effect, are those that act on it at an angle of something about 54° with the direction of its motion. My reason for this assertion is, because it is proved (Vince's Fluxions, Max. and Min. p. 24, edition 5.) that the greatest effect produced by the wind on the sails of a windmill is when the wind acts on them at an angle of 54° 44′. Since it is the same thing whether the body moves against the fluid or the fluid against the body (see Principles of Hydrostatics), it follows, that when the body is in motion, those particles which impinge on it at the aforesaid angle with the direction of its motion, resist it more powerfully than any other particles acting on the same side, and consequently have the same tendency to make it deviate from the line of projection as the causes already explained.

10. I have now, as briefly as I could consistently with per-

spicuity, endeavoured to develop the causes by which the body is made to ascend; and before I explain those which effect its return, I shall, with the aid of the annexed diagram, enumerate them, and their effects, for the satisfaction of some of your readers. The figure is a parallelipipedon.



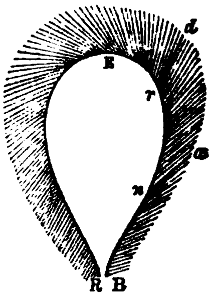
Let the body be projected from A in the direction A D with a velocity which would carry it to D in the same time that it would descend by gravity to R. Also let A N represent in quantity and direction a third force, acting on the body for the same time, and equivalent to the force so often mentioned already as acting on the plane side. I need not say that A N is composed of all the successive forces acting on the plane side of the body, during its motion over different volumes of air in the same time that the projectile force would carry it to D, and gravity to R. The fourth and fifth forces spoken of (1.) are already accounted for, and not estimable here (3.). Since these three forces act on the body at once, it will be found at the end of the given time at I (see Comp. and Res. of Forces,) the solid angle of the parallelipipedon opposite to A; and, because the forces are not uniform, it will not have described the diagonal A I, but a curved line, as A O I. It is evident that the elevation of I, and consequently of the body, will depend on the magnitude of the angles which A D and A N respectively make with the horizon. By drawing the diagonal A P, which is equivalent to the forces of projection and gravity, (see Mechanics, Comp. and Res. of Forces,) we may see that gravity contributes to augment the resistance to the plane side; for combined with the force of projection, it increases the action of, and thereby causes greater reaction to, the plane side.

11. I shall now mention what appear to me the causes of its return. When arrived at its greatest elevation the force of projection has ceased, and with it the forces generated by it. It is evident, however, that although the projectile force is spent, the gyration on its axis will not instantly cease, but continue for a few seconds (First Law of Motion). The only force now acting on the body is gravitation. You may not be aware that the angle of inclination, i. e. the angle which the plane side makes with the horizon, has increased very much, so much indeed that if it was projected at 40° or 50° it may have become 60° or 70°, the cause of which I may have occasion, perhaps, to explain in a future communication. Should it increase so far as to become a right angle, it is obvious that the body would instantly descend by gravitation,

for in that case there would be no supporting power. But let it be granted that it has become only 60° or 70°, and we may easily perceive the effect that will follow.

12. When gravity begins to act on the body in these circumstances, its plane surface impinges on the column of air beneath, the particles of which immediately react on the plane surface, and by this means cause it to deviate from the perpendicular to the horizon, down which it had commenced to descend by the force of gravity. And because the plane surface is, at this juncture, removed from the projector, and that the resistance is against the plane side, the direction of its deviation from the perpendicular will be towards him. This occurs in the first instant of descent, the same in the second, and so on, until its return to the earth; with this exception, that the body descends with a velocity increasing at every point, as it retires from the highest (see Mechanics, Oscillations of Bodies). Hence the amazing velocity with which it moves immediately before reaching the earth. I need scarcely add, that since the two forces causing its return are not uniform, the line of its descent must be curved.

13. An additional cause of its returning is the direction (relative to the line of its motion, and relative to its plane side,) in which those particles impinge on its plane side, which produce a greater effect than any of the other particles acting on the same side. The direction to which I allude is the angle of 54° 44′ mentioned above (9.). The annexed figure will render this much clearer than language can do. B E R is the curve through which it moves; and each of the lines *n a*, *r d*, &c. will represent the angle 54° 44′ at which the most powerful particles already spoken of act on the plane side of the body; for when by the impact of the first series of the most powerful particles on the plane side, the position of that side becomes altered, it is manifest that a change will also be effected in the position, not in the magnitude of the angle, at which the next series of most powerful particles will act on the same side. And because this process continues in the manner represented in the figure, their tendency is evidently to make it return.



14. It will be observed that as the body descends, the angle of inclination gradually decreases, until the plane side becomes parallel to the horizon. The reason of this, and some other observations, which I intended to offer on the na-

ture of the ascending and descending curves, and on the angle at which the body ought to be projected, in order to return, I must defer, as I have already trespassed too far on your very valuable space. Should any of your readers misapprehend my meaning, or look upon anything I have said as requiring explanation, I shall feel happy in affording any additional elucidation in my power.

Belfast, Jan. 20, 1838. B. D.

LIII. *Method of finding the Equation to* Fresnel's *Wave-Surface. By* Archibald Smith, *Esq., Fellow of Trinity College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I WILLINGLY comply with the request of your Correspondent, that I should communicate to your Magazine the method of finding the equation to Fresnel's wave-surface, which was published in the 6th volume of the Cambridge Transactions*. I shall do this as briefly as possible, indicating the principal steps; the intermediate steps will, I believe, offer no difficulty.

If v be the length of the perpendicular from the origin on a tangent plane of the wave-surface and $l\ m\ n$ its *direction-cosines*, a^2, b^2, c^2 the coefficients of elasticity; the equation to the tangent plane is

$$l x + m y + n z = v \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1.)$$

and we have the two relations

$$l^2 + m^2 + n^2 = 1 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (2.)$$

$$\frac{l^2}{v^2-a^2} + \frac{m^2}{v^2-b^2} + \frac{n^2}{v^2-c^2} = 0 \quad \ldots \quad (3.)$$

To find the equation to the wave-surface we differentiate these equations, making l, m, n, v vary. Eliminating the differentials by the method of indeterminate multipliers, we get the following equations:

$$x = \mathrm{A}\, l + \frac{\mathrm{B}\, l}{v^2-a^2} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (4.)$$

$$y = \mathrm{A}\, m + \frac{\mathrm{B}\, m}{v^2-b^2} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (5.)$$

$$z = \mathrm{A}\, n + \frac{\mathrm{B}\, n}{v^2-c^2} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (6.)$$

$$1 = \mathrm{B}\, v \left\{ \left(\frac{l}{v_2-a^2}\right)^2 + \left(\frac{m}{v^2-b^2}\right)^2 + \left(\frac{n}{v^2-c^2}\right)^2 \right\} \qquad (7.)$$

---

* See p. 78 and 261 of the present volume.

From these *seven* equations the *six* quantities l, m, n, v, A, B are to be eliminated, and the resulting equation will be that to the *wave-surface.*

Such an elimination would in general be quite impracticable; in the present instance, and in many others of the same class, it is facilitated in a remarkable manner by the forms of the equations.

The equations (4.), (5.), and (6.) multiplied by l, m, n respectively and added, give

$$v = A \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (8.)$$

The same equations squared and added, give

$$x^2 + y^2 + z^2 = A^2 + \frac{B}{v}.$$

If we put r^2 for $x^2 + y^2 + z^2$, and for A the value just found, we shall find

$$B = v\,(r^2 - v^2) \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (9.)$$

If these values of A and B be substituted in equation (4.) it may easily be put under the form

$$\frac{a^2\,x}{a^2 - r^2} = \frac{v}{r^2 - v^2}\,(l\,r^2 - x\,v^2).$$

The same substitution made in (5.) and (6.) will give two similar equations; and if these three equations be multiplied by x, y, z respectively, and added, the right-hand side will be found equal to zero; and thus we get finally

$$\frac{a^2\,x^2}{a^2 - r^2} + \frac{b^2\,y^2}{b^2 - r^2} + \frac{c^2\,z^2}{c^2 - r^2} = 0$$

as the equation to the wave-surface.

There is another form of the equation which is rather more easily obtained than the above, which will be found in a paper inserted by Mr. Gregory in the first Number of the Cambridge Mathematical Journal. The form I have given has this advantage, that we may derive from it immediately the construction by means of the ellipsoid; for this I may refer to an article in the second number of the Journal just mentioned.

Jordanhill, near Glasgow,
March 6, 1838.

ARCH. SMITH.

LIV. *Description of two Calculi composed of Cystic Oxide.* *By* THOMAS TAYLOR, *M.R.C.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE unfrequent occurrence of calculi composed of cystic oxide, induces me to believe that a description of two specimens of that substance would not be unacceptable to some of your readers. They were found among a number of unexamined calculi in the museum of St. Bartholomew's Hospital, which, by the kindness of Mr. Stanley, one of the surgeons of the establishment, and the anatomical lecturer, I was permitted to examine and arrange.

The larger specimen weighed 740 grs.; it was of an oval figure, somewhat flattened, measuring one inch nine tenths through the long axis, and respectively one inch four tenths and one inch one tenth through the two short axes. When sawn through it exhibited the confusedly crystallized structure characteristic of this species; the crystals apparently radiated from the centre, their summits projecting at its external surface; these were not however sufficiently defined to render their form evident. The whole had a light yellow colour, its sp. gr. = 1·13. When heated before the blowpipe, it emitted the peculiar odour of cystic oxide, and left a small ash, which was partially fusible. In all its other chemical relations it entirely agreed with that substance.

Ten grains of the sawings yielded by analysis:

Cystic oxide	9·10
Phosphate of lime	0·38
Phosphate of ammonia and magnesia	0·10
Animal matter and loss	0·42
	10·00

Uric acid could not be detected in it. As far as I am aware this specimen is the largest and finest upon record. Of the other the museum possessed only one half; it was of a lighter colour, and the crystalline structure was not so well marked. Crystallization appeared to have taken place from three points. It measured one inch seven tenths by one inch three tenths. Its exterior was coated in parts by a thin layer of the fusible calculus mixed with a little cystic oxide. Its chemical characters corresponded with the other, but when burnt, it left a much smaller residue, which was alkaline. Unfortunately no history has been preserved of either of these calculi, although there are strong grounds for believing that the larger calculus was taken from the bladder of a young man after death, who during life had exhibited no symptoms of any affection of the urinary organs.

Should this meet with your approval, its insertion in your ensuing Number will much oblige,

Gentlemen, yours, &c.,

March 13, 1838. THOMAS TAYLOR,
New Bridge Street, Blackfriars. M.R.C.S.

LV. *On the Composition of certain Mineral Substances of Organic Origin. By* JAMES F. W. JOHNSTON, *M.A., F.R.SS., L. and E., F.G.S., Professor of Chemistry and Mineralogy, Durham**.

II. *Hatchetine.*

THIS mineral is known to occur, though rarely, in connection with the iron ores of the coal measures in Glamorganshire, and in some of the Midland counties of England. The specimen to which the following description and analysis applies was from the former locality, and I have been indebted for it to the liberality and kindness of Sir David Brewster.

It is transparent, yellowish, consists of thin laminæ of a nacreous lustre, has the consistence of soft wax, is greasy to the touch; at ordinary temperatures has no perceptible smell, but when heated emits a fatty odour. Its specific gravity at 60° Fahr. is 0·916, and it melts at about 115° Fahr. I am in possession of too small a quantity to enable me to ascertain its boiling point. By a cautious application of heat it *appears* to distil over without change.

Exposed to the air for a length of time it blackens on the surface, and becomes opake, and it is found in most cabinets in this state. When melted, the black particles, probably charcoal from the slow decomposition of the mineral, float in the fluid and exhibit much lustre.

Boiling alcohol dissolves it very sparingly, and from the solution it is nearly all precipitated on cooling. Æther in the cold also dissolves a very small quantity; in boiling æther it is more largely soluble. On cooling, the solution coagulates into a mass of minute fibres (prisms), from which the æther may be separated by agitation or compression, and which have a crystalline nacreous lustre. In recent specimens the mineral is said sometimes to occur in large crystals, with the form of which I am unacquainted. After repeated boiling with æther there remains still a minute portion undissolved, mixed with the particles of charcoal by which its surface had been blackened.

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* Communicated by the Author.

Concentrated and boiling sulphuric acid chars and decomposes it. In boiling nitric acid it undergoes no apparent change.

According to Sir David Brewster it polarizes light in patches.

Of an uncoloured portion selected for analysis from the centre of the mass, 5·14 grs. gave 15·97 of carbonic acid, and 6·765 of water. These quantities are equal to

		Experiment.	Theory.
1 atom of carbon	= 76·437	= 85·910	85·965
1 atom of hydrogen	= 12·479	= 14·624	14·035
	88·916	100·534	100·

The excess of hydrogen is to be attributed to the unusual quantity of moisture left in the oxide of copper, which the volatility of the substance prevented me from heating sufficiently high to permit the water to be wholly driven out.

This substance therefore belongs to the group of which olefiant gas is the best known type, and it differs from paraffine chiefly in its tendency to crystallize, and to decompose and blacken by long exposure to the air, or by the action of concentrated sulphuric acid. In the last two properties it agrees with the Middletonite described in the preceding Number of this Journal, p. 261.

Durham, March 1838.

LVI. *Researches upon the Products of the Decomposition of Cyanogen in Water*[*]. *By* MM. PELOUZE *and* RICHARDSON.[†]

CHEMISTS have possessed up to the present time but very incomplete notions respecting the alteration which an aqueous solution of cyanogen undergoes when exposed simply to the action of light.

M. Vauquelin, who was occupied with this subject in 1818, found that besides ammonia, and a peculiar black matter, there was formed, by the action of cyanogen upon the elements of the water, three distinct acids, viz. carbonic acid, prussic acid, and a new acid which he considered as composed of cyanogen and oxygen.

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[*] This note is the first part of an examination which we have undertaken upon the alteration which several azotized bodies undergo by the action of water, heat, &c., and upon the state of the azote in charcoals of animal origin.

[†] Communicated by Mr. Richardson.

The opinion of M. Vauquelin upon the nature of this latter substance was solely founded upon theoretical views, for he had neither isolated his new acid, nor studied any of its combinations.

The experiments which we proceed to detail, authorise us to say, that M. Vauquelin had deceived himself in announcing the formation of cyanic acid by the decomposition of cyanogen in water, and that the matter which he had considered as cyanate of ammonia was a mixture of urea and oxalate of ammonia.

A solution of cyanogen in water, prepared in the ordinary manner, was exposed to the action of light, till all odour of cyanogen had disappeared. The new liquid had a strong smell of prussic acid; its colour was slightly yellow, and its reaction neutral. A black, flocky, light substance had fallen to the inferior part of the solution. It was collected upon a filter, and freed from all foreign soluble matters by washing with distilled water. After this purification it was slightly soluble in water and alcohol, insoluble in æther; soluble, on the contrary, in acetic acid and the caustic alkalies, and possessed the property of forming true salts with the various bases.

The small quantity at our disposal did not permit us to submit it to an examination as rigorous and extended as we could have desired. However, from the analysis of its combination with the oxide of silver we have reason to believe that its true composition may be expressed by the following formula:

$$N_8\ C_8\ H_8\ O_4.$$

A part of the liquid was boiled, and the vapour disengaged was conducted through lime-water. An abundant precipitate of carbonate of lime was formed, leaving no doubt that *carbonic acid* was formed during the decomposition of the cyanogen in water. The remainder of the liquid disengaged during its concentration a quantity of *ammonia* and *prussic acid.*

The dry residue had a slight yellow colour, and a saline, sharp taste. Treated with alcohol, it was divided into two nearly equal parts. The portion soluble in the alcohol possessed all the characters of *urea.*

The residue, insoluble in alcohol, was *oxalate of ammonia.* From the analysis of these two substances and the minute examination of their properties, there can be no doubt respecting their production in the spontaneous decomposition of cyanogen dissolved in water. If M. Vauquelin had pursued the examination which he had commenced of the products of this decomposition, he would, perhaps, have been the first to make the beautiful discovery, which fifteen years afterwards was made by M. Wöhler in the artificial production of an animal matter; but the small quantity of matter upon which he operated, did not permit him to analyse completely a subject to which he never afterwards returned.

It is exceedingly curious to see a substance of such a simple composition as cyanogen, a substance which is placed by its characters in the system of chemistry, not at the side, but in the very middle of the elements, giving birth, in reacting upon water, to so many different products.

In admitting for the black matter the formula $N_8\,C_8\,H_8\,O_4$, we can explain the decomposition of cyanogen in water by the following equation:

1 atom urea	N_4	C_2	H_8	O_2
6 atoms prussic acid	N_6	C_6	H_6	
4 atoms carbonic acid.........		C_4		O_8
2 atoms ammonia	N_2		H_6	
1 atom oxalate of ammonia	N_2	C_2	H_8	O_4
1 atom black substance	N_8	C_8	H_8	O_4
	N_{22}	C_{22}	H_{36}	O_{18}

LVII. *Notes to Analytical Development, &c.* By J. J. Sylvester, *of St. John's College Cambridge, Professor of Natural Philosophy in University College, London.*[*]

Note 1.

In the paper above adverted to, I showed that the meridian plane, i. e. the plane containing the ray and normal, always passed through a line of vibration in the corresponding point. Now the line of force called into action by a displacement in the line of vibration clearly lies in this very plane; for the resolved part of it lies in the line of vibration itself.

Harmony and analogy concurred in making me suspect that as two of these four lines are perpendicular to each other, so are also the other two, or in other words, that the ray is always perpendicular to the direction of unresolved force.

The following investigation verifies this conjecture.

Let x, y, z be the coordinates of a point taken at distance unity from the origin and in any line of vibration; then the cosines of the angles made by the line of force with the axes are as $a^2 x : b^2 y : c^2 z$ respectively.

Let a be the inclination between the line of vibration and the line of force, then

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[*] Communicated by the Author: see vol. xi. p. 461 *et seq.* and present volume, p. 73 *et seq.*

$$\cos\omega = \frac{a^2x\,.\,x+b^2y+c^2z\,.\,z}{\sqrt{(a^4x^2+b^4y^2+c^4z^2)(x^2+y^2+z^2)}}$$

$$= \frac{a^2x^2+b^2y^2+c^2z^2}{\sqrt{a^4x^2+b^4y^2+c^4z^2}}\,.$$

Let $\sqrt{a^4x^2+b^4y^2+c^4z^2} = \text{P},$

then $\text{P}^2 = v^4(\sec\omega)^2.$

Now let α, β, γ be the angles of inclination between the coordinate planes and the front in which the line of vibration lies, and λ some quantity to be determined. I have shown in proposition (3.)

that if $\lambda\cos\alpha = (a^2-v^2)\,x$

then will $\lambda\cos\beta = (b^2-v^2)\,y$

and $\lambda\cos\gamma = (c^2-v^2)\,z$

$$\therefore\quad \begin{aligned}\lambda^2 &= a^4x^2+b^4y^2+c^4z^2\\ &\quad -2v^2(a^2x^2+b^2y^2+c^2z^2)\\ &\quad +v^4\\ &= \text{P}^2-v^4.\end{aligned}$$

Again,

$$\lambda^2.\left(\frac{(\cos\alpha)^2}{(a^2-v^2)^2}+\frac{(\cos\beta)^2}{(b^2-v^2)^2}+\frac{(\cos\gamma)^2}{(c^2-v^2)^2}\right) = x^2+y^2+z^2 = 1.$$

$$\therefore\quad \frac{1}{\text{P}^2-v^4} = \frac{(\cos\alpha)^2}{(a^2-v^2)^2}+\frac{(\cos\beta)^2}{(b^2-v^2)^2}+\frac{(\cos\gamma)^2}{(c^2-v^2)^2}\,.$$

Now $\dfrac{1}{\text{P}^2-v^4} = \dfrac{1}{v^4(\sec\omega)^2-v^4} = \dfrac{1}{v^4}\,(\cos\omega)^2.$

And in Mr. Smith's investigation of the form of the wave surface (already alluded to*) by great good fortune I find ready to my hand

$$\frac{(\cos\alpha)^2}{(a^2-v^2)^2}+\frac{(\cos\beta)^2}{(b^2-v^2)^2}+\frac{(\cos\gamma)^2}{(c^2-v^2)^2} = \frac{1}{v^2(r^2-v^2)},$$

(r) being the radius vector to the point whose tangent plane is parallel to the point in question.

Hence $(\cot\omega)^2 = \dfrac{v^4}{v^2(r^2-v^2)} = \dfrac{v^2}{r^2-v^2},$

$$= \frac{p^2}{r^2-p^2},$$

(p) being the length of the perpendicular from the centre upon the tangent plane for $p = v$.

Hence $(\cot\omega)^2$ = the square of the cotangent of the angle between radius vector and normal.

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* See p. 78, 261, and 335.

Or, in other words, the line of force is as much inclined to the line of vibration as the ray is to the normal.

Now the normal is perpendicular to the line of vibration, and all four lines lie in one plane.

∴ the ray is perpendicular to the line of force. Q. E. D.

I may be allowed to conclude this long paper with a summary of some of the most remarkable consequences which I have extricated from Fresnel's hypothesis.

1. The two meridian planes corresponding to any given radius are perpendicular to each other*.
2. So are the two corresponding to any given normal.
3. Every meridian plane bisects the angle formed by two planes drawn through the radius and the two prime radii.
4. It also bisects the angle formed by two planes drawn through the normal and the two prime normals.
5. Each meridian plane contains one line of vibration and the corresponding line of force.
6. The ray is perpendicular to the line of force. All these conclusions, except the fourth, are, I believe, original.

The theory of external and internal conical refraction follows immediately as a particular consequence from the third and fourth combined as already shown; the same propositions also enable us to draw a tangent plane to any point of the wave-surface by mere Euclidean geometry. May not some of these conclusions serve to suggest to physical inquirers the question, Has the theory been started from the most natural point of view ?†

University College, Feb. 24, 1838.

Note 2.—*Investigation of the Wave-Surface.*

Since the appearance of the preceding parts, I have succeeded in completing the self-sufficiency of my method by deducing the equation to the wave-surface from the expressions given in Prop. (5.) for the angles between a front and the principal planes in terms of its two velocities. If these angles be ω, ϕ, ψ, and the two velocities $v_{,}$ $v_{,,}$, we found

$$\cos\omega = \sqrt{\frac{(a^2-v_{,}^2)(a^2-v_{,,}^2)}{(a^2\ b^2)(a^2-c^2)}}$$

$$\cos\phi = \sqrt{\frac{(b^2-v_{,}^2)(b^2-v_{,,}^2)}{(b^2-a^2)(b^2-v^2)}}$$

$$\cos\psi = \sqrt{\frac{(c^2-v_{,}^2)(c^2-v_{,,}^2)}{(b^2-a^2)(c^2-b^2)}}$$

---

* I have defined the meridian plane to be that which contains radius vector and normal belonging to the same point.

† This investigation supplies the step which Mr. Tovey was desirous should appear in the Magazine.

Let the tangent plane to the wave surface be written

$$\frac{\cos\omega.}{v_{,}}\cdot x+\frac{\cos\phi}{v_{,}}\cdot y+\frac{\cos\psi}{v_{,}}\,z=1\quad\ldots\ldots\ldots\quad(\alpha.)^{*}$$

then
$$\frac{d\,\frac{\cos\omega}{v_{,}}\dagger}{d\left(\frac{1}{v_{,}}\right)^{2}}\cdot x+\frac{d\cdot\frac{\cos\phi}{v_{,}}}{d\left(\frac{1}{v_{,}}\right)^{2}}\cdot y+\frac{d\,\frac{\cos\psi}{v_{,}}}{d\left(\frac{1}{v_{,}}\right)^{2}}\,z=0\quad\ldots\ldots\quad(\beta.)$$

$$\frac{d\,.\cos\omega}{d\,(v_{,,})^{2}}\cdot x+\frac{d\,.\cos\phi}{d\,(v_{,,})^{2}}\cdot y+\frac{d\cos\psi}{d\,(v_{,,})^{2}}\cdot z=0\quad\ldots\ldots\ldots\quad(\gamma.)$$

Let $\frac{1}{v_{,}}\cdot\sqrt{\frac{a^{2}-v_{,}^{2}}{a^{2}-v_{,,}^{2}}}=\xi \qquad \sqrt{(a^{2}-b^{2})\,(a^{2}-c^{2})}=\frac{1}{A}$

$\frac{1}{v_{,}}\cdot\sqrt{\frac{b^{2}-v_{,}^{2}}{b^{2}-v_{,,}^{2}}}=\eta \qquad \sqrt{(b^{2}-a^{2})\,(b^{2}-c^{2})}=\frac{1}{B}$

$\frac{1}{v_{,}}\cdot\sqrt{\frac{c^{2}-v_{,}^{2}}{c^{2}-v_{,,}^{2}}}=\zeta. \qquad \sqrt{(c^{2}-a^{2})\,(c^{2}-b^{2})}=\frac{1}{C}$

then = " (γ) becomes $A\,\xi\,x+B\,\eta\,y+C\,\zeta\,z=0\quad\ldots\ldots\ldots\ldots\quad(1.)$

and = "(β) ...... $\frac{A\,a^{2}}{\xi}\cdot x+\frac{B\,b^{2}}{\eta}+\frac{C\,c^{2}}{\zeta}\,z=0\quad\ldots\quad(2.)$

and = "α) may be written under two forms, viz.

$$(a^{2}-v_{,,}^{2})\,A\,\xi\,x+(b^{2}-v_{,,}^{2})\,B\,\eta\,y+(c^{2}-v_{,,}^{2})\,C\,\zeta\,z=1\quad(3.)$$

or $\left(\frac{a^{2}}{v_{,}^{2}}-1\right)\frac{A}{\xi}\,a+\left(\frac{b^{2}}{v_{,}^{2}}-1\right)\frac{B}{\eta}\,y+\left(\frac{c^{2}}{v_{,}^{2}}-1\right)\frac{C}{\zeta}\,z=1\ (4.)$

From (1.) $A\,\xi\,x+B\,\eta\,y=-\,C\,\zeta\,z\quad\ldots\ldots\ldots\ldots\ldots\ldots\quad(5.)$

From (2.) $A\,\frac{a^{2}}{\xi}\,x+\frac{B\,b^{2}}{\eta}\,y=-\,\frac{(c^{2})}{\zeta}\,z\quad\ldots\ldots\ldots\ldots\quad(6.)$

From (2.) and (1.) $A\,(a^{2}-c^{2})\,\xi\,x+B\,(b^{2}-c^{2})\,\eta\,y=1\quad(7.$

From (3.) and (4.) $A\,(a^{2}-c^{2})\,\frac{x}{\xi}+B\,(b^{2}-c^{2})\,\frac{y}{\eta}=c^{2}\quad(8.)$

From (5.) and (6.) $C^{2}.c^{2}.z^{2}-B^{2}\,b^{2}\,y^{2}-A^{2}\,a^{2}\,x^{2}$

$$=A\,B\,x\,y\left(a^{2}\,\frac{\eta}{\xi}+b^{2}.\frac{\xi}{\eta}\right)\quad\ldots\ldots\ldots\ldots\quad(9.)$$

---

[*] In lieu of $v_{,}$ we might write $v_{,,}$ in the denominator without affecting the result.

[†] Observe, that $\frac{\cos\omega}{v_{,}}=\sqrt{\frac{\left(\frac{a^{2}}{v_{,}^{2}}-1\right)(a^{2}-v_{,,}^{2})}{(a^{2}-b^{2})\,(a^{2}-c^{2})}}$, and so on for the rest.

From (7.) and (8.) $C^2 - B^2(b^2 - c^2)^2 y^2 - A^2(a^2 - c^2)^2 x^2$

$$= A\,B\,x\,y\left(\frac{\eta}{\xi} + \frac{\xi}{\eta}\right) \times (a^2-c^2)(b^2-c^2) \quad (10.)$$

From (9.) and (10.) $A\,B\,(a^2-b^2)(a^2-c^2)(b^2-c^2)\,x\,y\,\frac{\eta}{\xi}$

$$\begin{aligned} &= a^2 c^2 - (a^2-c^2)(b^2-c^2)\,C^2 c^2 z^2 \\ &-(a^2.(b^2-c^2)^2 - b^2(a^2-c^2)(b^2-c^2))\,B^2 y^2 \\ &-(a^2.(a^2-c^2)^2 - a^2(a^2-c^2)(b^2-c^2))\,A^2 x^2 \\ &= a^2 c^2 - c^2 z^2 - c^2 y^2 - a^2 x^2 \;\ldots\ldots\ldots\ldots \quad (11.) \end{aligned}$$

From (11.), interchanging (a, x, ξ) with (b, y, z) we have

$$A\,B\,(b^2-a^2)(b^2-c^2)(a^2-c^2)\,x\,y\,\frac{\xi}{\eta} = b^2 c^2 - c^2 z^2 - c^2 x^2 - b^2 y^2$$

$$\ldots\ldots\ldots\ldots \quad (12.)$$

Finally, from (11.) and (12.) we have

$$\begin{aligned} &(a^2 c^2 - \overline{a^2-c^2}\,.\,x^2 - c^2\,.\,\overline{x^2+y^2+z^2}) \\ &\times (b^2 c^2 - \overline{b^2-c^2}\,.\,y^2 - c^2\,\overline{x^2+y^2+z^2}) \\ &= (a^2-c^2)(b^2-c^2)\,x^2 y^2 \\ \text{i. e. } &(x^2+y^2+z^2)(a^2 x^2 + b^2 y^2 + c^2 z^2) \\ &-a^2(b^2+c^2)+x^2 - b^2(a^2+c^2)\,y^2 - c^2(b^2+a^2)\,z^2 \\ &+a^2 b^2 c^2 = 0. \end{aligned}$$

The $=^{n}$ required.

University College, March 5, 1838.

LVIII. *On the Occurrence of the form* $\frac{0}{0}$ *in passing from general to particular Values of certain Algebraic Functions.*

By G. B. JERRARD, *Esq.*[*]

SUPPOSE that we have two equations for determining x,

$$x^m + A\,x^{m-1} + B\,x^{m-2} \ldots\ldots + V = 0 \qquad (1.)$$

$$y = P + Q\,x + R\,x^2 \ldots\ldots + L\,x^\lambda \qquad (2.)$$

so that $$x = \frac{\phi\,(y, P, Q, R, \ldots L)}{\psi\,(y, P, Q, R, \ldots L)};$$

A, B, C, ... V being constant quantities involved in the meaning of the functions ϕ and ψ.

Let us further suppose that P, Q, R, ... L are in such a manner dependent on certain equations of condition that we are able to foresee that the expression for x will be determinate when m is general in value. If then it should appear that for a certain value of m the equation

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[*] Communicated by the Author.

$$y = P' + P''x + P'''x^2 \ldots + P^{(m)}x^{m-1},$$

which arises from combining the equations (1.) (2.), would take the form

$$0 = 0 + 0x + 0x^2 \ldots + 0x^{m-1},$$

in what light ought we to regard this result? The expression for x will most certainly in this case assume the form

$$x = \frac{0}{0}:$$

but ought this to be regarded as the ultimate form for x? May not the expression $\frac{0}{0}$ become interpretable by following the known methods for the treatment of such functions; or rather, must it not be susceptible of an ulterior form?

To fix our ideas, let

$$m = 4, \qquad \lambda = 4,$$

also let

$$P = f_1(S), \;\; R = f_2(S), \;\; Q = f_3(S), \;\; y = f_4(S), \;\; T = 1.$$

Accordingly we shall have

$$x = \frac{\Phi(S)}{\Psi(S)}.$$

Then if

$$y = 0, \quad P' = 0, \quad P'' = 0, \quad P''' = 0, \quad P^{\text{iv}} = 0,$$

or,

$$f_4(S) = 0, f_1(S) - D = 0, f_3(S) - C = 0, f_2(S) = 0, \;\; S = 0;$$

that is to say, if the functions designated by f_1, f_2, f_3, f_4 be such that

$$f_4(0) = 0, \;\; f_1(0) - D = 0, \;\; f_3(0) - C = 0, \;\; f_2(0) = 0:$$

ought we to conclude generally that

$$\frac{\Phi(S)}{\Psi(S)}$$

will not admit of a definite interpretation when

$$S = 0, \quad \Phi(0) = 0, \quad \Psi(0) = 0?$$

If all the quantities y, P', P'', ... $P^{(m)}$ could be shown to be determinate in value, and any one of them except y and P' not equal to zero, it would not be necessary to proceed to the ultimate form of x, in order to show the possibility of expressing x by a determinate function of y, P, Q, R, ... L involving the solution of an equation of less than m dimensions.

For a similar reason, in discussing the possibility of effecting those very general transformations given in my "Mathematical Researches," in which λ may have a fixed value while m is increased without limit, the question does not arise respecting the illusory form of the series for y. But below a certain limit the method of proof there followed evidently leaves us without any information respecting the occurrence of the form $\frac{0}{0}$; and it becomes interesting to inquire how far the general solution may extend itself even through the form $\frac{0}{0}$ to a particular value of m.

West Park, Bristol, March 13, 1838.

LIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 280.]

Report of the Proceedings of the Council for the past year.

THE principal business of public interest which has occupied the attention of the Council relates to the extension of accurate magnetical and meteorological observations in different parts of the world.

A communication having been made by Lieut. William Denison, of the Royal Engineers, of a proposal from General Mulcaster, Inspector-General of Fortifications, that the officers of engineers generally should be employed, under the direction of the Royal Society, in promoting the advancement of science, by carrying on connected series of observations relating to Natural History, Meteorology, Magnetism, and other branches of physical science, and suggesting an application to Government for a grant of funds necessary for effecting so desirable an object; a Committee was appointed to consider of the proposed measure, and of the means of carrying into effect the recommendations contained in the letter of Baron Von Humboldt, addressed in April last to His Royal Highness the President*. Conformably with the report made by this Committee, the Council fixed on the ten following places, namely, Gibraltar, Corfu, Ceylon, Hobart Town, Jamaica, Barbadoes, Newfoundland, Toronto, Bagdad, and the Cape of Good Hope, as being the most eligible for carrying on magnetic observations according to the plan recommended by Baron Von Humboldt; those places being permanent stations, where officers of engineers and clerks are always to be found. The Council also determined that, for the present, the observations of magnetism may be limited to those of the direction of the magnetic needle, and the meteorological observations restricted to those made on the four days, and in the manner recommended in Sir John Herschel's instructions.

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* See p. 271.

A grant of 500*l.* from the public funds has since been obtained from the Lords Commissioners of Her Majesty's Treasury, in aid of the purchase of the necessary instruments for carrying on the magnetic observations, according to the plan proposed by the Committee, and under the directions of the Royal Society.

A statement having been also laid before the Council by Mr. Christie of the importance of a more accurate determination than has hitherto been made of the variation of the magnetic needle at several points on the coasts and in the interior of Great Britain and Ireland, and likewise of the dip and of the intensity of terrestrial magnetism, the Council, fully concurring in these views, presented to the Lords of the Admiralty a strong recommendation that steps should be taken for carrying into effect the course of observations pointed out by Mr. Christie; and their Lordships have in consequence appointed a Committee to meet and examine into this important subject.

The Council having deemed it desirable that the difference of level between the brass mark fixed by Capt. Lloyd on the north-east landing stairs of the New London Bridge, and Mr. Bevan's mark on the basement of the pilasters of the north-east landing stairs of Waterloo Bridge, should be accurately determined, requested Sir John Rennie to undertake this determination. Sir John Rennie has reported to the Council that, after repeated trials, the greatest variation of which did not exceed two-tenths of an inch, he found that the mark on Waterloo Bridge is 3 feet and 1·65 inches above that on New London Bridge*.

The Council have awarded the Copley Medal of this year to M. Becquerel for his various Memoirs on the subject of Electricity, published in the "Mémoires de l'Académie Royale des Sciences de l'Institut de France", and particularly for those on the production of Crystals of Metallic Sulphurets and of Sulphur, by the long-continued action of electricity of very low tension, and published in the tenth volume of those Memoirs †.

Among those who have been engaged in investigating the phænomena of electricity, M. Becquerel holds an eminent rank, and the Memoirs of the Royal Academy of Sciences of Paris bear ample testimony to the success which has attended his researches in this department of science. He appears early to have been sensible that, for the detection of phænomena which may occur at the instant of incipient molecular attraction, and which become masked by the more general effect of the transfer of the elements when powerful electric currents are employed, it was necessary to substitute for these currents of very low tension ‡. Following out the view, carefully adjusting the strength of the current to the power of the affinities brought into action, he succeeded, by electric decomposition, and by subsequent recomposition of the elements, in obtaining crystals of some of the metallic sulphurets, of sulphur, of the iodurets of lead and copper, of the insoluble sulphates of lime and barytes, of the carbonate of lead, and other substances, a few of which had previously, by other means, been obtained crystallized, but of which the great majority had only been recomposed in an amorphous state. In the Memoirs to which the Council have particularly adverted in the award of the Copley Medal to M. Becquerel, he had especially in view to explain, by the agency of electricity of very low tension, continued for an indefinite time, the occurrence of crystallized substances in mineral veins. The success with which his experiments were crowned in obtaining by such means crystals of the metallic sulphurets and of other substances, perfectly resembling those found abundantly in mineral veins, is favourable to the correctness of the views he had entertained; and these views derive additional support from the results obtained by others, in perfect accordance with his own, by means differing from those he employed, but involving precisely the same principles. Mr. Fox, in his experiments, which appear to have been conducted on a larger scale than those of M. Becquerel, endeavoured more closely to imitate the arrangements of nature, by introducing, between the substances acted on, walls of clay, in imitation of the "flucan courses" in the Cornish mines; these walls performing the same functions as the moistened clay in M. Becquerel's experiments; and he infers from his results, that the phænomena presented by the mineral veins of Cornwall are explicable on principles which are similar to those pointed out by M. Becquerel*. It is thus rendered highly probable that the long-continued action of electricity of low tension has been at least one of the means by which crystallized bodies now existing in mineral veins have been produced.

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* See p. 205.

† See Scientific Memoirs, vol. i. p. 414.

‡ *Annales de Chimie*, tom. xxxiv. p. 152. *Mémoire lu à l'Académie Royale des Sciences*, &c., 21 Aout, 1826.

But quite independently of the bearing of M. Becquerel's results on a question of great geological interest, the formation of crystals of metallic sulphurets and other substances by the agency of electricity was a great step in chemical science. As M. Becquerel very justly observes, the two branches of chemistry, analysis and synthesis, are at present in very different states. With the exception of crystals derived from aqueous solution,—which are by far the least abundant of natural crystals,—and a few from fusion, the great mass of crystallized bodies existing in nature had as yet remained inimitable by chemical processes. In the Memoirs referred to, not only are experiments described by which crystals of several of these substances have been obtained, but the principles are pointed out, by the application of which we may anticipate that large classes of others will be produced. M. Becquerel has thus opened a new field for inquiry and discovery, in which he has himself gathered the first fruits, but which still offers to future labourers the prospect of an abundant harvest of knowledge as regards both the recomposition of crystallized bodies, and also the processes which may have been employed by nature in the production of such bodies in the mineral kingdom.

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* See Lond. and Edinb. Phil. Mag., vol. xi. p. 203.

A Copley Medal has been awarded to John Frederick Daniell, Esq., for his two papers on Voltaic Combinations, published in the Philosophical Transactions for 1836*.

The Council are desirous of testifying, by this award, their sense of the great value of Mr. Daniell's invention of a new form of the voltaic battery, capable of producing, for a considerable length of time, a perfectly equal and steady current of electricity. The principles on which his apparatus, which he terms *the constant battery*, is constructed, were the results of a series of well-devised experiments, directed to the discovery of the cause of those great and often rapid variations in the power of the ordinary battery, which have hitherto limited its utility when employed for purposes of philosophical research, and the removal of which has greatly extended the range and multiplied the applications of this powerful instrument of chemical analysis.

The train of reasoning that led Mr. Daniell to this discovery, originated in an inquiry which he undertook with the view of determining with precision the influence exerted by the different parts of the voltaic battery in their various forms of combination. For this purpose he contrived an apparatus which he designates by the name of *the dissected battery*, and which consists of a series of cylindrical glass vessels capable of holding the fluid electrolyte, with a pair of metallic plates immersed in it, each plate communicating below by means of a separate wire, with a small quantity of mercury, as the medium of the various communications which may at pleasure be made with other metallic parts of the apparatus. This arrangement affords peculiar advantages for studying the difference of effect in reference to the quantity and the intensity of the electric current, consequent on the different modes of connecting the elements of the battery, and also the influence of retarding forces resulting from other modes of connexion. In the course of these researches, Mr. Daniell, observing the great extent of negative metallic surface over which the deoxidating influence of the positive metal appeared to manifest itself, was induced to institute a more careful examination of the circumstances attending this class of phænomena, and was led to the discovery of the gradual deposition of zinc on the platina plates being the principal cause of the progressive decline of the power of the battery. It was then that the means of counteracting this tendency presented itself to his mind. His plan consists in the constant application of a solution of sulphate of copper to the copper surface, while, at the same time, diluted sulphuric acid is constantly applied to the zinc surface, on which it exerts an oxidating and a solvent power, and is constantly renovated as it becomes charged with zinc. The two fluids are separated from one another by a partition formed of membrane, or other porous substance, which prevents intermixture, but offers no obstacle to the transmission of galvanic action. Two principal objects are accomplished by this arrangement of the constituent parts of the battery; first, the removal out of the circuit of the oxide of zinc, the deposit of which gradually reduces, and at length suspends, the action of the ordinary battery; and secondly, the absorption of the hydrogen evolved upon the surface of the copper, without the precipitation of any substance tending to counteract the voltaic action of that surface.

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* Abstracts of Mr. Daniell's papers were given in Lond. & Edinb. Phil Mag., vol. viii. p. 421, and vol. ix. p. 376.

The advantages likely to arise to science from the invention of the constant voltaic battery are numerous and important. Mr. Daniell has shown how it may be made to supply a measure of chemical affinity, and has applied it with effect in the investigation of the influence of changes of temperature on voltaic action. The construction of a constant battery of large dimensions, which he has recently completed, has already opened new views of the possible application to economical purposes of the powers of voltaic electricity, an agent, of which the influence appears to be so energetic and so widely diffused throughout nature.

The Council have adjudged one of the Royal Medals, in conformity with the announcement made in 1834, to Mr. Whewell, for his series of Researches on the subject of the Tides, which have been published in our Transactions during the last three years.

Mr. Whewell's researches have been chiefly directed to the three following points: first, the motion of the tide-wave at different points of the ocean; secondly, the comparison of the *observed* laws at certain places with the *theory*; and lastly, the laws of the diurnal inequality of the tide.

It is to Mr. Lubbock that we are indebted for the first accurate comparison of the theory of the tides as given by Bernoulli in his treatise *Du flux et reflux de la mer*, with the results of observation as deduced from a period of nineteen years in the port of London. In this memoir, which was published in our Transactions for 1831, there was given a most elaborate discussion by Mr. Dessiou, under Mr. Lubbock's directions, of more than 13,000 observations, and the results were of great importance, not merely as furnishing the materials and the general rules for the construction of tide tables, but also for the general accordance which they exhibited with the equilibrium theory of Bernoulli, particularly with respect to the *semimenstrual inequality*. This agreement was the more important, as affording the indication of the real existence of a physical connection between the theory and observation, and as consequently justifying such a further examination of its consequences as might lead to the discovery or suggestion of such modifications of it as would lead to its general accordance with the laws of all the facts observed.

In a subsequent discussion of the tides of Liverpool, published in our Transactions in 1835 and 1836, Mr. Lubbock showed, as had partly indeed been suggested by Mr. Whewell in his papers on the empirical laws of the tides of London and Liverpool, that by referring the tide, not to the lunar transit immediately preceding, but to an anterior lunar transit, one, two, or more days before, the formulæ furnished by the equilibrium theory would be brought into almost perfect accordance with the observed inequalities in the heights and times of the tides which are due to the changes in the moon's parallax. This was a most important step in the connexion between theory and observation, and has been found to apply, to a considerable extent, to all the periodical inequalities of the tides, though very different epochs are required for different inequalities. Thus Mr. Whewell has shown that the diurnal inequality in the heights of high and low water, which is due to the change in the moon's declination, would require to be referred to the lunar transit four days preceding.

But though the formulæ furnished by theory can be thus adjusted to represent generally the results of observation for any assigned station, yet our theory is quite incompetent to assign the physico-mathematical grounds upon which such adjustments are made: the complete solution of such a problem would probably require a knowledge of the laws of hydrodynamics much beyond that which we now possess.

The first memoir which was published by Mr. Whewell was an "Essay towards a first approximation to a map of cotidal lines," and appeared in our Transactions for 1833.

By *cotidal* lines, Mr. Whewell means those lines which may be drawn through all those points of the ocean which have high-water at the same moment of absolute time.

By analysing the movements of the tides as determined by the most simple considerations of the laws of fluid motion in open seas and in channels, and by explaining the circumstances of their convergence or divergence, their interference with each other, their retardation in shallow water, and their consequent tendency to sweep round the coasts and to approach them almost perpendicularly; and further, by discussing very carefully all the materials which nautical surveys and books of navigation could furnish him, Mr. Whewell was enabled to construct a map, which not only represented the general circumstances of the tides of the coasts of Great Britain, but likewise the movement of the great tidal wave, on the coasts of Europe, in the Atlantic Ocean, in the Indian seas, and on the coasts of New Zealand.

It was with a view to correct this first approximation to a map of cotidal lines that Mr. Whewell procured a very extensive series of observations to be made on the coasts of Great Britain and Ireland at 547 stations of the Coast Guard for an entire fortnight in June, 1834. These observations were repeated in June, 1835, and were accompanied by simultaneous observations made by the great maritime powers of Europe and North America, at the request of the Government of this country, at various stations on their coasts. The immense mass of observations, thus furnished, were reduced, under Mr. Whewell's directions, at the expense of the Admiralty, and some of the results, which are extremely important and interesting, have been communicated by him to the Royal Society in two Memoirs in our Transactions for 1835 and 1836. The last of these Memoirs was accompanied by a second map of the cotidal lines of the coasts of Europe, accompanied also by indications, effected by a peculiar notation, of the total range, in yards, of the tides at the different stations at which observations had been made.

Many very remarkable conclusions with respect to the motion of the tide-wave have resulted from these observations; amongst others may be mentioned the rotatory motion of the tide-wave which enters the German Ocean between the Orkneys and Norway, sends a southerly detachment along the coasts of Great Britain, which is reflected from the projecting coast of Norfolk upon the north coast of Germany, and meets the main wave again on the coast of Denmark.

It is impossible in the course of a very brief abstract like the present to notice all Mr. Whewell's researches in detail. His second great object was to compare the observed laws of the tides with the theory, or to propose such modifications of the forms of the theory as would reconcile it with the observations.

The interest which attaches to such investigations, which is so great during the progress of the structure which is to be raised upon them, ceases in many cases when the fabric is completed: a remark which is applicable to many of the most important researches and discoveries in philosophy, where we are accustomed to regard the last form only in which the theory is compared with the facts which are observed, and to forget or to neglect the series of laborious investigations which have led to its establishment, but which are no longer necessary for its explanation or proof. This observation may be applied, in some degree, to Mr. Whewell's very ingenious Memoir "On the Empirical Laws of the Port of London", in which he attempts to deduce from observation and from very simple general considerations, the character of the formulæ for determining the establishment, the semimenstrual inequality, the corrections for lunar and solar parallax and declination, both as affecting the times and the height of high water. Similar observations may be extended to his papers on the "Empirical Laws of the Tides of the Port of Liverpool," and also on the "solar inequality and diurnal inequality" of the tides at the same place, which are full of valuable suggestions which the subsequent investigations of Mr. Lubbock have, in some cases, very remarkably confirmed and extended.

The last of the series of researches of Mr. Whewell relate to the diurnal inequality of the height of the tide, which the discussion of the tides at Liverpool had exhibited, though under circumstances much less striking than those which characterize its appearance in other places. The first of his memoirs on this subject relates to the diurnal inequality at Plymouth and Sincapore, at the last of which places its magnitude is very remarkable, making a difference of not less than six feet in the height of morning and evening tide, and quite sufficient to obliterate, under certain circumstances, one of the semidiurnal tides, and explaining certain phænomena in the tides which have been considered as cases of interference. Mr. Whewell was led, from certain remarkable changes in the epoch of this phænomenon, which seemed to be deducible from the observations at Bristol, Liverpool and Leith, to suspect that its progress along the coasts of Europe and Great Britain was retarded according to some regular law. His subsequent discussion, however, of the simultaneous observations made in June, 1835, with an especial view to this inequality, showed that the differences of diurnal inequality were governed by local causes, and consequently negatived altogether the hypothesis of its progressive propagation according to a law distinct from that of the other inequalities of the tides.

The preceding abstract of Mr. Whewell's Researches on the Tides is necessarily very brief and imperfect, and little calculated to convey to the minds of those who have not read his very extensive series of memoirs an adequate notion of the amount of labour and of thought which the discussion of such extensive series of observations must have required.

The importance of the results which have been obtained by him and Mr. Lubbock, may be best estimated by the rapid advancement which has been made in our knowledge of the laws which regulate the movements of the tides during the last six years, and which is entirely owing to their joint labours. Theory, though little cultivated and little known, was then in advance of observation: tide tables were constructed by unpublished rules, which formed a profitable possession to those to whom the secret was known: and the distinctive characters of the tides in the different ports of this kingdom, that of Liverpool perhaps excepted, were confined to the experience and tact of those who were accustomed to use them; but how different is the case at present! The rules for the construction of tide tables are not only public property, but are based upon the most extensive observations: laws, whose existence was hardly suspected, are now distinctly laid down: the progress of the waves in the most frequented parts of the ocean is beginning to be accurately developed: theory, which was formerly in advance of observation, though greatly improved in those parts of it which do not involve the hydrodynamical laws of the ocean, is now greatly behind it; and such a basis of facts has been laid down as may enable the mathematician to commence such a series of investigations, as may terminate in enabling another Laplace to give to the theory of the tides a form which may rival, in the certainty of its predictions, the almost perfect theories of physical astronomy*.

The following were then elected as Officers and of Council:

President.—His Royal Highness the Duke of Sussex, K.G. *Treasurer.*—Francis Baily, Esq. *Secretaries.*—Peter Mark Roget, M.D.; Samuel Hunter Christie, Esq., M.A. *Foreign Secretary.*—William Henry Smyth, Capt. R.N. *Other Members of the Council.*—John Bostock, M.D.; The Earl of Burlington; John George Children, Esq.; John Frederick Daniell, Esq.; Sir Philip Grey Egerton, Bart.; Davies Gilbert, Esq., D.C.L.; Charles Konig, Esq.; The Marquis of Northampton; Rev. George Peacock, M.A.; William Hasledine Pepys, Esq.; Stephen Peter Rigaud, Esq., M.A.; John

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* Abstracts of Mr. Whewell's papers on the tides will be found in Lond. and Edinb. Phil. Mag., vol. iii. p. 216; vol. iv. p. 223; vol. vii. p. 136; vol. viii. p. 147, 430, 547; vol. ix p. 528; vol. x. p. 317, 380; vol. xi. p. 195.

Forbes Royle, M.D.; Benjamin Travers, Esq.; James Walker, Esq.; Charles Wheatstone, Esq.; Rev. William Whewell, M.A.

December 7, 1837.—No paper was read.

Dec. 14, 1837.—The reading of a paper, entitled "On low Fogs and stationary Clouds." By William Kelly, M.D. Communicated by Captain Beaufort, R.N., F.R.S., &c., was resumed and concluded.*

The object of the present paper is to point out the circumstances which influence the formation of low fogs, and to show what analogy exists between the causes that produce them and those that occasion certain forms of clouds, which may be considered as differing from fogs only in position. Having been attached for several years to the naval party employed in the survey of the gulf and river of St. Lawrence, the author had ample opportunities of observing the phenomena in question. He concludes that the fogs described occur chiefly when the air is nearly saturated with moisture, and when at the same time the temperature of the water on which they rest either exceeds that of the air, or is considerably below it. These fogs are generally very dense, often limiting the sphere of vision to a few fathoms; but seldom extend to any considerable height. They do not often cover the land to any distance from the shore; and the tops of the hills, close to the water's edge, are clear, while the bases, or sides, are enveloped in the mist.

The following papers were then read:—

"On the Colours of Mixed Plates." By Sir David Brewster, K.G.H., F.R.S., &c.

In the prosecution of his optical inquiries, the author was induced to study the phenomena of mixed plates, (originally discovered by Dr. Young, and described by him in the Philosophical Transactions for 1802,) as he had observed similar appearances in various mineral bodies under analogous circumstances, to which he had been led to ascribe an origin different from that assigned by Dr. Young. In order to obtain a more distinct view of these colours, Sir David Brewster employed, instead of the substances used by Dr. Young, the white of an egg, beat up into froth, and pressed into a thin film between plates of glass. From observations of the colours exhibited by plates so prepared, and also by the edge of a thin film of nacrite in contact with copaivi balsam, the author deduces the conclusion, that all these phenomena, as well as those often seen in certain specimens of mica through which titanium is disseminated, and also in sulphate of lime, are cases of diffraction, where the light is obstructed by the edges of very thin transparent plates placed in a medium of different refractive power. If the plate were opake, the fringes produced would be of the same kind as those often noticed, and which are explained on the principle of interference; but, owing to the transparency of the plate, fringes are produced within its shadow; and, owing to the thinness of the plate, the light transmitted through it is retarded, and, interfering with the partial waves which pass through the plate, and with those which pass beyond the diffracting edge with undiminished velocity, modify the usual system of fringes in the manner described by the author in the present paper.

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* We here resume our regular report of the Society's Proceedings, interrupted in p. 206.

"Of such Ellipsoids, consisting of homogeneous Matter, as are capable of having the Resultant of the Attraction of the Mass upon a Particle in the Surface, and a Centrifugal Force caused by revolving about one of the Axes, made perpendicular to the Surface." By James Ivory, K.H., M.A., F.R.S. L. and Ed., Inst. Reg. Sc., Paris, Corresp. et Reg. Sc. Gotting. Corresp.

Lagrange, who has considered the problem of the attractions of homogeneous ellipsoids in all its generality, and has given the true equations from which its solution must be derived, inferred from them that a homogeneous planet cannot be in equilibrium unless it has a figure of revolution. But M. Jacobi has proved that an equilibrium is possible in some ellipsoids of which the three axes are unequal and have a certain relation to one another. His transcendental equations, however, although adapted to numerical computation on particular suppositions, still leave the most interesting points of the problem unexplored.

The author of the present paper points out the following property as being characteristic of all spheroids with which an equilibrium is possible on the supposition of a centrifugal force. From any point in the surface of the ellipsoid draw a perpendicular to the least axis, and likewise a line at right angles to the surface: if the plane passing through these two lines contain the resultant of the attractions of all the particles of the spheroid upon the point in the surface, the equilibrium will be possible, otherwise it will not. For the resultant of the centrifugal force and the attraction of the mass must be a force perpendicular to the surface of the ellipsoid, which requires that the directions of the three forces shall be contained in one plane. This determination obviously comprehends all spheroids of revolution; but, on account of the complicated nature of the attractive force, it is difficult to deduce from it whether an equilibrium be possible or not in spheroids of three unequal axes, a problem which is unconnected with the physical conditions of equilibrium, and which is a purely geometrical question respecting a property of certain ellipsoids.

The author then enters into an analytical investigation, from which he deduces the fundamental equation.

$$B - \frac{A}{1 + \lambda^2} = C - \frac{A}{1 + \lambda'^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

the three axes of the ellipsoid being

$$k, \quad k\sqrt{(1 + \lambda)}, \quad k\sqrt{(1 + \lambda'^2)},$$

and A, B, C, constants, afterwards expressed by certain definite integrals. He then remarks that every ellipsoid which verifies this formula is capable of an equilibrium when it is made to revolve with a proper angular velocity about the least axis; for, in this case the centrifugal force will be represented in quantity and direction by a line such that the resultant of this force and the whole attraction of the ellipsoid upon a point in the surface will be perpendicular to the surface. Lagrange had concluded that the equation (1), which results immediately from his investigations, admits of solution only in spheroids of revolution, that is when $\lambda = \lambda'$ and $B = C$; but by expressing the functions A, B, C in elliptic integrals, M. Jacobi has found that the equation may be solved when the three axes have a particular relation to one another. In order to ascertain the precise limits within which this extension of the problem is possible, and to determine the ellipsoid when the centrifugal force is given, the author has recourse to the equations of Lagrange, which contain all the necessary conditions, and he deduces the equations

$$f = B - \frac{A}{1+\lambda^2}, \quad f = C - \frac{A}{1+\lambda'^2}, \quad . \quad . \quad . \quad (2.)$$

where f represents the intensity of the centrifugal force at the distance equal to unity from the axis of rotation; and he remarks that these equations coincide with the equations of Lagrange. Substituting for A, B, C certain definite integrals given in the *Mécanique Céleste*, he deduces three equations expressing the value of g, the ratio of the intensity of the centrifugal to that of the attractive force, one of these being expressed in terms of the density, and the other two in the form of definite integrals; and then remarks that "these equations comprehend all ellipsoids that are susceptible of equilibrium on the supposition of a centrifugal force."

He then applies these equations to the more simple case of the spheroid of revolution, where $\lambda = \lambda' = l$, and determines the value of l

$$l = 2{\cdot}5293,$$

and the corresponding maximum value of $g = 0{\cdot}3370$, and remarks that, with respect to spheroids of revolution, it thus appears that an equilibrium is impossible when g, or its value in terms of the density, is greater than 0·3370. In the extreme case, when g is equal to 0·3370, there is only one form of equilibrium, the axes of the spheroid being

$$k \text{ and } k\ \sqrt{\{1 + (2{\cdot}5293)^2\}} \text{ or } 2{\cdot}7197\ k;$$

but when g is less than 0·3370 there are two different forms of equilibrium, the equatorial radius of the one being less, and of the other greater than 2·7197 k, k being the semi-axis of rotation.

The number of the forms of equilibrium in spheroids of revolution, he remarks, is purely a mathematical deduction from the expression of the ratio of the centrifugal to the attractive forces; and as this has been known since the time of Maclaurin, the discussion of it was all that was wanted for perfecting this part of the theory.

Returning to the general equations of the problem, the author deduces the equations

$$g = \frac{d\varphi}{dp} p$$

$$0 = \frac{d\varphi}{r\,dr},$$

where φ is a definite integral, such that

$$g = \frac{d\,\varphi}{d\,\lambda} \,.\, \lambda, \quad g = \frac{d\,\varphi'}{d\,\lambda'}.$$

$$p = \lambda\,\lambda' \text{ and } r^2 = (\lambda - \lambda')^2,$$

which equations apply exclusively to ellipsoids with three unequal axes, and solve the problem with regard to that class. From these he derives another equation, which he states is no other than a transformation of his first fundamental equation, and is equivalent to other transformations of the same equation found by M. Jacobi and M. Liouville.

He also remarks that a limitation of one of the constants, which the verification of this formula requires, agrees with the limitation of M. Jacobi; and further, that the relations which may subsist between the constants proves that there does exist an infinite number of ellipsoids not of revolution, which are susceptible of an equilibrium.

After determining the corresponding limits of these relations of the constants, p being contained between the limits 1·9414 and 1, while r^2 increases from zero to infinity, he remarks that an elliptical spheroid formed of a homogeneous fluid can be in equilibrium by the action of a centrifugal force only when it revolves about the least axis.

He next deduces the general value of g (the ratio of the forces,) and thence its value in one extreme case, when $r^2 = 0$, or when λ and λ' are equal, and remarks that this is no other than the determination of g in a spheroid of revolution having its axes equal to

$$k \text{ and } k\sqrt{2{\cdot}9414} = k \times 1{\cdot}7150.$$

In the other extreme case, when r^2 is infinitely great, g is zero.

From this investigation the conclusion is arrived at, that for every given value of r^2 there is only one value of p, and only one ellipsoid; and that to every such ellipsoid there is an appropriate value of g: and, further, that for every possible value of g there will be only one value of r^2, and consequently only one ellipsoid susceptible of an equilibrium.

Also the reading of a paper, entitled, "Experimental Researches in Electricity." Eleventh Series. By M. Faraday, Esq., D.C.L., F.R.S., Fullerian Professor of Chemistry at the Royal Institution, was commenced.

December 21, 1837.—The reading of Mr. Faraday's eleventh series of Experimental Researches in Electricity was resumed, but not concluded.

The Society then adjourned over the Christmas vacation to meet again on the 11th of January 1838.

January 11, 1838.—The reading of a paper, entitled "Experimental Researches in Electricity," Eleventh Series, by Michael Faraday, Esq., D.C.L., F.R.S., Fullerian Professor of Chemistry at the Royal Institution, &c., was resumed and concluded*.

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* Having inadvertently inserted in our number for February a notice of this paper in an incomplete form, we now give the complete and corrected official abstract of it.

The object of this paper is to establish two general principles relating to the theory of electricity, which appear to be of great importance; first, that induction is in all cases the result of the actions of contiguous particles; and secondly, that different insulators have different inductive capacities.

The class of phænomena usually arranged under the head of *induction* are reducible to a general fact, the existence of which we may recognise in all electrical phænomena whatsoever; and they involve the operation of a principle having all the characters of a first, essential and fundamental law. The discovery which he had already made of the law by which electrolytes refuse to yield their elements to a current when in the solid state, though they give them forth freely when liquid, suggested to the author the extension of analogous explanations with regard to inductive action, and the possible reduction of many dissimilar phænomena to one single comprehensive law. As the whole effect upon the electrolyte appeared to be an action of the particles when thrown into a peculiar polarized state, he was led to suspect that common induction itself is in all cases an *action of contiguous particles*, and that electrical action at a distance, which is what is meant by the term *induction*, never occurs except through the intermediate agency of intervening matter. He considered that a test of the correctness of his views might be obtained by tracing the course of inductive action; for if it were found to be exerted in curved lines it would naturally indicate the action of contiguous particles, and would scarcely be compatible with action at a distance. Moreover, if induction be an action of contiguous particles, and likewise the first step in electrolyzation, there seemed reason to expect some particular relation of this action to the different kinds of matter through which it is exerted; that is, something equivalent to a specific electric induction for different bodies; and the existence of such specific powers would be an irrefragable proof of the dependence of induction on the intervening particles. The failure of all attempts to produce an absolute charge of electricity of one species alone, independent of the other, first suggested to the author the notion that induction is the result of actions among the individual and contiguous particles of matter, having both forces developed to an extent exactly equal in each particle.

The author describes various experiments, with the view of showing that no case ever occurs in which an absolute charge of one species of electricity can be given. His first experiments were conducted on a very large scale: an insulated cube, twelve feet in the side, consisting of a wooden frame, with wire net-work, every part of which was brought into good metallic contact by bands of tin foil, had a glass tube, containing a wire in connexion with a large electrical machine, passed through its side, so that about four feet of the tube entered within the cube and two feet remained without; but it was found impossible in any way to charge the air within this apparatus with the least portion of either electricity.

For investigating the question whether induction is an action of contiguous particles, the author employed, as an electrometer, the torsion balance of Coulomb with certain alterations and additions; and for deciding that of specific inductive capacity, a new apparatus, constructed for that express purpose. This apparatus consisted of two hollow brass spheres, of very unequal diameters, the smaller placed within the larger, and concentric with it; the interval between the two being the space through which the induction was to be effected. The apparatus had a tube in the lower part, furnished with a stop-cock, by means of which it might be connected with an air-pump or filled with any required gas. In place of the lower hemispherical shell of air, occupying the interval between the two spheres, any solid dielectric, of the same form, such as shell-lac, glass, or sulphur, might be substituted. Two of these instruments, precisely similar in every respect, were constructed, and the author ascertained that the inductive power was the same in both, by alternately charging each and dividing the charge with the other, and finding that, in all cases, the charge remaining in the one, and also that received by the other, was very nearly half the original charge.

The experiments on which the author principally relies in support of the correctness of his views relative to induction being exerted in curved lines, are the following: a brass ball being laid on the top of an excited cylinder of shell-lac placed vertically, the charge which a carrier ball received when brought to different points near to the brass sphere was measured by means of the electrometer; and it was inferred, from the character of the electricity, that the charge was one by induction, and from its measure, that it proceeded in curved lines. By substituting for the brass sphere a disc of metal above the shell-lac cylinder, it was found that when the carrier ball was brought near to the middle of the disc no charge was communicated, although a sensible one was obtained at the edge of the disc, and also at a point above its centre, further removed from the excited cylinder. Corresponding and very striking results were obtained when a brass hemisphere was placed on the top of the cylinder of lac. The charge communicated at the centre of the hemisphere was only one-third of that obtained at the edge of its periphery; but by taking it at a point at some height above the centre, and consequently much further removed from the inducing cause, the charge was nearly equal to that of the periphery. Here, the author remarks, the induction fairly turned a corner, exhibiting both the curved lines or courses of its action, when disturbed from their rectilineal form by the shape, position and condition of the metallic hemisphere; and also a lateral tension, so to speak, of these lines on one another; all depending on induction being an action of the contiguous particles of the dielectric thrown into a state of polarity and tension, and mutually related by their forces in all directions. In the foregoing experiments the dielectric was air; but they were afterwards varied by substituting a fluid, as oil of turpentine, and likewise a few solid dielectrics, namely, shell-lac, sulphur, carbonate and borate of lead, flint-glass, and spermaceti, and with these, corresponding results were obtained. These results, the author considers, cannot but be admitted as arguments against the received theory of induction, and in favour of that which he has put forth.

In the course of these experimental researches, some effects due to conduction, which had not been anticipated, and which were similar to the residual charge in the Leyden jar, had been obtained with such bodies as glass, lac, sulphur, &c. If the inductive apparatus, fitted with a hemispherical cup of shell-lac, after having remained charged for fifteen or twenty minutes, was suddenly and perfectly discharged, and then left to itself, it would gradually recover a very sensible charge; the electricity which thus returned from an apparently latent to a sensible state being always of the same kind as that given by the charge. This return charge is attributed to an actual penetration, by conduction, of the charge to some distance within the dielectric at each of its two surfaces, and several experiments are adduced in support of this view. With shell-lac and spermaceti the return charge was considerable; with glass and sulphur it was much less; but with air, no decided effect of the kind could be obtained. As this was an effect which might interfere with the results, in the method the author adopted for deciding the question of specific inductive capacity, and as time was requisite for this penetration of the charge, its influence on these results was guarded against by allowing, between the successive operations, as little time as possible for this peculiar action to arise.

The author thus states the question of specific inductive capacity which he had proposed to investigate:—Suppose A an electrified plate of metal suspended in the air, and B and C two exactly similar plates, placed parallel to and on each side of A, at equal distances, and uninsulated; A will then induce equally towards B and C. If in this position of the plates, some other dielectric than air, as shell-lac, be introduced between A and C, will the induction between them remain the same; or will the relation of C and B to A be altered by the difference of the dielectrics interposed between them?

The experiment of Coulomb, from which it appeared that a wire surrounded by shell-lac took exactly the same quantity of electricity from a charged body, as the same body took in air, seemed to the author to be no proof of the truth of the assumption, that, under such variation of the circumstances as he had supposed, no change would occur. Entertaining these doubts as to the conclusions deducible from Coulomb's result, he had the apparatus previously described constructed, as being well adapted for this investigation. After rejecting glass, resin, wax, naphtha, oil of turpentine, and other substances, as unfit for the purpose in view, he chose shell-lac as the substance best calculated to serve as an experimental test of the question.

For the purpose of comparing the inductive capacities of shell-lac and air, a hemispherical cup of shell-lac was introduced into the lower hemisphere of one of the inductive apparatus, so as to nearly fill the lower half of the space between the two spheres; and their charges were divided in the manner already described; each apparatus being used in turn to receive the first charge, before its division with the other. As the two instruments were known to have equal inductive powers when air was contained in both, any deficiencies resulting from the introduction of the shell-lac would show a peculiar action in it, and, if unequivocally referable to a specific inductive influence, would establish the point in question.

The air apparatus being charged, and its disposable charge being 290°, this charge was divided between the two. After the division the charge in the lac apparatus was 113°, and in the air apparatus 114°. From this it appears, that whilst by the division the induction through the air lost 176°, that through lac gained only 113°. Assuming that this difference depends entirely on the greater facility possessed by shell-lac of allowing or causing inductive action through its substance than that possessed by air, then the capacity for electric induction would be inversely as the respective loss and gain; and assuming the capacity of the air apparatus as unity, that of the shell-lac apparatus would be $\frac{176}{113}$ or 1·55.

When the shell-lac apparatus was first charged, and then the charge divided with the air apparatus, it appeared that the lac apparatus, in communicating a charge of 118°, only lost a charge of 86°. This result gives 1·37 as the capacity of the lac apparatus.

Both these results, the author considers, require a correction; the former being in excess, the latter in defect. Applying this correction, they become 1·50 and 1·47. From a mean of these and several similar experiments, it is inferred that the inductive capacity of the apparatus having the hemisphere of lac is to that with air as 1·50 to 1.

As the lac only occupied one half of the apparatus containing it, the other half being filled with air, it would follow from the foregoing result, that the inductive capacity of shell-lac is to that of air as 2 to 1.

From all these experiments and from the constancy of their results the author deems the conclusion irresistible, that shell-lac does exhibit a case of *specific inductive capacity.*

Similar experiments with flint-glass gave its capacity 1·76 times that of air. Using in like manner a hemisphere of sulphur, it appeared that the inductive capacity of that substance was rather above 2·24 times that of air, and the author considers this result with sulphur as one of the most unexceptionable.

With liquids, as oil of turpentine and naphtha, although the results are not inconsistent with the belief, that these liquids have a greater specific inductive capacity than air, yet the author does not consider the proofs as perfectly conclusive.

A most interesting class of substances, in relation to specific inductive capacity, the gases or aeriform bodies, next came under the author's review.

With atmospheric air, and likewise with pure oxygen, change of density was found to occasion no change in the inductive capacity. Nor was any change produced, either by an increase of temperature or by a variation in the hygrometric state.

The details are then given of a very elaborate series of experiments with atmospheric air, oxygen, hydrogen, nitrogen, muriatic acid, carbonic acid, sulphurous acid, sulphuretted hydrogen, and other gases, undertaken with the view of comparing them one with another under a great variety of modifications. Notwithstanding the striking contrasts of all kinds which these gases present, of property, of density, whether simple or compound, anious or catious, of high or low pressure, hot or cold, not the least difference in their capacity to favour or admit electrical induction through them could be perceived. Considering the point established, that in all these gases induction takes place by an action of contiguous particles, this is the more important, and adds one to the many striking relations which hold among bodies having the gaseous form.

In conclusion, the author remarks, that induction appears to be essentially an action of contiguous particles, through the intermediation of which the electric force originating or appearing at a certain place, is propagated to or sustained at a distance, appearing there as a force of the same kind and exactly equal in amount, but opposite in its direction and tendencies. Induction requires no sensible thickness in the conductors which may be used to limit its extent, for an uninsulated leaf of gold may be made very highly positive on one surface, and as highly negative on the other, without the least interference of the two states, as long as the induction continues. But with regard to dielectrics, or insulating media, the results are very different; for their thickness has an immediate and important influence on the degree of induction. As to their quality, though all gases and vapours are alike, whatever be their state, amongst solid bodies, and between them and gases, there are differences which prove the existence of specific inductive capacities.

The author also refers to a transverse force with which the direct inductive force is accompanied. The experimental proof of the existence of such a force, in all cases of induction, is, from its bearing on the phænomena of electro-magnetism and magneto-electricity, of the highest importance; and we cannot but look forward with the greatest interest to the promised communication in which these and other phænomena relating to this subject will be reviewed.

Jan. 18.—"On the Variation of a Triple Integral." By Richard Abbott, Esq. F.R.A.S. Communicated by Benjamin Gompertz, Esq., F.R.S.

In the calculus of variations, the discovery of which has immortalized the name of Lagrange, that illustrious mathematician, by differentiating the function with respect to a new variable which enters into it, reduced the general problem of indeterminate maxima and minima to the solution of an equation depending on the variation of the given integral, whether single or multiple, and whose differential coefficient contains any number of variables, or which even depends on other integrals. The author investigates, in the present memoir, the case in which the given function is a triple integral; its variation being composed of two distinct parts, namely, a triple integral and another part, the determination of which must be sought from the limits of the triple integral.

"Explanation of the Phænomena of Intermitting Springs." By W. L. Wharton, Esq. Communicated by James F. W. Johnston, Esq., M.A., F.R.S. L. & Ed.

The author, considering the generally received explanation of intermitting springs, founded on the operation of a simple syphon, as being insufficient to account for the phænomena, inasmuch as the water which has risen above the lower side of the bend of the syphon will merely trickle down its longer leg, and be expended before it can fill the whole area of that part of the syphon, has proposed the following hypothesis for the solution of the difficulty. He conceives that the stream, while falling obliquely down the long leg of the syphon, is broken into drops, and carries along with it numerous air-bubbles, which, if the lower end of the tube have an abrupt bend upwards, will be impelled forwards, and escape at the open part; thus occasioning a rarefaction of the remaining air in the tube sufficient to ensure its full operation as a syphon. A model is described, which the author constructed for the purpose of illustrating and corroborating his views.

Jan. 25.—A paper was in part read, entitled, "Fourth Letter on Voltaic Combinations." Addressed to Michael Faraday, Esq., D.C.L., F.R.S., by John Frederic Daniell, Esq., F.R.S.

Feb. 1.—The reading of a paper, entitled "Fourth Letter on Voltaic Combinations, with reference to the mutual relations of the generating and conducting surfaces;" addressed to Michael Faraday, Esq., D.C.L., F.R.S., &c. By John Frederic Daniell, Esq., F.R.S., Professor of Chemistry in King's College, London, was resumed and concluded.

In this communication the author describes a series of experiments, made for the purpose of determining the distribution of the voltaic force from its source in the generating metal, as indicated by the deposition of reduced copper in the constant battery; and, considering that the voltaic combination most perfect in theory would be one formed by a solid sphere, or point, of the generating metal, surrounded by a hollow sphere of the conducting metal, with an intervening liquid electrolyte, he constructed an apparatus making as near an approximation as possible to these conditions. It consisted of two hollow brass hemispheres, applied to each other by exterior flanges, and rendered water-tight by an intervening collar of leather. In the centre of the hollow sphere thus formed, a ball of amalgamated zinc was suspended by a well-varnished copper wire, connected with one of the cups of a galvanometer, and was contained in a membranous bag holding the acid solution; the whole being introduced through a short tube in the top of the upper hemisphere, and the remaining space being filled with a saturated solution of sulphate of copper. The galvanic circuit was completed by wires establishing connexions between either hemisphere and the other cup of the galvanometer. For measuring the forces developed, sometimes the ordinary magnetic, but in the greater number of instances the calorific galvanometer of De la Rive was employed; the indications given by these instruments were noted, on the completion of the circuit, in various ways; and the deposition of copper in the hemispheres was examined after the apparatus had been in action for a certain number of hours.

The following are the conclusions which the author deduced from a series of experiments thus conducted:

1st. The force emanating from the active zinc centre diffuses itself over every part of the upper hemisphere, from which there is a good conducting passage for its circulation.

2nd. The same amount of force is maintained by either hemisphere indifferently; but when both conducting hemispheres are in metallic communication there is no increase of force.

3rd. Although the force is not increased, it spreads itself equally over the whole sphere.

4th. When one hemisphere is connected with the zinc centre by a short wire capable of affording circulation to the whole force, and the other hemisphere is connected by a long wire, through the galvanometer, with the same centre, the equal diffusion of the force over the whole sphere is maintained.

5th. There is no greater accumulation of precipitated copper about the point with which the conducting wires are brought into contact, and towards which the force diffused over the whole sphere must converge, than at any other point; proving that the force must diverge from the centre equally through the electrolyte, and can only have drawn towards the conducting wires in the conducting sphere itself. Other experiments showed that the force is but slightly increased by a great increase of the generating surface.

The author's attention was next directed to ascertaining the nature of the law according to which the force emanates from the zinc centre to the surrounding conducting sphere. With this view, a variety of experiments were made with the zinc in different positions in the interior of the sphere; and from these it appeared that, whatever may be its position, the whole force is the same. From these results it is inferred, that the force emanating from the zinc ball diffuses itself over the surrounding conducting sphere in obedience to the well-known law of radiant forces being in the inverse duplicate ratio of the distance.

Experiments of the same kind were likewise made with the previous combination inverted, that is, with a small copper ball in the interior of a large hollow sphere of zinc; and from these the author concludes that, in this case also, the law of radiation is maintained, although the force is reduced to one half of that obtained from the former combination.

In order to ascertain the effect of cutting off the lateral radiation from the zinc ball, it was placed in a glass tube, six inches long, within half an inch of the lower aperture, over which a piece of membrane was tied, and the tube plunged into the solution of copper contained in a brass hemisphere, so as to rest upon the bottom. The results obtained by this arrangement, as also those when the zinc ball was raised in the tube to the surface of the solution, showed that the action of the zinc ball had been propagated from the aperture of the glass tube, as from a centre, diverging from this in the solution.

The experiments next described appear to have an important bearing on a question of vital interest in the theory of electricity, which has been discussed by Mr. Faraday, in a paper recently read to this Society: viz., whether the forces emanating from a centre of electric action act, like other central forces, in straight lines; or whether they are propagated from particle to particle in the surrounding matter, and may, consequently, when obstacles interfere with their rectilinear propagation, exert their influence in curved lines. An elliptical plate of copper, one side of which was covered with lac varnish, was placed in an earthen pan, with the varnished side upwards, and covered to the depth of a few inches with the acid solution of copper. The zinc ball, placed in the tube half an inch from the diaphragm, was plunged just below the surface of the solution, and the circuit being completed, the galvanometer indicated an action nearly equal to that which had been previously observed when both sides of the copper had been exposed. The under side of the copper presented the appearance of a border of precipitated compact pink copper, varying from 1¼ to ¾ of an inch in width, and the remainder was covered with precipitated copper of a darker red colour, into which the border gradually passed; and similar results were obtained with a circular disc of copper, having one side varnished. It hence appears, that the under surface, which, by itself, is capable of sustaining from the ball in the centre of the solution an action nearly as great as the upper surface, when combined with the latter adds no more than about one-eighth part of its efficiency; and whereas, with the upper surface, the action varies in some inverse ratio of the distance of the generating from the conducting surface, with the under surface, there is a maximum point, on both sides of which it decreases: and this point is doubtless dependent on the angle at which the force which radiates from the ball meets the edge of the plate. The author having been led to the conclusion, that the force developed by voltaic combinations is subject to the law of radiant forces, had been utterly at a loss to understand how, upon this hypothesis, it could extend its influence to the side of a plate opposite to that to which it was directed in right lines; but having perused Mr. Faraday's "Eleventh series of experimental researches in Electricity," all his own results appeared to fall in naturally with the general views therein explained. He considers, that the direction of the force through an electrolyte may be expressed in the very words employed in that paper to describe that of the direct inductive force in statical electricity, simply substituting the term *Electrolyte* for *Dielectric*, and the term *Current* for *Induction*.

Experiments are further described, in which the effects of various combinations of different generating and conducting surfaces, placed at different distances apart, were measured by the calorific galvanometer, from which the following conclusions are drawn:

1st. That the energy of the force is about sextupled by the absorption of the hydrogen at the conducting surface; except in the case of equal plates, when it is more than quadrupled.

2nd. That the effect of distance is much more decided in the instances where the amount of the circulating force is greater, than in the contrary cases.

3rd. That the amount of force put into circulation from a large surface of zinc towards a central ball of copper, is, as in former instances of similar combinations, about one half of that from the reverse arrangement.

4th. That a ball of zinc, exposing a surface of 3·14 square inches, placed over the centre of a plate of copper, exposing on its two sides a surface of 28 square inches, sustains an action of nearly the same amount as a plate of zinc, of the same dimensions as the copper, placed at the same distance.

In conclusion, the author remarks, that the principal circumstance which limits the power of an active point within a conducting sphere, in any given electrolyte, is the resistance of that electrolyte, which increases in a certain ratio to its depth or thickness; and this thickness may virtually be considered the same wherever the included point may be placed, but increases with the diameter of the sphere. In an insulated hemisphere, however, the approximation of the active point to the lower surface virtually decreases the thickness of the electrolyte, and consequently the force increases. In this respect, the action of a point upon a plate may be considered the same as upon an indefinitely large hemisphere, towards which, as the point approaches, the force increases.

Feb. 8.—A paper was read, entitled, "Researches towards establishing a Theory of the Dispersion of Light", No. IV.* By the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry in the University of Oxford.

In his former communications to the Royal Society the author had instituted a comparison of the results of observation and of theory with regard to the dispersion of light, in the instances of the respective indices for the standard rays in fifteen different cases of transparent media; and had found a sufficiently close agreement in the cases which gave the lower numbers; but there yet appeared to be an increasing discrepancy as an advance was made towards the higher. The theoretical formula employed in this investigation was one derived from the undulatory hypothesis, by a process involving some limitations, which rendered it only approximative. By pursuing the calculations to a greater degree of development, or by adopting methods of a more precise character, such as those of M. Cauchy and of Mr. Kelland, the author was led to hope that a more close coincidence might be obtained. The formulæ of M. Cauchy, however, involved calculations of so elaborate and overwhelming a character, that he was induced to make trial of the method of Mr. Kelland, applying it, in the first instance, to the case of the most highly dispersive substance, namely, oil of Cassia, in which the greatest discrepancy had before appeared.

The object of the present communication is to state the results obtained, together with the necessary data employed in the calculations; and also to elucidate the general method, so as to render it more easily applicable to other cases which may arise in the further prosecution of the determination of specific indices. For this purpose a general statement is given of Mr. Kelland's method, in whose formulæ, it is easy, knowing the value of the wave-length in air, and taking the indices as given by observation for that particular medium, to introduce the values of the wave-length in the medium. Two of the constants are then determined for that medium; and by the aid of these, combined with the indices given by observation, a value of the third constant is deduced for each ray: and the verification of the theory will result from the equality of the respective values of this latter constant thus obtained.

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* Abstracts of Prof. Powell's former papers on this subject have been given in vol. vi. p. 374; vol. viii. p. 413; vol. x. p. 221: see also vol. xi. p. 477.

The author then gives tables exhibiting the comparison of observed refractive indices with the results of Mr. Kelland's theory; first, in the case of sulphuret of carbon, at a temperature of 12° (centigrade); next, of the same substance at 22°; and lastly, of oil of Cassia: from which it appears, that the accordance between the results of observation and of theory is sufficiently within the limits of the errors in the experimental data to satisfy all reasonable expectation.

A paper was also in part read, entitled, "Experimental Researches in Electricity." Twelfth Series. By Michael Faraday, Esq., D.C.L., F.R.S., &c.

A letter was read from Dr. Marshall Hall, in reply to a note contained in the paper of Mr. Newport, published in the last volume of the Philosophical Transactions.

[To be continued.]

ROYAL IRISH ACADEMY.

Inaugural Address delivered by the President, Sir W. Rowan Hamilton, *A.M., F.R.S., Astronomer Royal of Ireland, December* 11, 1837.

MY LORDS AND GENTLEMEN OF THE ROYAL IRISH ACADEMY,

The position in which your kindness has placed me, entitles me, perhaps, to address to you a few remarks. Called by your choice to fill a chair, which Charlemont and Kirwan, and others, not less illustrious, have occupied, I cannot suffer this first occasion of publicly accepting that high trust to pass in silence by, as if it were to me a thing of course. Nor ought I to forego this natural opportunity of submitting to you some views respecting the objects and prospects of this Academy, which, if they shall be held to have no other interest, may yet be properly put forward now, as views, by the spirit at least of which I hope that my own conduct will be regulated, so long as your continuing approbation shall confirm your recent choice, and shall retain me in the office of your President.

First, then, you will permit me to thank you for having conferred on me an honour, to my feelings the most agreeable of any that could have been conferred, by the unsolicited suffrages of any body of men. Gladly, indeed, do I acknowledge a belief which it would pain me not to entertain, that friendship had, in influencing your decision, a voice as potent as esteem. An Irishman, and attached from boyhood to this Academy of Ireland, I see with pleasure in your choice a mark of affection returned. But knowing that the elective act partakes of a judicial character, and that the exercise of friendship has its limits, I must suppose that the same long attachment to your body, which had won for me your personal regard, appeared also to you a pledge, more strong than promises could be, that if any exertions of mine could prevent the interests of the Academy from suffering through your generous confidence, those exertions should not be withheld; and that you thought they might not be entirely unavailing. After every deduction for kindness, there remains a manifestation of esteem, than which I can desire no higher honour, and for which I hope that my conduct will thank you better than my words.

And yet, Gentlemen, it is to me a painful thought, that the opportunity for your so soon bestowing this mark of confidence and esteem has arisen out of the deaths, too rapidly succeeding each other, of the two last Presidents of our body, who, while they are on public grounds deplored, and for their private worth were honoured and beloved by all of us, must ever be remembered by me with peculiar love and honour;—Brinkley, who introduced me to your notice, by laying on your table long ago my first mathematical paper; and Lloyd, whose works, addressed to the University of Dublin, first opened to me that new world of mind, the application of algebra to geometry. But of these personal feelings, the occasion has betrayed me into speaking perhaps too much already. Into that fault, I trust, I shall not often fall again. I pass to the exposition of views respecting the objects and prospects of our Society.

The Royal Irish Academy was incorporated (as you know) in 1786, having been founded a short time before, for the promotion generally, but particularly in Ireland, of Science, Polite Literature, and Antiquities. Its objects were to be the *True*, the *Beautiful*, and the *Old*: with which ideas, of the True and Beautiful, is intimately connected the coordinate (and perhaps diviner) idea of the *Good*. So comprehensive, therefore, was the original plan of this Academy, that it was designed to include nearly every object of human contemplation, and might almost be said to adapt itself to all conceivable varieties of study; insomuch that scarce any meditation or inquiry is directly and necessarily excluded from a place among our pleasant labours: and precedents may accordingly be found, among our records, for almost every kind of contribution. If only a diligence and patient zeal be shown, such as befit the high aims of our body; and if due care be taken, that the spirit of love be not violated, nor brother offend brother in anything; no strict nor narrow rules prevent us from receiving whatever may be offered to our notice, with an indulgent and joyful welcome. And though we meet only as studious, meditative men, and abstain from including among our objects any measures of immediate, outward, practical utility, such as improvements in agriculture, or other useful arts,—a field which had been occupied, in this metropolis, by another and elder society, before the institution of our own; yet no philosopher nor statesman, who has reflected sufficiently on the well-known connexion between theory and practice, or on the refining and softening tendencies of quiet study, will think that therefore we must necessarily be useless or unimportant as a body, to Ireland, or to the Empire.

The *object* of this Academy being thus seen to be the encouragement of STUDY, we have next to consider the *means* by which we are to accomplish, or to tend towards accomplishing that object. Those means are of many kinds, but they may all be arranged under the two great heads of *inward* and *outward* encouragement; or, in other words, *stimuli* and *assistances*; in short, SPURS and HELPS to study. The encouragement that is given may act as supplying a motive, or as removing a hindrance; it may be indirect, or it may be direct: invisible or visible; mental or material. Not that these two great kinds of good and useful action are altogether separated from each other. On the contrary, they are usually combined; and what gives a stimulus, gives commonly a facility too. In our *meetings*, for example, the *stimulating* principle prevails; yet in them we are not only caused to feel an increased *interest* in study generally, through the operation of that social spirit, or spirit of sympathy, of which I spoke so largely, in the presence of most of you, at the meeting of the British Association* in this city; but also are directly *assisted* in pursuing our own particular studies, by having the results of other studious persons early laid before us, and commented upon, by themselves and by others, in a fresh familiar way. We are not only spurred but helped to study, by mixing freely with other students.—A *library*, again, is designed rather to *assist* than to stimulate; and yet it is impossible for a person of ardent mind to contemplate a well-selected assemblage of books, containing what Milton has described as "the precious life-blood of a master-spirit, embalmed and treasured up on purpose to a life beyond life," without feeling a deep desire to add, to the store already accumulated, some newer treasure of his own. Our library, then, spurs as well as helps. The *prizes* which from time to time we award for successful exertion in the various departments of study, might seem to be *stimulants* only; yet if we were to act sufficiently upon the spirit of precedents, of which we have several among our past proceedings, and which allow us to make our awards in part pecuniary, as well as honorary, they might become important *assistances*, and not merely *excitements* to study; they might serve, for instance, to enrich the private libraries of the authors on whom they were conferred. Why might we not, for example, instead of giving one gold *medal*, which can (according to the custom of this country) only be gazed at for a while and then shut up, allow the author who has been thought worthy of a prize to select any *books* for himself, which he might think most useful for his future researches, within a certain specified limit of expense; and then not only purchase those books for him out of our own prize funds, but also stamp them with the arms of the Academy, or otherwise testify that they were given to him by us as a reward? Or might not some such presentation of books be at least combined with the presentation of medals? But the whole system of prizes will deserve an attentive reconsideration, for which this is not the proper time nor place; and anything that I may now have said, or may yet say on that subject, in this address, is to be looked upon as merely intended to *illustrate* a few general *views* and principles, and not as any *proposal* of *measures* for your adoption; since, upon measures of detail, I have not as yet even made my own mind up; and am aware that, by the constitution of our Society, all measures of that kind must first be matured in the Council, before they are submitted to the Academy at large for final sanction or rejection.

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* See the Address printed in the Fifth Report of the British Association for the Advancement of Science.—*Note by* PRESIDENT.

The publication of our *Transactions* is another field of action for our body, and perhaps the most important of all; in which it is not easy to determine whether the stimulating or the assisting principle prevails; so much both of inducement and of facility do they give to study and to its communication. It is, indeed, a high reward for past, and inducement to future labours, to know that whatever of value may be elicited by the studies of any members of this body, (nor are we to be thought to wish to *confine* the advantage to *them*,) is likely or rather is sure to be adopted by the Society at large, and published to the world, at least to the learned world, in the name and by the order of the whole:—the responsibility for any errors of detail, and the credit for any merit of originality, remaining still in each case with the author, while the Academy exercises only a right of preliminary or *primâ facie* examination, and a superintendence of a general kind. Nay, the more rigorous this preliminary examination is, and the more strict this general superintendence, the greater is the compliment paid to the writer whose productions stand the test; and the more honourable does it become to any particular essay, to be admitted among the memoirs of a Society, in proportion as those memoirs are made more select, and expected and required to be more high. But besides this honorary stimulus, which we should all in our several spheres exert ourselves to make more effective, by each endeavouring, according to his powers, to contribute, or to judge, or to diffuse, there is also a powerful and direct *assistance* given to study, by the publishing of profound intellectual works at the expense of a corporate body, rather than at the expense of individuals; a course which spares the private funds of authors and of readers; and thus procures, for the collections of learned and studious men, many works of value, which otherwise might never have appeared. Indeed, the publication of Transactions has long been regarded by me as the most direct and palpable advantage resulting from the institution of scientific and literary societies like our own; and, I believe, that I expressed myself accordingly, on the occasion* to which I lately alluded. But having *then* to deal with science only, I felt that it was unnecessary, and would have been improper for me to have introduced any view of the connexion and contrast between science and other studies, which are, not less than science, included among the objects of this Academy, and may therefore be fitly, if briefly, brought now before your notice. The union of all studies is indeed that at which we aim; but the three great departments, which our founders distinguished without dividing, may now also with advantage be distinctly considered, and separated, that they may be recombined; a clearness of conception being likely to be thus attained, without any sacrifice of unity.

Directing our attention, therefore, first to science, or the study of the True,—

> Inter sylvas Academi quærere verum,—

we find that, even when thus narrowed, the field to be examined is still so wide as to make necessary a minuter distinction; whether we would inquire, however briefly, what has been already done by this Academy, or what may fitly be desired and hopefully proposed to be done. Were we to rush into this inquiry without any previous survey of its limits, and, as were natural, allowed ourselves to begin by considering the actual and possible relation of our studies to the primal science, or First Philosophy, the Science of the Mind itself; we might easily be drawn, by the consideration of this one topic, into a discussion, interesting indeed, and (it might be) not uninstructive, but of such vast extent as to leave no room for other topics, which ought even less to be omitted, because they have hitherto come, and are likely to come hereafter, more often than it, before our notice, in actual contributions to our Transactions. Indeed I think it prudent at this moment to resist altogether the temptation of expatiating on this attractive theme of Philosophy, eminently so called; and to content myself with remarking, that as metaphysical investigation has more than once already found place among the scientific labours of this Academy, so ought it to take rank among them still, and to reappear in that character, from time to time, in our pages.

Confining ourselves, therefore, at present to Science, in the usual acceptation of the term, and inquiring what are its chief divisions, in relation mainly to the connected distribution or classification of scientific essays in our Transactions, we soon perceive that three such parts of science may conveniently be distinguished from each other, and marked out for separate consideration; namely those three, which, with some latitude of language, are not uncommonly spoken of as Mathematics, Physics, and Physiology. The first, or *mathematical* part, being understood to include not only the pure, but the mixed mathematics; not only the results of our original intuitions of time and space, but also the results of the combination of those intuitions with the not less original notion of cause, and with the observed laws of nature, so far and no farther than that ever-widening sphere extends, within which observation is subordinate to reasoning; in short, all those deductive studies in which Algebra and Geometry are dominant, though the dynamical and the physical may enter as elements also. The second, or *physical* part of science, embracing all those inductive studies respecting unliving or unorganized bodies, which proceed mainly through outward observation or experiment, and can as yet make little progress in "the high *priori* road." And finally, the third, or *physiological* part, including all studies of an equally inductive kind, respecting living or organized bodies. (I do not pretend that this arrangement is the most philosophical that can be imagined, but it may suffice for our present purpose.)

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* See Address, already cited, p. xlvi.—*Note by* PRESIDENT.

In all these divisions of science, and in several subdivisions of each, our published Transactions contain many valuable essays; and there seems to be no cause for apprehension that in *this* respect, at least, (if indeed in any other,) the Academy is likely to lose character. Death has, it is true, removed some mighty names from among us—elders and chiefs of our society: but the stimulus and instruction of their example have not been thrown away: an ardent band of followers has been raised up by themselves to succeed them. To keep the trust thus handed down, is an arduous, but noble charge, from which it is not to be thought that any here will shrink, whatever his share of that charge may be.

And yet, while Mathematics and Physics seem likely not to be neglected here, or rather certain to be ardently pursued, it may be pardoned me if I express a fear and a regret, that Physiology, or more precisely, the study of the phænomena and laws of life and living bodies, has not been represented lately in the published Transactions of our Academy, to a degree correspondent with the eminence of the existing School of physiological study in Dublin. Our medical men and anatomists, our zoologists and botanists also, will take, I hope, this little hint in good part. They know how far I am from pretending to criticise their productions, and that I only wish to have more of their results brought forward here, for the instruction of myself and of others. *That* is not, I think, too much to ask from gentlemen who have subscribed the obligation which is signed by every member of this body, and who are qualified, by intellect and education, to take an enlarged yet not exaggerated view of the importance of a central society. I know that many other, and, indeed more appropriate outlets exist, for the publication of curious, isolated, or semi-isolated facts: but it is not so much remarkable *facts*, as remarkable *views*, that I wish to see communicated to us and through us to the world; although such views ought, of course, to be illustrated and confirmed by facts.

It seems possible, that in each of the three great divisions of science already enumerated, our Transactions may be enriched in future, through a judicious system of rewards, (of the kinds to which I lately alluded,) intended to encourage contributions of a more elaborate kind than usual, from strangers as well as from members of our body. It has appeared, for example, to some members of your Council, and to me, that for each of those three divisions of science a *triennial prize* might be given; these three triennial prizes succeeding each other in such rotation, for mathematics, physics, and physiology, that a prize should be awarded every year, on some one principal class of scientific subjects, for the best essay which had been communicated for publication, on any subject of that class, whether by a member or by a stranger, during the three preceding years. A plan of this sort has been lately tried, and (it would seem) with advantage, in the distribution of the Royal Medals entrusted by the late King* to the Royal Society of London; and the principle is not unsanctioned by you, that a greater range of investigation may sometimes be allowed to the authors of prize-essays, than the terms of an ordinary prize-question would allow. So that it only remains for your Council to consider and report to you, as they are likely soon to do, to what extent this principle may advantageously be pushed, and by what regulations it may conveniently be carried into effect. In saying this, I do not presume to pronounce that it is expedient to give up entirely the system of proposing occasionally prize-questions, of a much more definite kind than those to which I have been referring as desirable; but thus much I may venture to lay down, that original genius in inquirers ought to be as far indulged as it is possible to indulge it, both in respect of subject and of time; and that due time ought also to be allowed to those members of a Scientific Society, on whom is put the important and delicate office of pronouncing an award in its name.

The length at which I have spoken of our relations to Science, as a Society publishing Transactions, though far from exhausting that subject, leaves me but little room, in this address, to speak of our relations to Literature and Antiquities; subjects to which, indeed, I am still less able to do justice, than to that former theme. But the spirit of many of my recent remarks applies to these other subjects also; and you will easily make the application, without any formal commentary from me. A word or two, however, must be said on some points of distinction and connexion between the one set of subjects and the other.

As, in Science, or the study of the *True*, the highest rank must be assigned to the science of the investigating Mind itself, and to the study of those Faculties by which we become cognizant of truth; so, in Literature, or the study of the *Beautiful*, the highest place belongs to the relation of Beauty to the mind, and the study of those essential Forms, or innate laws of taste, in and by which, alone, man is capable of beholding the beautiful. Above all particular fair things is the Idea of Beauty general: which in proportion as a man has suffered to possess his spirit, and has, as it were, won down from heaven to earth, to irradiate him with inward glory, in the same proportion does he become fitted to be a minister of the spirit of beauty, in the poetry of life, or of language, or of the sculptor's, or the painter's art. The mathematician himself may be inspired by this in-dwelling beauty, while he seeks to behold not only truth but harmony; and thus the profoundest work of a Lagrange may become a scientific poem. And though I am aware that little can be communicated by expressions so general (and some will say so vague) as these, and check myself accordingly, to introduce some remarks more specific and definite; yet I will not regret that I have thus for a moment attempted to give words to that form of emotion, which many here will join with me in acknowledging to be the ultimate spring of all genuine and genial criticism, in literature and in all the fine arts. For we, in so far as we are an Academy of Literature, are also a Court of Criticism;—Criticism which is to Beauty, what Science is to Nature. Between the divine of genius and the human of enjoyment, we hold a kind of middle place; creating not, nor merely feeling, but aspiring to understand: and yet incapable of rightly understanding, unless we at the same time sympathize.

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* And continued by her present Majesty, whose gracious intention of becoming Patroness of the Royal Irish Academy has been made known since the delivery of this Address.—*Note by* PRESIDENT.

To express myself then in colder and more technical terms, I should wish that metaphysico-ethical and metaphysico-æsthetical essays,—those which treat generally of the beautiful in action and in art, and are connected rather with the study of the beauty-loving mind itself, than of the particular products or objects which that mind may generate or contemplate,—should be considered as entitled to the foremost place among our literary memoirs. After these *à priori* inquiries into the PRINCIPLES of beauty, which are rather *preparatory* to criticism than criticism itself, or which, at least, deserve to be called *criticism universal*, should be ranked, I think, that important but *à posteriori* and inductive species of criticism, which, from the study of some actual master-pieces, collects certain great RULES as *valid*, without deducing them as *necessary* from any higher principles. And last, yet still deserving of high honour, I would rank those researches of DETAIL, those particulars, and helps, and applications of criticism, which, if they be, in a large philosophical view, subordinate and subsidiary to principles, and to rules of universal validity, yet form perhaps the larger part of the habitual and ordinary studies of men of erudition; such as the differences and affinities of languages, and the explication of obscure passages in ancient authors. Whatever metaphysical preference I may feel for inquiries of the two former kinds, no one, I hope, will misconceive me as speaking of this last class of researches with any other feelings than those of profound respect, and of desire and hope to see them cultivated here; nor as presenting other than hearty congratulations to the Academy on the fact, that whereas no single paper on Literature appeared in our last volume, two memoirs, interesting and erudite, have been presented to us, and probably are by this time printed, to be in readiness for our next publication;—one, on the Punic Passage in Plautus, by a near and dear relative of my own; and the other, on the Sanscrit Language, by a gentleman of great attainments and of high station in our national University: from which seat of learning, it seems not too much to hope, that we shall soon receive many other contributions in the department of Polite Literature, as well as in other departments. It is, of course, understood that the awarding of prizes is not to be confined to scientific papers, but is to be extended, as indeed it has always been, under some convenient regulations, to literary and antiquarian papers also.

I was to say a few words respecting that other department of our Transactions, namely Antiquities, or the study of the Old; and if, at this stage of my address, those words must be very few, I regret this circumstance the less, because I know that the study is deservedly a favourite here, and that I am surrounded by persons who are, beyond all comparison, more familiar with the subject than myself.

In general, I may say, that whether the study of Antiquities be regarded in its highest aspect, as the guardian of the purity of history,—the history of nations and mankind; or as ministering to literature, by recovering from the wreck of time the fragments of ancient compositions; or as indulging a natural and almost filial curiosity to know the details of the private life of eminent men of old, and to gaze upon those relics which invest the past with reality, as the palæontologist from his fossils reconstructs lost forms of life: in all these various aspects, the study is worthy to interest any body of learned men, and to occupy a considerable part of the Transactions of any society so comprehensive as our own. The historian of the Peloponnesian war was also himself an antiquarian; and prefaced that work which was to be "a possession for ever," by an inquiry into the antiquities of Greece. And while he complained of the *οὕτως ἀταλαίπωρος τοῖς πολλοῖς ἡ ζήτησις τῆς ἀληθείας*, that easy search after truth which cost the multitude nothing; he also claimed to have arrived at an *ἑξῆς τεκμηρίον*, a linked chain of antiquarian proof, by which he could establish his correction of their errors. Indeed, the uninitiated are apt to doubt,—perhaps too they may sometimes smile,—when they observe the earnest confidence which the zealous Antiquary reposes in results deduced from arguments which seem to them to be but slight; nor dare I say that I have never yielded to that sort of sceptical temptation. But I remember a fact which ought to have given me a lesson, on the danger of hastily rejecting conclusions which have been maturely considered by others. A learned Chancellor of Ireland, now no more, assured me often and earnestly, that he gave no faith to the inductions of astronomers respecting the distances and sizes of the sun and moon; and hinted that he disliked our year, for containing the odd fraction of a day. Yet this was a man, not only of great private worth, but of great intellectual power, and eminent in his profession as in the state. Astronomers and methematicians, it may be, look sometimes on other inductions with a not less unfounded incredulity. It is one of the advantages of an Academy, so constituted as ours is, that it brings together persons of the most different tastes and the most varied mental habits, and teaches them an intellectual toleration, which may ripen into intellectual comprehension. Thus, while the antiquary catches from the scientific man his ardent desire for progression, and for that clearer light which is future, the man of science imbibes something in return of the antiquarian reverence for that which remains from the past. The literary man and the antiquary, again, re-act upon each other, through the connexion of the Beautiful and the Old, which in conception are distinct, but in existence are often united. And finally, the scientific man learns elegance of method from the man of literature, and teaches him precision in return.

Before I leave the subject of Transactions, I may remark that their value, both as stimulants and as assistants to study, must much depend on the rapidity and extent of their circulation, and on the care that is taken to put them as soon as possible into the hands or within the reach of studious men abroad. Reciprocally it is of importance that measures should be taken for obtaining speedy information here of what is doing by such men in other countries. On both these points, some reforms have lately been made, but others still are needed, and will soon be submitted to your Council. On these and all questions of improvement, I rely upon receiving the assistance of all those gentlemen who are in authority among us; but especially am encouraged by the hope of the cordial co-operation of your excellent Vice-President, Professor Lloyd, who has done so much already for this Academy, in these and in other respects.

It may deserve consideration, as connected with the last-mentioned point, whether Reports upon some foreign memoirs of eminent merit, accompanied by extracts, and, perhaps, translations, might not sometimes be advantageously called for. There is, I think, among our early records, some hint that the Academy had once a paid Translator. It may or it may not be expedient to revive the institution of such an office; or to give direct encouragement to the exertions of those,* who without any express reference to our own body, work in this way for us, while working for the public; but no one can doubt that it is desirable to diminish the too great isolatedness which at present exists among the various learned bodies of the world. The Reports of the British Association on the actual state of science in each of its leading subdivisions, do not exactly meet the want to which I have alluded; because, upon the whole, they aim rather at condensing into one view the ultimate *conclusions* of scientific men in general, than at diffusing the fame and light of individual scientific genius, by selecting some few great foreign works, and making known at home their *method* as well as their results. Besides, we must remember, that far as that colossal Association exceeds the body to which we belong, in numbers, wealth, and influence, yet in plan it is less comprehensive; since it restricts itself to science exclusively, while we aspire, as I have said, to comprehend nearly the whole sphere of thought,—at least of thought as applied to merely human things: in making which last reservation, I shall not, I hope, be supposed wanting in reverence for things more sacred and divine.

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* For instance, Mr. Richard Taylor, of London, F.S.A., &c., who lately began to publish *Scientific Memoirs*, selected and translated from the Transactions of Foreign Academies of Science, and other foreign sources; which valuable publication is now suspended for want of sufficient support from the public.—*Note by* PRESIDENT.

With that powerful and good Association, however, we should endeavour to continue always on our present, or if possible, on closer terms of amicable relation. I need not say that we should also aim to preserve and improve our friendly relations with all the other Scientific, Literary, and Antiquarian Societies, of these and of foreign countries. Especially we ought to regard, with a kind of filial feeling of respect and love, the Royal Society of London—that central and parent institution, from which so many others have sprung; over which Newton once presided; and in which our own Brinkley wrote. While feelings of this sort are vigilantly guarded, and public and private jealousies excluded vigilantly, a vast and almost irresistible moral weight belongs to companies like these, of studious men; and, amid the waves of civil affairs, the gentle voice of mind makes itself heard at last. Societies such as ours, if they do their duty well, and fulfil, so far as in them lies, their own high purpose, become entitled to be regarded as being, on all purely intellectual and unpolitical questions, hereditary counsellors of crown and nation. The British Association has already made applications to government with success, for the accomplishment of scientific objects; and I am not without hopes that our own recent memorial, for the printing, at the public expense, of some valuable manuscripts in our possession, adapted to throw light on history, and interesting in an especial degree to us as Irishmen, will receive a favourable consideration.

On the present occasion, which to me is solemn, and to you not unimportant, I may be pardoned for expressing, in conclusion, the pleasure which it gives me to believe, that while we cautiously abstain from introducing polemics or politics, or whatever else might cause an angry feeling in this peaceful and happy society, some great and fundamental principles, of duty to Heaven and to the state, are universally recognised amongst us. Admitted at an early age to join your body, I now have known you long, and hope to know you longer; but have never seen the day, and trust that I shall never see it, when piety to God, or loyalty to the Sovereign, shall be out of fashion here.

LX. *Notices respecting New Books.*

A Letter from Alexandria on the Evidence of the practical Application of the Quadrature of the Circle in the Configuration of the Great Pyramids of Gizeh. By H. C. AGNEW, Esq. London: Longman and Co., 1838, 4to. pp. 57; Plates 9.

THE author conceives that while the principal purpose which the ancient Egyptians had in view, in the construction of the pyramids, was that of their serving as sepulchral monuments, they at the same time intended to perpetuate, by their means, the knowledge they had acquired of certain geometrical relations between lines appertaining to the circle; and that, conformably with this intention, they proceeded upon a regular system of geometrical construction, in selecting those relative dimensions in regard to the length of the sides of their bases, their perpendicular altitudes, the length of their edges, and their respective angles of inclination of the faces to one another, and to the horizontal plane, which expressed various geometrical relations, derived from the quadrature of the circle, and the proportions between its area and that of squares inscribed within, or circumscribed about the circle, according to certain methods.

He has taken great pains to obtain the most accurate measurements of the lengths of the sides, and the inclinations of the faces and edges to the horizon, of each of the three great pyramids of Gizeh, both by collecting and comparing the statements of former travellers, and by verifying and correcting them from his own personal observation: and he presents the whole of his numerous measurements in a tabular form.

In the development of his hypothesis, the author proceeds on the principle that the three pyramids were all parts of one system; all their dimensions being derivable from one and the same parent circle, which he terms the *holy circle*, and the properties of which they were all intended to represent. Then describing an inscribed, and a circumscribed square, and also a square, partly the one and partly the other, (that is, having one of its sides tangent to the circle, and the angular points terminating the opposite side being situated in the circumference of the same circle,) he infers from his measurements that the three pyramids are constructed with reference to these squares; inasmuch as he finds that the perpendicular, or altitude of the second pyramid, is the radius of a second circle, of which the circumference, joined to the circumference of the great circle, is equal to the united perimeters of the two squares; and that, in the third pyramid, the perpendicular is the radius of a circle, whose circumference is equal to the perimeter of its base.

The author enters at great length into the investigation of various other recondite geometrical properties of these congenerous squares and circles, which he conceives are exemplified and illustrated by the several proportions existing among the lines and angles of the system of pyramids of Gizeh.

LXI. *Intelligence and Miscellaneous Articles.*

IMPROVEMENTS IN MAGNETICAL APPARATUS BY THE REV. W. SCORESBY.

AT the meeting of the British Association, held in Bristol, in 1836, the Rev. W. Scoresby made a communication to the Physical Section, on an improved mode of construction in magnetic needles for compasses, &c. by the combination in a parallel series, *not* in contact, of several thin plates of tempered steel. A variation instrument, which he at that time exhibited, constructed on this principle, was stated to have a far greater directive energy than any instrument, of the nature of a compass, previously constructed. Since that period Mr. Scoresby has been pursuing, as opportunity offered, an extensive series of investigations on the subject; both as to the law of combination in steel plates and bars, and as to the effect of temper, thickness, &c. on the aggregate power; with the view of producing more powerful instruments for determining the delicate variations in, and the actual condition of, the earth's magnetism; a subject now engaging attention in some of the principal observatories in Europe. The results, which have been successful beyond the objects originally contemplated, have been recently communicated to the Institute of France. One of these results likely to be of much importance in magnetical science, to which it is extensively applicable, is that of producing permanent artificial magnets of almost unlimited power. On the principle of construction of compound magnets hitherto adopted, only a very limited number of bars could be combined with advantage, in consequence of the great deterioration of power occasioned by the condition of violence. Mr. Scoresby found, on combining very superior plates of tempered steel of two feet in length and about $\frac{1}{24}$th of an inch in thickness that the first six plates received so much power that no additions, however great the number, were capable of producing more, in the aggregate, than about double that power. Aiming, however, to counteract the tendency to such rapid deterioration, Mr. Scoresby made some magnetical combinations of *perfectly hard* steel plates, (which he has a ready method of magnetizing and testing,) by means of which an almost unlimited power can be obtained. Already this combination has been carried, with no inconsiderable augmentation of the aggregate energy, to the very last, to the extent of several dozens of hard plates, 15 inches in length, so as to produce, by such combination, a compound magnet of very extraordinary power for its mass. The application of this principle to apparatus for magnetic electricity will obviously be of much advantage for compactness and power; whilst the application of the discovery to variation needles, dipping needles, and, probably, to sea compasses also, promises to be of much importance in experimental science, as well as for practical and œconomical purposes. Mr. Scoresby's investigations have also led to other practical results, such as the means of testing most rigidly the quality and temper of steel plates, and of bars intended for compound magnets on the ordinary construction, by which the best plates can be selected and the most powerful combinations may be obtained.

ON THE CONSTITUTION OF SOME ORGANIC ACIDS. BY M. DUMAS AND M. LIEBIG.

Citric Acid.—Gay-Lussac and Thenard have analysed citrate of lead. Berzelius has since determined the composition of citric acid, citrate of lead, and fixed the constitution of this acid in a manner which seemed definitive. However, from researches since made by Berzelius, he has found that citric acid, being considered as composed of $C^8 H^4 O^4$, as was at first admitted, produced salts possessing very extraordinary properties. Citrates of soda and baryta heated to the temperature of 200° C. lose water which they do not contain. The acid therefore appears to be decomposed. However if water is added to these salts, the ordinary citric acid reappears with all its properties. This mobility of the elements of citric acid has engaged the attention of chemists. We have found that by using certain precautions, the same quantity of water may be driven off from many other citrates as well as from the citrates of soda and baryta. It must be therefore admitted that this water does not really form a part of the constitution of citric acid. This point established, another difficulty remains to be solved, which is, that, in both Berzelius's experiments and in ours, each equivalent of citric acid lost one third of an equivalent of water only, and no more. This difficulty could not be overcome in the former opinions entertained of the nature of acids, but in supposing the equivalents of citric acid to be tripled, so that in the neutral citrates there would be three equivalents of base, which may be represented as follows:

$C^{24} H^{10} O^{11}$, real acid.
$C^{24} H^{10} O^{11}$, $3\,H^2 O$ dry acid.
$C^{24} H^{10} O^{11}$, $3\,H^2 O$, $2\,H^2 O$ crystallized acid.

$$C^{24} H^{10} O^{11}, \left.\begin{array}{l} 3\,NaO \\ 3\,BaO \\ 3\,AgO \end{array}\right\} \text{real citrates.}$$

Tartaric Acid.—The admitted formula for this acid cannot be made to agree with the results obtained. According to Berzelius this acid is represented by $C^8 H^4 O^5$. M. Dumas says this analysis is not doubtful in itself; but we have reasons for thinking that tartaric acid is capable, like citric acid, of losing water, formed at the expense of its elements. To verify this, we have submitted tartar emetic to a number of analyses, and are convinced that it loses two equivalents of water, which it does not contain. Therefore every equivalent of acid entering into the composition of tartar emetic loses one equivalent of water. Instead of representing dry tartar emetic .... by $C^{16}, H^8, O^{10}, KO, Sb^2 O^3$
it must be represented by $C^{16}, H^4, O^8, KO, Sb^2 O^3, 2\,H^2 O$.

These two equivalents are driven off at 220° Centigrade, and are independent of the water of crystallization of the tartar emetic.

Other Acids.—Meconic and cyanuric acid present analogous phænomena. M. Dumas observes that there are a class of phænomena which have a tendency to become general, and which seem to follow a law which may be expressed as follows:

In citric, tartaric, meconic, and cyanuric acids, each equivalent of oxygen belonging to the bases to which they are united can displace and replace an equivalent of oxygen, which disappears in the state of water. These acids therefore do not form salts with excess of base, but salts of the same order as ordinary phosphates.

These remarkable phænomena may be looked upon in a more simple and general manner by considering these acids as hydracids of a new kind. Tartaric acid, for example, being considered as it has hitherto been, will give the following formulas:

$C^8 H^4 O^5$, real acid.

$C^8 H^4 O^5, H^2 O$ hydrated acid.

$C^8 H^4 O^5, K O$ neutral tartrate of potash.

$C^8 H^4 O^5, K O + C^8 H^4 O^5, H^2 O$ cream of tartar.

$2 C^8 H^4 O^5 + K O + Sb^2 O^3$ tartar emetic.

These complicated formulas become very simple when represented in the following manner:

$C^{16} H^4 O^{12} H^8$ hydrated acid.

$C^{16} H^4 O^{12} \left\{ \begin{matrix} K^2 \\ H^4 \end{matrix} \right\}$ neutral tartrate of potash.

$C^{16} H^4 O^{12} \left\{ \begin{matrix} K \\ H^6 \end{matrix} \right\}$ cream of tartar.

$C^{16} H^4 O^{12} \left\{ \begin{matrix} K \\ Sb^2 \end{matrix} \right\}$ anhydrous tartar emetic.

It may therefore be observed that dry tartaric acid does not exist, and that a radical $C^{16} H^4 O^{12}$ must be admitted, which with H^8 would constitute an hydracid of a new kind.

If this is admitted, all the combinations of the radical tartrate will be represented by saying,

That in all these combinations hydrogen is replaced either altogether or in part by its metallic equivalents, as it is presented in all its analogous substitutions. We could show without difficulty that the constitution of the citric, meconic, and cyanuric acids is subject to the same transformations, and that they could be also represented as hydracids.

In our memoir is contained an experimental discussion under this new point of view, which will give the opinions of M. Dulong concerning oxalic acid an unexpected extension.—*L'Institut*, Jan. 1838.

METEOROLOGICAL OBSERVATIONS FOR FEBRUARY 1838.

Penzance.—The greater part of this month has been extremely boisterous and rainy; scarcely a day has elapsed without a storm, which has generally been of short continuance, except in two instances, when it continued—in the former, on the 14th, 15th and 16th inst., with unabated fury for upwards of 40 hours, accompanied with an astonishing quantity of rain, amounting to two and a half inches. The wind during this period blew from SSE. to SE. The mercury was not so low in this instance as it was at the last great storm, on the 24th inst., its minimum at this time was 29 inches, but then it fell to 28 inches, a depression seldom experienced in this climate. The wind on this was certainly more tempestuous than on the former occasion, but not of so long duration; it blew from the SW.—A larger quantity of rain has fallen this month than in an equal space of time for many preceding years.—The former part of the month was cold, the thermometer showing 28°; the middle and latter parts of it have been very mild.

February 1838.

Meteorological Register by Mr. Hocking at Penzance.

Days of Month.	Barometer.		Therm.		Wind.	Rain.	Weather.
	Max.	Min.	Max.	Min.			
1	30·060	29·901	43	34	NE.	...	Cloudy, fair.
2	30·228	30·195	38	34	ENE.	...	Cloudy.
3	30·180	30·168	41	29	SSE.	...	Cloudy, clear frost.
4	30·200	30·154	34	28	E.	...	Fair and clear, frost.
5	30·021	29·876	39	32	SE.	...	Frost, cloudy.
6	29·735	29·387	48	34	SSE.	0·620	Cloudy, rain, stormy.
7	29·260	28·882	50	43	SW.	0·610	Rain, stormy.
8	28·885	28·652	48	45	SW.	0·760	Showers, heavy rain, wind.
9	28·866	28·716	47	35	SW.	0·980	Heavy rain and snow, stormy.
10	29·150	28·736	35	30	E.	...	Fair, cloudy, snow.
11	29·525	29·271	39	30	NE.	0·490	Rain.
12	29·461	29·280	42	30	ENE.	...	Fair.
13	29·383	29·297	39	32	ENE.	...	Fair and clear.
14	29·422	29·135	38	32	SE.	0·440	Rain, heavy gale of wind.
15	29·046	28·985	44	34	SSE.	1·720	Sleet and rain, very stormy.
16	29·198	29·100	49	44	WNW.	0·230	Stormy, showers.
17	29·738	29·476	49	35	E.	0·050	Fair with showers.
18	29·976	29·918	49	40	ESE.	...	Fair and clear.
19	29·780	29·595	46	42	ESE.	...	Fair, cloudy.
20	29·401	29·332	47	44	WSW.	0·410	Misty rain.
21	29·513	29·430	51	42	E.	...	Fair, cloudy.
22	29·380	29·202	47	44	SE.	0·250	Misty rain.
23	29·140	28·615	50	46	SSW.	1·110	Heavy rain and wind.
24	28·356	28·078	50	45	SW.	0·710	Stormy, heavy showers.
25	28·415	28·230	47	44	SE.	0·430	Stormy, showers.
26	28·784	28·614	49	40	SE.	...	Cloudy.
27	28·865	28·832	53	41	SSE.	0·110	Fair, light showers.
28	28·886	28·760	49	42	S.	0·130	Fair, windy.
Average.	29·343	29·145	45	38		9·050	

Chiswick.—Feb. 1. Cold dry haze. 2. Clear and cold. 3—6. Frosty. 7. Thawing: rain at night. 8. Cloudy: rain. 9. Stormy with rain. 10. Frosty: overcast. 11—14. Frosty. 15, 16. Bleak and cold. 17. Snowing. 18. Hazy: thawing. 19. Hazy. 20. Clear and frosty: fine. 21, 22. Hazy. 23. Foggy: rain. 24. Heavy rain. 25. Cloudy, and fine: stormy, with rain, at night. 26. Hazy: stormy and wet. 27. Hazy: rain. 28. Fine: rain at night.—The barometer was very low on the 9th; and a depression so great and continued as that observed towards the end of the month, is of very rare occurrence.

Boston.—Feb. 1. Snow. 2. Cloudy. 3. Cloudy: snow P.M. 4, 5. Fine. 6. Cloudy. 7. Rain. 8. Cloudy. 9. Rain: rain early A.M.: snow P.M. 10—15. Fine. 16. Stormy. 17. Stormy: snow A.M. and P.M. 18, 19. Cloudy. 20. Fine. 21, 22. Cloudy. 23. Cloudy: snow P.M. 24. Cloudy: rain early A.M.: rain P.M. 25. Fine. 26. Stormy: snow P.M. 27. Cloudy: snow P.M. 28. Rain.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; and by* Mr. Veall *at Boston.*

Days of Month. 1838. Feb.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8¼ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	London: Roy. Soc. 9 A.M.	Chisw. 1 P.M.	Boston.	London: Roy. So. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering. Max.	Min.	Max.	Min.								
☽ T. 1.	30·076	30·250	30·122	29·80	32·8	36·0	32·4	34	30	33	N.	NE.	calm	...	...	...	29
F. 2.	30·324	30·454	30·368	30·04	32·3	34·6	31·3	36	18	31	NNW.	N.	calm	...	...	...	28
S. 3.	30·368	30·424	30·416	30·10	30·8	34·4	29·5	36	22	29	NNE.	NE.	calm	...	...	...	27
☉ 4.	30·358	30·396	30·377	30·11	27·3	34·4	26·5	35	22	27	NNW.	NE.	calm	...	...	...	21
M. 5.	30·288	30·323	30·218	30·04	29·7	33·2	26·3	36	25	31·5	NE.	NE.	calm	...	...	...	23
T. 6.	30·040	30·091	29·848	29·82	29·8	35·8	28·2	40	25	31	ENE.	E.	calm	...	...	...	25
W. 7.	29·514	29·572	29·208	29·27	39·3	40·0	29·8	49	39	34	SSE.	S.	NE.	·208	·10	·20	29
T. 8.	29·112	29·147	29·066	28·67	44·2	46·0	38·8	50	38	43·5	SW.	SW.	W.	·033	·14	·04	35
○ F. 9.	28·708	29·071	28·647	28·35	43·7	48·4	41·8	45	24	42	SW. var.	SW.	calm	·100	·30	·16	39
S. 10.	29·166	29·217	29·167	28·86	32·5	46·2	31·6	35	22	28·5	NNE.	NE.	calm	·038	...	·10	30
☉ 11.	29·356	29·617	29·432	29·05	29·3	35·5	27·3	36	16	28	NW.	N.	calm	...	...	...	24
M. 12.	29·596	29·646	29·602	29·35	28·4	35·3	26·0	41	14	25	SW.	N.	calm	...	...	...	26
T. 13.	29·534	29·687	29·591	29·30	26·9	34·5	24·8	35	14	27	W.	NE.	calm	...	...	...	24
W. 14.	29·764	29·792	29·502	29·52	27·9	35·2	26·5	39	24	26	NW.	E.	calm	...	...	...	24
T. 15.	29·638	29·688	29·604	29·46	28·5	36·2	27·0	32	26	32	NE.	E.	calm	...	...	...	25
F. 16.	29·536	29·697	29·578	29·42	29·9	31·6	28·0	34	25	31	NE.	E.	calm	...	...	...	21
☾ S. 17.	29·644	29·821	29·695	29·46	28·6	32·5	26·3	35	26	31	NE.	E.	E.	...	·32	...	25
☉ 18.	30·048	30·250	30·106	29·76	33·2	35·3	28·7	38	30	32	NW.	W.	SE.	·222	...	...	28
M. 19.	30·166	30·222	29·969	29·85	33·5	35·5	32·3	39	23	32	NE.	SE.	calm	...	...	...	29
T. 20.	29·702	29·788	29·681	29·45	31·3	38·2	29·0	41	35	29·5	E.	SE.	calm	...	...	...	25
W. 21.	29·706	29·765	29·735	29·45	35·3	40·4	31·0	37	31	32	NNE.	NE.	calm	...	·03	...	31
T. 22.	29·700	29·753	29·650	29·48	34·2	38·3	32·2	36	31	32	NE.	E.	calm	...	...	...	30
F. 23.	29·478	29·541	29·224	29·30	33·8	37·4	33·6	41	32	33	NE. var	SE.	E.	...	·33	...	32
● S. 24.	28·770	28·861	28·740	28·64	39·8	40·6	33·5	47	40	35	ENE.	SE.	E.	·416	·29	·72	35
☉ 25.	28·754	28·931	28·792	28·52	43·7	46·0	39·7	52	32	38	SW.	S.	E.	·250	·02	·11	36
M. 26.	28·950	29·088	28·968	28·77	38·8	48·7	38·2	40	35	35	NE.	NE.	E.	...	·32	...	37
T. 27.	29·136	29·177	29·143	28·92	37·7	40·4	35·0	45	37	34·5	ENE.	E.	E.	·333	·11	...	35
W. 28.	29·114	29·177	29·129	28·86	42·2	43·4	37·3	53	38	35	S.	SW.	E.	·077	·26	·29	36
Mean.	29·591	29·694	29·555	29·34	33·8	38·4	31·2	38·71	27·64	32·				1·677 Sum.	2·22	1·62	28·9 Mean.

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LXII. *Remarks on "A singular Case of the Equilibrium of incompressible Fluids; by* M. Ostrogradsky;" *(translated in the "Scientific Memoirs," Vol. I. Part IV. from Mémoires de l'Académie Impériale des Sciences de St. Petersbourg, Vol. III. Part III.) By the Rev.* J. H. Pratt, *M.A., Fellow of Caius College, Cambridge.*[*]

IN the *Mémoire* here referred to M. Ostrogradsky deduces the ordinary equation of fluid pressure, $dp = \varrho\,(X\,dx + Y\,dy + Z\,dz)$, in an original manner, and then proceeds to apply it to a case which he imagines the established theory of fluids is not sufficient to explain. The object of the present communication is to endeavour to show that this alleged insufficiency does not exist.

M. Ostrogradsky conceives the case of an incompressible and homogeneous fluid, the surface of which is entirely free and suffers no external pressure: the fluid forms a spherical shell of any given thickness, and each of its molecules is attracted towards the centre by a force, which varies as a function of the distance between the molecule and the centre; in which case equilibrium will necessarily subsist. And the author attempts to show, that the *theory* of fluids would lead us to suppose, that the resultant of X, Y, Z for points near the interior surface will act *towards* the interior of the fluid, while, in matter of *fact*, the resultant always acts towards the centre of the spherical surfaces, and therefore *from* the interior of the fluid.

The fallacy seems to lie in this: *It is assumed, that, when a fluid is in equilibrium, the pressure at the surface always*

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[*] Communicated by the Author.

equals zero. From this it follows, that near the surface $d\,p$ is positive; therefore $X\,d\,x + Y\,d\,y + Z\,d\,z > 0$, which requires that the resultant of X, Y, Z should be directed towards the interior of the fluid mass.

But the pressure at the surface of a fluid in equilibrium does *not* always equal nothing. For what do we mean by *the surface*, when we speak of pressure? Not the geometrical surface, but the external layer of particles. The term *pressure* implies the contact of two molecules; and the equation of fluid pressure at any point is deduced upon the supposition, that at the point two molecules are in contact. Therefore unless we can show that the pressure upon the interior surface of the external layer is an infinitely small quantity, we cannot say that the pressure at the surface is equal nothing. Because there is no pressure at the geometrical surface, (for the *conception* of pressure has no place there,) we must not say that the pressure at the surface equals nothing; for this would imply that the analytical expression for the pressure at any point in the fluid becomes zero at the surface, which is not always the case, as we shall now show.

In the cases of ordinary stable fluid equilibrium (for example the *external* surface in the instance advanced by M. Ostrogradsky), the pressure decreases as we approach the geometrical surface; and the pressure on the internal surface of the external layer is an indefinitely small quantity, and is smaller the less the thickness of the external layer. Hence, in these cases, it *happens*, that the function expressing the value of the pressure *does* become zero at the *geometrical* surface.

But in cases of unstable equilibrium of the nature of that of the *internal* surface in M. Ostrogradsky's example, the pressure increases as we approach the geometrical surface, and is greater the less the thickness of the outward layer; but this layer must always have some thickness, for immediately we reach the geometrical surface, the conception of pressure vanishes, and the equation of fluid pressure has no existence. If we could discover a function which would represent both the magnitude of the pressure at any point of the fluid, and the fact that there is no pressure at the geometrical surface, this function would be *discontinuous* at the geometrical surface.

This seems to be the explanation of the difficulty. In ordinary cases of stable equilibrium (as has been shown) there is no actual necessity for these distinctions, though they may be very *useful*, even in those instances, to clear up our views.

March 19, 1838.

LXIII. *On the Dimorphism of the Chromate of Lead.* *By* JAMES F. W. JOHNSTON, *A.M., F.R.SS. L. & E., F.G.S., Professor of Chemistry, University of Durham.*[*]

BESIDES those substances which, like the biniodide of mercury and the carbonates of lime and of lead, are known to be dimorphous as *individuals*, there are other bodies, simple and compound, known to be dimorphous as *groups*, though the individual members of these groups have not yet been observed to assume more than one form. The analogous compounds of the tungstic, molybdic, and chromic acids, present groups of this kind. The tungstate and molybdate of lead and the tungstate of lime occur in square prisms, while the chromate of lead has hitherto been observed only in oblique rhombic prisms; the general formula for all of them being $\dot{R}\,\dddot{R}$.

In my report on the present state of our knowledge in regard to dimorphous bodies presented to the last meeting of the British Association, and which will appear in the ensuing volume of their Transactions, I had already attributed this difference of form exhibited by the several members of the above, and many other analogous groups, to the existence of a true dimorphism; and while I considered that such groups, being chemically analogous, might also be considered as crystallographically dimorphous or heteromorphous, I expressed my conviction that further observation would prove the several members of these groups to be also heteromorphous, each individual assuming the forms already observed in any of the others. I am now enabled to confirm these views by a very interesting example.

On a late visit to the cabinet of my friend Mr. Brooke, whose skill as a crystallographer is so highly and so deservedly estimated, he showed me a small specimen of what he called *molybdate of lead* (crystallographically so) *with the colour of the chromate.* Of this specimen he has since kindly favoured me with a few minute fragments, in all not exceeding the fifth of a grain, but sufficient to enable me to determine that the beautiful red crystals were not molybdate having the colour of chromate of lead, but *chromate in the form of the molybdate.* Fused with borax the mineral gave in both flames a beautiful green bead; with microcosmic salt a bead which at a high temperature was nearly colourless, as it cooled became reddish brown, and when solid was of a beautiful green. A larger addition of the mineral rendered the glass opake, but did not blacken it. It dissolved without residue in nitric and muriatic acids, giving with the latter a greenish solution, and on evaporation chloride of lead mixed with a green substance (chloride of chromium?).

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[*] Communicated by the Author.

From these characters there can be no doubt that the substance is chromate of lead.

The specimens on which these minute red crystals occur is from the Bannat. None of the crystals appear to exceed the sixteenth of an inch in length, and they rest on a thin yellow coating which resembles molybdate of lead. Besides the small specimens he possesses Mr. Brooke informs me he has never seen but one other. He has measured two of the crystals, and satisfied himself that in form they are identical with the molybdate.

We are justified therefore in including the chromate of lead among known dimorphous bodies, and in more confidently anticipating that the analogous tungstates and molybdates will prove so also. A strict examination of the specimens already existing in cabinets may be expected to fill up several of the gaps. In the following table:

	Oblique Rh. Prism.	Square Prism.
Chromate of lead.	Common form.	Rarer form from Bannat.
Tungstate of lead.	unknown.	Common form.
Tungstate of lime.	do.	do.
*Molybdate of lead.	do.	do.

There is another member of this group, the tungstate of iron and manganese (wolfram) which, though represented by the formula $\dot{\mathrm{Fe}}\,\dddot{\mathrm{Tu}} + \dot{\mathrm{Mn}}\,\dddot{\mathrm{Tu}}$, in which the bases are isomorphous, yet crystallizes in a form different from either of those above mentioned. The two oblique rhombic prisms have

in wolfram .... M, M = 101·5 P, M = 110° 50′
in chromate of lead = 93° 30′ = 99° 10′

We have here therefore a third form in which the members of this group may possibly crystallize. As wolfram however is a double salt, it is equally possible that this third form may result from the union of the other two. A square prism of tungstate of iron, with a less oblique prism of tungstate of manganese, may be capable of producing the more oblique prism of wolfram, or the union of the two salts in some other way may produce the third form, without rendering it absolutely necessary at present to have recourse to a trimorphism. In a former paper on the dimorphism of baryto-calcite †, I

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* I need not draw attention to the link which this new fact affords for connecting the molybdic with the sulphuric and other analogous acids.

† Lond. and Edin. Phil. Mag., vol. vi. p. 1.

suggested a similar solution for a difficulty of a similar but less striking character. At the same time it should be observed that there is nothing in the idea itself of a body assuming three or more incompatible forms which should induce us to reject it. It is the absence of *direct* proof of the fact which alone makes it prudent, in the present state of our knowledge, to endeavour to explain away appearances such as that presented by the group we are considering.

We should however expect to find wolfram in the two other forms, even if its own special form do not belong to the group of simple tungstates, chromates, and molybdates. It has indeed been met with frequently both at Huel Maudlin in Cornwall, and Schönfeld in Saxony, in square prisms, or octohedrons; but these have generally been considered pseudomorphous,—as mere casts of former crystals of tungstate of lime. I have not seen any of these crystals, and cannot therefore judge of the evidence on which this opinion rests; but as there appears no reason why wolfram should not in favourable circumstances assume the form of the square prism, it is not unworthy the attention of mineralogists to examine how far these supposed pseudomorphous crystals are really and *always* so.

Durham, March 1838.

Note.—Since the above paper was written I have seen some of the octohedrons of wolfram from Huel Maudlin, and externally many of them are perfect; internally, however, they are often more or less hollow, exhibit no cleavage, but a structure radiating from the surface inwards, while the interior of the hollows is often studded with minute brilliant terminal facets. These characters appear to justify the conclusion that they are pseudomorphous. Their interior cavity would seem to imply that they are also epigene, and that, as in the case of some of the Chessy malachites, the change has commenced on the exterior of the original crystal.

LXIV. *On the Composition of certain Mineral Substances of Organic Origin.* By JAMES F. W. JOHNSTON, *A.M., F.R.SS. Lond. and Ed., F.G.S., Professor of Chemistry and Mineralogy, Durham.*[*]

III. *Ozocerite from Urpeth Colliery, near Newcastle-upon-Tyne.*

THE attention of chemists and mineralogists has for several years past been drawn to a species of fossil wax found in

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[*] Communicated by the Author.

Moldavia*, in sufficient quantity to be employed for œconomical purposes, and to which the name of Ozocerite has been given. This substance is of a brown colour, of various shades, has the consistence and translucency of wax, a weak bituminous odour, sometimes a foliated structure and conchoidal fracture, and can be reduced to powder in a mortar. In burning, it emits considerable light, and is said to be used for the manufacture of a species of candles.

The chemical and physical properties of this substance were first examined by Magnus (*Ann. de Chem. et de Phys.*, lv., p. 218); more lately by Schrötter (*Bibliothèque Univ.*, May, 1836); and most recently by Malaguti (*Ann. de Chem.*, lxiii. p. 390); who agree in representing it as a mixture of several substances, differing in their physical properties, yet possessing the same ultimate chemical constitution.

The occurrence of a fossil body, possessing many of the characters of Hatchetine, and having much resemblance to the fossil wax of Moldavia, in a coal mine in this neighbourhood, where no doubt could exist as to its origin, has afforded me an opportunity of adding to our knowledge of this class of mineral compounds, while it seems to indicate pretty clearly their common organic origin wherever they may occur.

In driving through a *trouble* in Urpeth Colliery, at a depth of about 60 fathoms from the surface, this substance was found in cavities near the sides of the trouble, and sometimes in the solid sandstone rock; it occurred in considerable quantity, and was sufficiently soft to be made up into balls by the workmen.

The specimen sent to me by my friend Mr. Hutton, of Newcastle, is soft, unctuous, sticking to the fingers, and giving a greasy brown stain to paper; semi-transparent; by transmitted light, of a brownish yellow colour; by reflected light, yellowish green and opalescent; having a slight fatty odour, more perceptible when the substance is melted. It fuses at 140° Fahr., attains its greatest fluidity at about 160°, and begins to boil at 250°. It distils without apparent decomposition, the colourless oil which passes over concreting as it cools into a colourless unctuous mass. As it distils, however, the boiling point of the residue rises very considerably, and it becomes darker coloured. Boiled in a retort with water, it is also volatilized in small quantity, and floats like wax on the water which collects in the receiver. Heated over a lamp in a platinum spoon, it takes fire, and burns with a pale blue, surmounted by a white flame, having little smoke, and leaving no residue.

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* It is found, according to Dr. Meyer, at the foot of the Carpathians near Slanik, beneath a bed of bituminous slate clay, in masses sometimes from 80 to 100 pounds weight. Not far from the locality are several layers of brown amber. It is associated with the gres bigarré, with rock salt, and with beds of coal.

It undergoes no apparent change when boiled in concentrated nitric, muriatic, or sulphuric acid. Alcohol, even absolute and boiling, dissolves it very sparingly. The solution is rendered milky by water; and by spontaneous evaporation, deposits the dissolved portion in white flocks. Æther, in the cold, dissolves about four-fifths of the whole, giving a solution which, like the substance itself, is brown by transmitted light, and by reflected light exhibits the greenish opalescence, observable in the ozocerite of Moldavia. The solution, by spontaneous evaporation, deposits the dissolved portion in brown flocks, which, at 102° Fahr., melt into a yellow brown liquid. The mass, on cooling, presents the external characters of the original substance, but has less consistence and density. Its specific gravity is 0·885, and it melts at 102° Fahr. A further small portion of the brown undissolved matter is taken up by boiling æther and alcohol. Obtained by evaporation from these solutions, this second portion is colourless, or of a pale yellow; has the appearance and consistency of wax, and melts at 136° Fahr.,—about 16 degrees lower than the fusing point of bees'-wax. The remaining portion, which is almost insoluble in boiling alcohol and æther, has a dark brown colour, and the consistence of soft wax; its density is 0·965; it melts at 163° Fahr., and boils at a temperature above 500° Fahr. The vapour has a peculiar and slightly bituminous odour. It constitutes about one-sixth of the mineral mass.

As it occurs in nature, therefore, this substance contains at least three several compounds, agreeing in their indifference to acids, but differing in physical properties and in their relations, especially to æther. The following table exhibits a comparative view of the properties of the mixed mineral,—of its three constituent parts—of the specimens of fossil wax from Moldavia, examined by Schrötter and Malaguti—and of the substance obtained from it by the latter on distillation.

	How obtained, or where.	Colour.	Consistence.	Density.
I. Ozocerite (Schrötter).	Found native in Moldavia.	Brown.	Hard, brittle.	0·953 at 15° C.
II. Ditto A. (Malaguti).	Ditto.	Ditto.	Ditto.	0·946 at 20·50
B.	By distilling A.	0.	Of wax.	0·904 at 17°C.
III. Do. from Urpeth. A.	Urpeth Colliery.	Brown.	Of tallow.	
B.	From A, by cold æther.	Ditto.	Ditto.	0·885
C.	From residue, by boiling æther.	Yellow.	Of soft wax.	?
D.	Residue, after boiling A in æther.	Dark brown.	Of wax.	0·955

The fossil wax examined by Magnus, seems to have been identical with that of Malaguti, only it melted at 82° C.

The inspection of this table shows that these mineral products contain at least four substances, possessed of different properties, chemical and physical, of which three are present in that from Urpeth Colliery.

1. One charred by sulphuric acid and insoluble in æther. —(*II. Malaguti.*)
2. One soluble in cold æther.—(*I. and II. B.*)
3. One soluble in boiling æther, and sparingly in boiling alcohol.—(*II. B. III. C.*)
4. A residual portion of greater density scarcely acted on by either of these menstrua.—(*III. D.*)

The different substances composing the ozocerite appear as I have already stated to be identical in chemical constitution, being entirely composed of carbon and hydrogen, in the same proportions as in olefiant gas. That the substance from Urpeth Colliery contains no oxygen, is proved by its not affecting the lustre of potassium, when melted along with it. The carbon and hydrogen were ascertained by burning with oxide of copper.

1. 8·43 grs. of the crude mass, freed by fusion from adhering earthy matter, gave 10·69 grs. of water, or 1·187 grs. of hydrogen.
2. 5·47 grs. of the matter taken up by æther, gave 6·92 grs. of water, or 0·77 grs. of hydrogen.
3. 5·84 grs. of the same gave 7·39 grs. of water and 18·32 grs. of carbonic acid.
4. 5·47 grs. of the same gave 6·72 grs. of water and 16·58 grs. of carbonic acid.

Melts at	Boils at	In Æther.	Action of hot sulphuric acid.
62°C. = 141·6° F.	210° C. = 410° F.	Dissolves.	?
84°C. = 182° F.	300° C. = 572° F.	Almost insoluble.	Chars a portion of it.
56° to 57° C. = 133° to 134° F.	300° C. = 572° F.	In boiling æther, very soluble.	?
60° C. = 140° F.	121° C. = 250° F.	Largely soluble.	0
39° C. = 102° F.	?	Wholly soluble.	0
58° C. = 136° F.	?	Soluble in boiling æther.	0
73° C. = 163° F.	Above 260° C. = 500° F.	Very sparingly soluble in boiling æther.	0

These results give for the crude mixed mineral, and for the portion soluble in æther, the same composition.

	1.	2.	3.	4.
Hydrogen	14·09	14·07	14·06	13·649
Carbon...	85·81	85·83	86·80	83·812
	100	100	100·86	97·461

The ratio of the elements in the fourth analysis is that of atom to atom; the loss I attribute to the pumping out of a portion of the substance from the tube along with the moisture contained in the oxide of copper, the sand with which the tube was warmed in this experiment having been too hot for a substance boiling so low as 250° Fahr.

The small portion of matter at my disposal prevented me from subjecting to analysis either of the other compounds contained in the crude mass; the composition of this mass, however, as exhibited in No. I., shows that these also must contain the elements in the same proportion as the matter actually analysed.

The following table shows also the identity, in chemical constitution, of these several substances, with the different varieties of Ozocerite from Moldavia.

	Atoms.	Equivalents.	Per cent. Calculated.	Ozocerite.			
				Magnus.	Schrötter.	Malaguti.	Fm. Urpeth.
Hydrogen	1	12·479	14·0349	13·15	13·787	13·95	14·06
Carbon...	1	76·437	85·9651	85·75	86·204	86·07	86·80
		88·961	100·	98·86	99·991	100·02	100·86

The elementary composition of these different substances, therefore, is identical, and is the same as that of olefiant gas. The ozocerite found in Urpeth Colliery must have had its origin in the coal strata. Emitted, in the form of vapour, and carried along by the lighter gas (fire damp), given off at the same time, it would pass through the trouble, on its way to the surface, and be partly condensed in the cavities, and other cool places it came in contact with. It is highly probable that the other varieties of fossil wax may have been derived from a similar source.

In considering the inflammable and explosive substances existing in coal mines it is usual to limit the attention solely to the permanent gas given off, without adverting to the possibility of other substances, of a volatile nature, being also emitted in the state of vapour. The occurrence of this variety of Ozocerite, in Urpeth Colliery, shows us that the light carburetted hydrogen sometimes carries along with it other volatile substances, and there is strong reason for believing that the combustible portion of the atmosphere of our coal mines rarely, if ever, consists wholly of this light gas. To show the Proteus-like character of the compounds of carbon and hydrogen, in the ratio of atom to atom, and how little chemical analysis can avail directly in determining the total absence of these substances, I subjoin a table, exhibiting the characteristic properties of the numerous bodies we are already acquainted with, in which the elements exist in this proportion.

	How obtained, or where.	State at 60 degrees.	Density.	Becomes solid or liquid at	Boils at	Density of gas or vapour.
Sweet oil of wine.	In preparing æther.	Oily liquid.	0·917	Solid at 31° F.	536° F.	?
Solid oil of wine.	Ditto.	Prisms.	0·980	Liq. at 230°	500+	?
Solid oil of roses.	In oil of roses.	Crystalline plates.	?	Do. at 95°	536° to 572°	?
Paraffine.	From wood, coal, and animal tars.	Ditto.	0·87	1 ° to 111°	?	?
Naphtha.	From natural wells, and from coal tar.	Liquid.	0·75 to 0·78	?	176° to 212°	2·833
Methylene.	Exists in wood spirit.	Gas.	0·4903	?	?	0·4903
Olefiant gas.	By heating alcohol with twice its bulk of sulphuric acid.	Gas.	0·9806	?	?	0·9806
Faraday's light liq.	By compres^{s} oil gas.	Ditto.	1·9612	Liq. at 0°	Below 32°	1·9612
Cetene.	Distilling æthal with phosphoric acid.	Oily fluid.	?	?	527°	7·844
Elaene.	Distilling metaoleic and hydroelaic acids.	Ditto.		?	230°	4·488
Oleene.		Ditto.		?	131°	2·875 to 3·02
Hatchetine.	Found native.	Solid	0·916	Liq. at 115°	?	?
Ozocerite.	Ditto.	Ditto.	0·885 to 0·955	Liq. from 102° to 182°	250° to 572°	?
Caoutchene.	Distill^{g} caoutchouc.	Liquid.	0·65 at	Liq. at 14°	58·2	?
Heveéne.	Ditto, or from caoutchouc by sulp. acid.	Dense do.	0·921 at	?	579°	?

A glance at the second column of this table shows that several of these substances are obtained from the products of the distillation of coal; and though it has not been *demonstrated* that any of them actually exist ready formed in the mass of the coal itself, yet the very low temperature at which some of them are given off lends to this opinion a considerable degree of probability. Reichenbach states that bituminous coal, by distillation with water, yields 1·320,000th of an æthereal oil, which is identical with native naphtha; and he concludes that the naphtha and petroleum springs of Persia, India, Italy, and South America, have their origin in the slow distillation of large beds of coal, by the ordinary heat of the earth. The fossil wax of Moldavia, and the hatchetine of England, are probably derived from vegetable matter by a like agency.

Naphtha is a comparatively dense fluid, requiring a temperature of upwards of 173° Fahr. to boil it; and, therefore, unless present in large quantity, it will rarely escape from the coal so rapidly, as alone to render the atmosphere combustible; but, suppose the very light liquid discovered in oil gas to exist in the coal, it will at once escape as a highly inflammable gas, and materially injure the atmosphere. Because such substances have not hitherto been observed in the air of mines, we ought not hastily to conclude that they do not exist, ready formed, in the great laboratory of nature. The difficulty of detecting them in a limited portion of gaseous matter will, probably, long present insuperable obstacles to the analytical chemist, while the more we learn of the carbohydrogens the more likely it appears that several of them should be occasionally present in the air which circulates through mines of bituminous coal.

The common fire damp requires, for its perfect combustion, ten times its bulk, the vapour of Faraday's light liquid thirty times, and that of naphtha forty-five times its bulk of common air. A very small portion of either of the latter, therefore, would render an atmosphere dangerous. The sudden outburst of a small reservoir would pollute a working previously considered safe, and give rise to an explosion where none was considered possible. In a district of country like the north of England, where rich bituminous coal is so abundant, where mines are worked at the very verge of the inflammable state, and where the most serious accidents from explosions occasionally occur, it is of importance, I think, that the probable presence of such substances, in the state of vapour, should be taken into account. Where the coal is richer than usual, and where troubles occur in which these compounds, as at Urpeth, may exist in a liquid or solid state, the rapid escape of combustible matter may be anticipated; while the probability of such escape affords a rational explanation of those sudden and unexpected emissions of gaseous matter which have occasionally been followed by consequences so disastrous*.

An observation familiar to practical men in the English coal fields leads to the same conclusion. In mines where candles or open lamps are used, it is by the appearance of the flame that the miner judges of the purity of the atmosphere, and the presence of combustible matter. When little inflammable gas is mixed with the air, the flame carries over it a very short pale blue *head*, which increases in length as the quantity of the carbo-hydrogen increases, until the whole atmosphere becomes one explosive mixture. But in different coal fields, the length of *head*, as it is called, which indicates an approach to the explosive state, is very different. In the Newcastle and Leeds coal fields 1½ inches indicate danger; in S. Wales 4 or 5 in. are not unusual. The colour of the head is also a criterion by which the miner judges; when blue, combustible matter is present, and an explosion is to be feared; if brown and muddy, carbonic acid is suspected, and the danger is less.

Though no *particular* conclusions can be drawn from these observations, yet the general result does force itself upon us, that various compounds of carbon are at different times present in the atmosphere of coal mines and in various quantities; and that sudden explosions may often be caused by the escape from cavities in the coal strata of other compounds than that usually called the fire damp, and to which all the mischief is usually attributed.

Durham, March 1838.

Note.—I have just seen in the possession of Prof. Graham, of University College, a candle formed of a substance said to be found in considerable quantity in the coal mines near Linlithgow in Scotland. It resembles in every respect the Ozocerite candles of Moldavia. The substance is dull brown, and after fusion almost black, reflected and reddish brown by transmitted light; mass opake but translucent at the edges and in thin layers; is greasy to the touch (like Hatchetine), easily scratched by the nail, has a conchoidal fracture, and when cold has no perceptible smell.

I may here mention also that the Middletonite described in a former paper, has since been met with in the mass of the coal in the Newcastle coal-field. May not this substance be the resin of the trees of the carboniferous æra more or less changed?—April 16.

[* Another explanation had previously been given by Mr. Hutton, in following up an idea originally suggested we believe by Dr. Paris: see L. and E. Phil. Mag., vol. ii. p. 303; and Paris's Life of Davy, p. 395. Both are probably true.—Edit.]

LXV. *Sequel to an Essay on the Constitution of the Atmosphere published in the Philosophical Transactions for* 1826; *with some Account of the Sulphurets of Lime. By* JOHN DALTON, *D.C.L., F.R.S. &c.*

[Continued from p. 168, and concluded.]

On the Quantity of Oxygen in the Atmosphere.

SINCE the commencement of the present century it has been ascertained beyond dispute that the chief constituents of the atmosphere, oxygen gas and azotic gas, are in the same proportion in all countries and at all times, except when influenced by local circumstances; namely, 21 per cent. of volume of oxygen, and 79 per cent. of azote, neglecting fractions: other elements are found in the atmosphere, but they are comparatively insignificant in quantity, namely aqueous vapour, carbonic acid, &c. The experiments have generally been made on air collected at the surface of the earth; and it may be remembered that I have endeavoured to prove in various essays that the diffusion of gases one amongst another as well as *in vacuo*, is owing to the repulsive powers peculiar to the particles of each particular gas, otherwise we should never have the feeble efforts of carbonic acid and aqueous vapour diffusing those elements against the immense pressure of the atmosphere. The principle I contend for has, I believe, obtained general assent; but I apprehend few have been aware of the consequences. If we suppose a carbonic acid atmosphere of 15 inches of mercury pressure and a hydrogen atmosphere of the same pressure, together constituting a mixture of the two amounting to 30 inches of pressure, were to surround the earth, I think no one would hazard a conjecture that these two would be found in equal proportions at every elevation in the atmosphere; yet a similar supposition seems prevalent with regard to our present atmosphere of oxygen and azote. It has been an object of investigation with me for many years to find how the fact stands in this respect; that is, whether the oxygen is more abundant relatively in the lower strata of the atmosphere than in the higher, as it ought to be in a stagnant column; or whether the constant agitation of the atmosphere and the predominant mechanical power of the azotic part of it do not prevent that equilibrium which a stagnant mixture of aerial fluids of different specific gravities would effect. From the experiments about to be related, I have reason to believe that the higher regions of the atmosphere are somewhat less abundant in the proportion of oxygen than the lower, though the reverse might be expected from the enormous consumption of oxygen by daily processes on the surface of the earth, when we know of no proportionate consumption of azote. It appears, however, that the disproportion of the two elements at different elevations is by no means so great as theory requires; and therefore we must conclude the unceasing agitation of the atmosphere by currents and counter-currents is sufficient to maintain an almost uniform mixture at the different elevations to which we have access.

The subject is one involving an important principle. I have kept it continually in view for the last forty years, and have made innumerable experiments with a view to its elucidation. As the value of such experiments depends much upon a thorough acquaintance with the nature of the operations and the several sources of error to which they are liable, it may be needful to point out certain particulars, which, as long experience has taught me, require attention in order to secure a due approximation to accuracy. I allude more particularly to the use of Volta's eudiometer as applied to determine the proportions and quantities of oxygen and hydrogen gases.

1. Hydrogen gas procured over water is sure to contain some common air, whether the water has been previously boiled or not; it arises out of the water and may amount to 1 or 2 per cent.; the same observation applies to oxygen gas; the proportion of oxygen and azote is usually that in common air nearly. When a phial of hydrogen gas, by long keeping or by accident, has acquired a portion of common air, and then stood some weeks after, the oxygen seems to diminish, either by slow combustion or by absorption in the water, and so leaves the azote and oxygen in another proportion to that of common air. Before using such hydrogen the oxygen in it should be tested by nitrous gas, and the percentage of hydrogen by oxygen gas. It is best not to rely too much upon hydrogen taken from a bottle half filled with water.

2. Oxygen gas, and others, will show carbonic acid by sending them up through a narrow eudiometer tube filled with lime-water, provided the acid gas amounts to $\frac{1}{2}$ per cent. of the original; but it does not show any carbonic acid in this way in atmospheric air, though the acid is always present to the amount perhaps of $\frac{1}{1000}$th part. The proportion of pure oxygen in any sample containing from 90 to 100 per cent. of that gas, may be found either by hydrogen gas or nitrous gas; and if great accuracy is required, I recommend testing it both ways, as has already been mentioned under the head nitrous gas.

3. The gradual deterioration of oxygen, hydrogen, nitrous gas, common air, &c. when by use the phial became $\frac{1}{4}$, $\frac{1}{2}$, or $\frac{3}{4}$ filled with trough water, is a circumstance by no means to be overlooked. The entrance of water that has been some-

time stagnant in the cistern, though preserved carefully from any material impurities, always affects the remaining air, though the phial be well corked and immersed in a cup of water. The cause is obvious to those acquainted with the laws that regulate the absorption of gases by water. The common air in the water (the quantity of which varies much as to the oxygen part) is continually either making its escape into the incumbent air of the phial, or this last air is entering the water, so that the degree of purity is continually changing in a small degree. This renders it necessary to test the actual state of this gas after it has been some time in the phial, before we recommence the use of it. A phial of air may be pure at first, and only 90 per cent. at its conclusion. I have known samples of common air kept in bottles at first containing 21 per cent. of oxygen, and after some months a small residue was found to contain only 19 per cent.

4. It may not be improper here to relate some unpublished results which I formerly obtained when experimenting on subjects here discussed. In my memoranda for 1816, I find that I took water well boiled (supposed ¼ of an hour or more) and then poured it gently into a Florence flask, filling it up into the narrowest part of the neck, and left it so, exposed to the atmosphere for three days without any agitation. At the end of this, 2700 grains of water imbibed 49 grain measures of atmospheric air by agitation, which is about ⅘ of a full share; hence ⅕ of a full share must have been, both the air that was left in after boiling, and that acquired from the atmosphere in three days by absorption from the small exposed surface.

Water boiled in a kettle for three or four minutes, then suddenly cooled and transferred without agitation into a bottle containing 2700 grains, and then agitated with atmospheric air, imbibed 32 measures, which are about half a charge; whence it may be inferred that water boiled for three or four minutes loses about half of its air.

I boiled a kettle full of water for a quarter of an hour; let it stand a day or two to cool, then transferred it carefully by a siphon into a cylindric jar of 8 inches diameter and 10 inches deep; afterwards drew off daily by a siphon 2700 grain measures from the middle or near the bottom of the jar, and charged it with air to the full by agitation. The bottle of water imbibed

The first day ...	16 measures.
The second day	15 measures.
The third day..	12 measures.
The fourth day	10 measures.
The fifth day ...	10 measures.

The sixth day	9 measures.	
The seventh day	4 measures.	The water taken near the surface.
The eighth day	7 measures.	These portions taken up consisted nearly one half of oxygen.
The ninth day ...	9 measures.	
The tenth day ...	7 measures.	
The fifteenth day	2 or 3 measures.	

From these experiments it would appear that by boiling water briskly for three or four minutes, about half of the atmospheric air previously in the water escapes along with the steam. But it requires much longer boiling and keeping the atmospheric air as much as possible from the surface of the water to get the rest of the air expelled. It is never all expelled by boiling, except in the construction of a good water hammer. Any one air not chemically combined with water is easily and effectually expelled from it by repeatedly agitating the water with another kind of air.

It also appears that water deprived of its atmospheric air, if kept at rest, acquires the air again slowly, and more so if the surface exposed is small. But if violent agitation of the water, so as to mix the atmospheric air and it intimately together, be used, the full impregnation is effected in one or two minutes, as I have elsewhere shown.

Trough waters being mentioned above (3.) it may be well to explain some of the circumstances affecting it. The waters I use for the chemical trough is *rain-water*; it is preferable to pump water by its freedom from carbonic acid and earthy salts; it is slightly coloured at first when drawn from the cistern, but it soon becomes clarified by standing: my trough contains about nine gallons when in work. I take great care to put nothing in it which can materially affect its purity; small portions of lime water and of some iron and other salts are the chief impurities which are admitted; no sulphurets or hydrosulphurets are allowed to enter, and very little of either acids or alkalies. I examine the state of the water occasionally; lately, after it had been more than half a year in the trough, though not very frequently used, I had the curiosity to examine its state before the trough was emptied. The water was neutral by the colour test; it contained about 50 grains of saline matter in the gallon; it was transparent, but slightly milky; prussiate of potash gave sensible blue; oxalate of ammonia, muriate of barytes, and carbonate of soda produced a white precipitate. The taste was like that of earthy pump water. It had its full share of azotic gas, but rather less than half of its share of oxygen gas; that is, it had about 4 or 5 cubic inches of azote in the gallon, and only 1 cubic inch of oxygen.

In the following train of experiments on the oxygen in the atmosphere I have mostly used from 50 to 70 measures of hydrogen for 100 air, unless otherwise mentioned. Possibly this may not be thought the best proportion for securing the complete abstraction of the oxygen. The limits are, 100 air with 42 of hydrogen for the minimum, and 100 air with 170 hydrogen for the maximum. In the former case the hydrogen is barely sufficient for the oxygen; in the latter case the oxygen is barely enough to admit of a complete combustion, being only $\frac{1}{13}$th of the mixture. Perhaps the best proportion would be 100 air to 100 hydrogen to ensure complete combustion, because it is about the mean of the two extremes; but it must be considered that if the hydrogen should contain even a very small portion of oxygen, the whole of it in 100 measures would be included in the atmospheric oxygen, so that in practice it would probably be safest to use a mean between 40 and 100 of hydrogen. I have mostly endeavoured to keep between 50 and 70 of hydrogen for 100 air.

Experiments on the Quantity of Oxygen in Atmospheric Air.

Air from the Summit of Helvellyn*, July 14, 1824.

A phial, containing about half a pint, was filled with water at a clear rivulet on the ascent: this was emptied at the summit and well corked; the cork was drawn at the foot of the mountain in a trough of clear running water, when a quantity of water was found to enter corresponding to the increased pressure of the atmosphere. The phial was then corked and inverted in a cup of water, and the air analysed a week afterwards.

Average of four experiments on this air with hydrogen, about 50 to 100 air, gave	20·70 oxygen per cent.
Average of four experiments of the common air taken in Manchester at the time of the analysis, and with same phial of hydrogen and same proportion, gave............	20·88 oxygen per cent.
Average of seven experiments on Helvellyn air made a day afterwards, gave	20·58 oxygen per cent.
Average of seven experiments, on air from an open place in the town next day with same hydrogen, gave.......................	21·1 oxygen per cent.

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* This mountain, situate at the head of Ullswater, separates Cumberland from Westmoreland; its height above the sea, which lies to the S.W., and from which it is distant about 20 miles, is upwards of 3000 feet; it is surrounded by other mountains, mostly of less elevation.

Average of eight experiments on the country air three miles from Manchester, July 29, with same phial of hydrogen, which now manifested a very slight trace of oxygen, gave .... } 21 oxygen per cent.

1824, November 23.—Barometer 28 inches, very low. Apprehending that this circumstance, attended by rain and a high wind S.E., might have some influence on the proportions of the atmosphere, I made the following experiments.

Average of six experiments gave 20·75 oxygen per cent.

When the remainder of this air had been kept five months in the bottle, it then yielded on an average of three experiment 20·67 oxygen per cent.

1825, January 8.—Barometer 30·94, extremely high, after a week of calm weather. Filled a bottle with air from the town.

Average of four experiments with two parts air and one hydrogen gave 21·12 oxygen per cent.

The remainder of this air, kept till August same year, gave 21·1 oxygen per cent.

June 8.—Average of four experiments from air in the town gave 20·97 oxygen per cent.; barometer 29·90.

June 10.—Air from a field near the town, barometer being 30·30, thermometer 70°, wind S.W.; sunny and sultry. Two parts of the air with one of pure hydrogen being mixed, the average of six experiments gave 20·58 oxygen per cent.

June 14.—Mixed some pure azotic gas with oxygen gas, which was marked 90 per cent. pure, in such proportions as to make a mixture of 21 per cent. oxygen. On trial with hydrogen the mixture gave, first experiment 21 + oxygen per cent.; the second experiment 20·9 oxygen per cent.

November 3.—Air in the town, barometer 28·76, thermometer 46°, rainy, with S.W. wind. Average of ten experiments gave 20·6 oxygen per cent.

Air from the Summit of Snowdon, 3570 feet above the sea, taken by John Blackwall, Esq., May 14, 1826, at 7 p.m.; wind N.E. light, barometer 26·20, thermometer 42°.

May 28.—Average of ten experiments gave 20·65 per cent. oxygen.

Country air three miles from Manchester, analysed the same day, average of six experiments gave 20·8 per cent. oxygen.

Again, Snowdon air in six experiments gave 20·66 oxygen per cent.; but the bottle being now half full of water, I did not examine the rest.

Another bottle of air was taken at the summit on another occasion, May 18, by the same gentleman; wind S.W., light.

May 25.—Analysed; average of six experiments gave 20·59 oxygen per cent.

Country air near Manchester at same time gave average 20·7 per cent.

A second bottle of air from Snowdon, taken at the same time, May 18, gave on an average of four experiments 20·9 oxygen per cent.

Air from the town at the same time, on an average of five experiments, gave 21·04 oxygen per cent.

1826, July, Air from the Summit of Helvellyn.

Average of ten experiments gave 20·63 oxygen per cent.

Average of the town air found at same time was 20·73 oxygen per cent.

Air taken in an Aerial Voyage over Cheshire.

Mr. Grafton was so good as to procure me a bottle of air taken in an aerial voyage over Cheshire with Mr. Green, June 26, 1827; height 9600 feet above the sea*. The air was transferred into two phials.

First Phial.

June 27.—Average of seven experiments of balloon air gave	20·7 oxygen per cent.
Average of seven experiments on town air gave	20·83
July 2.—Average of eight experiments of balloon air gave	20·2†
Average of eight experiments on town air gave	20·8

The second phial of balloon air was carefully preserved, the phial being filled and having a ground stopper. It was analysed.

1828, May 28.—Average of three experiments balloon air gave	20·70 oxygen per cent.
Average of three experiments town air gave	20·80
Aug. 5.—Average of thirteen experiments, being the whole of the balloon air, gave	20·52

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* Height found as under:

Capacity of bottle	10·47 ounces.
On drawing the cork under water there entered	2·77 ounces.
Left	7·7 ounces of air.

Also height of barometer and thermometer below given.

† The whole air in the first phial was spent in these fifteen experiments. The deterioration of the air in the first phial, by being kept half full of trough water for five days, is remarkable.

Average of thirteen experiments on town air, gave } 20·92 oxygen per cent.

On the last-mentioned day I received a bottle of air from the summit of Snowdon through the care and attention of my friend and pupil Mr. John Hall. It was corked and well sealed with wax; when opened under water a due portion of that fluid entered.

The average of the first two experiments gave 20·44 oxygen per cent.

The rest of the air after these two experiments was divided into two portions, and entered into two phials for examination. These were analysed a week or two afterwards.

Average of five experiments with first phial gave 20·25 oxygen per cent.

Average of four experiments, which emptied the first phial, gave 19·98 oxygen per cent.

Average of seven experiments of second phial gave 20·3 oxygen per cent.; and a considerable portion was left.

Average of the town air was during these experiments nearly 21 oxygen per cent.

I am not aware of any cause why this air was so much inferior in oxygen to that on former occasions.

1831, July 4.—Helvellyn air brought down from the summit by me; wind S.W., with rain and fog.

1. July 21.—Mixed two ounce measures of this air with one of hydrogen, so as to make six separate and successive explosions; the hydrogen had $\frac{4}{10}$ths of a grain measure per cent. of oxygen, and this is allowed for in the corrected results. These results on the average gave 20·57 oxygen per cent.; the highest was 20·68, and the lowest was 20·43.

The residues of the six explosions were collected, and found to have 5 per cent. of hydrogen and 1 in 120 of oxygen.

2. Mixed *equal volumes* of this Helvellyn air and the same bottle of hydrogen used above, and fired the mixture in successive portions. The average of six experiments gave 20·8 per cent. of oxygen. No oxygen was found in the residue.

By comparing the results of 1 and 2, it would seem that more oxygen is reduced from common air by firing equal volumes of common air and hydrogen than by firing one volume of common air with half a volume of hydrogen.

August 23.—Mixed 100 measures of town air and 120 of new pure hydrogen; this fired gave 21·5 oxygen per cent.; there was no oxygen in the residue. This would seem to point out $\frac{1}{240}$th of oxygen in the hydrogen, yet nitrous gas scarcely manifested so much.

1832, July 26.—Mr. Green, jun., and Mr. John Taylor of the Manchester gas works, ascended in a balloon from Manchester after 6 p.m., a fine, clear, calm evening, barometer being 30 inches, thermometer 65°; the balloon took a south direction, and landed in Cheshire about fourteen miles off. Mr. Taylor took a bottle of air when at the highest elevation, when the barometer stood at 16·8 inches, thermometer 55°; whence the altitude must have been about 15,000 feet.

Capacity of the bottle = 2406 grains of water.

On opening it under water in temp. 64° there entered 884 grains of water.

The air was soon after its reception on the 27th transferred into two small phials for examination.

The first phial was mixed with 60 per cent. of hydrogen, and fired in five portions; it yielded 20·59 oxygen per cent.

The second phial, mixed in like proportion, gave 20·65 oxygen per cent.

Air from the town the next day, fired with the same phial of hydrogen as the preceding, gave 20·95 on the average of five experiments.

Air from Switzerland, &c.

In the autumn of 1835 I was favoured with three samples of air taken in elevated situations in Switzerland by my friend W. D. Crewdson, jun. Esq., of Kendal. Each of these was taken in a two-ounce phial by pouring out the contained water and corking the phial immediately, leaving only a drop or two of water within. The cork was then well closed with sealing-wax. No. 1 was taken on the Mer de Glace, August 21, estimated at the height of 6000 feet above the sea; the second on the pass of the Simplon, August 29, at the height of 6174 feet above the sea; and the third on the Wengern Alp on the 15th September, at the height of 6230 feet. These airs were analysed in October with the following results.

		Oxygen per cent.
Mer de Glace.—	Average of four first experiments	20·2
	Average of four last experiments	19·4
Simplon.—	Average of four first experiments	19·98
	Average of four last experiments	19·53
Wengern Alp.—	Average of four first experiments	20·45
	Average of four last experiments	20·11

It may not be amiss to subjoin a few experiments on air in close chambers, where a number of people have been congregated for two hours, the air being taken at the moment of breaking up.

1802, March 6.—Got a 20-ounce phial filled at the close of a congregation of 500 people assembled for two hours with 50 candles burning; the air completely neutralized 150 grains of lime water, but took very little more; this accords nearly with 1 per cent. of carbonic acid gas. The oxygen was not examined.

1824, November 28.—Examined the air at the close of an ordinary congregation, perhaps 200 people, retained for two hours.

Average of five experiments gave the oxygen 20·42 per cent.

1826, March 16.—Examined the air from a crowded congregation after two hours' confinement, but some doors open.

Average of four experiments gave the oxygen 20·23 per cent.

There was a very slight appearance of carbonic acid each time a charge was passed up through lime water, a phænomenon never observed in ordinary atmospheric air.

The general conclusions, it seems to me, to be drawn from these experiments are, that the proportion of oxygen to azote in the atmosphere on the surface of the earth is not precisely the same at all places and times; and that in elevated regions the proportion of oxygen to azote is somewhat less than at the surface of the earth, but not nearly so much so as the theory of mixed gases would require; and that the reason for this last must be found in the incessant agitation in the atmosphere from winds and other causes.

June 6, 1837.

LXVI. *Note on an apparent Case of Isomorphous Substitution.* *By* H. J. Brooke, *Esq., F.R.S.*

To Richard Phillips, Esq., F.R.S.

My dear Sir,

I SOME time since pointed out the very near identity of the forms, cleavage, and angular measurements of zoizite and euclase, but I omitted to refer to their apparently analogous chemical composition.

	Silica.	Alumina.	Lime.	Glucina.	Protoxide.
Zoizite (Klaproth)	43,	29,	21,	——	3
Euclase (Berzelius)	43·22,	30·56,	——	21·78	2·22

This is apparently a case of isomorphous substitution, but on pointing it out lately to a chemical friend he observed, that the proportion of glucina in euclase was 2 *atoms*, while the lime in zoizite was only 1 *atom*, and that the identity of form must therefore be accidental; but if this be so, may not all other cases of isomorphous substitution (I do not allude to plesiomorphous binary compounds) be equally accidental, or rather are not all these governed by some other at present unknown law? Can you throw any light upon this difficult subject.

Yours truly,

H. J. Brooke.

LXVII. *Observations on Isomorphism, in reference to the preceding Communication by* Mr. Brooke. *By* R. Phillips, *F.R.S. L. & Ed.*

On comparing the analyses of zoizite and euclase quoted in Mr. Brooke's letter, it is evident that substituting glucina for lime, these minerals may be considered as similar; but whether this be a case of isomorphous substitution I now propose to inquire.

Professor Johnston (Report of British Association, vol. i. p. 423) states that "binary compounds which replace each other contain not only the same absolute number of atoms, but also the atoms of the two elements in the same relative proportion." According to Berzelius the equivalent of lime is 28·53, and the (double) equivalent of glucina is 77·13; then 28·53 : 21 :: 77·13 : 56·77, the quantity of glucina which on this supposition should replace 21 of lime: if we take the single equivalent of glucina the quantity of it contained in euclase would, of course, be 28·38; but the actual quantity is only 21·78, or about three fourths of what it should be, on the supposition most favourable to the doctrine of isomorphism.

But another difficulty presents itself in considering lime and glucina as isomorphous; Prof. Johnston observes, in the paper above quoted, "if in peroxide of iron the iron is to the oxygen in the atomic ratio of two to three, the atoms of aluminum and oxygen must in alumina have the same ratio, or both bases must be sesquioxides." Similar reasoning will, I presume, apply to other oxides which are supposed to be isomorphous; now, according to Berzelius, lime is a protoxide, while glucina is a sesquioxide; these substances, therefore, cannot be isomorphous.

There is however every reason to believe that this, and indeed any difficulty which is presented to the doctrine of isomorphism, will be readily overcome, when we observe the liberties which the expounders of it take with what appear to be the best established facts.

In the Records of Science (vol. iii. p. 433) there is a paper by Professor Clark, in which, on account of "a difficulty in isomorphism," he has proposed to double the atomic weights of certain substances. Addressing Professor Mitscherlich, Professor Clark states:

" Now the discrepancy I have alluded to is this:

The waterless sulphate of soda . . $\dot{Na}\dddot{S}$

and of course, the isomorphous salts $\dot{Ag}\dddot{S}$, $\dot{Na}\dddot{Se}$, $\dot{Ag}\dddot{Se}$, you found to be of like form,

Not with manganate of barytes . . . $\dot{Ba}\dddot{Mn}$

But with oxymanganate of barytes . . $\dot{Ba}\dddot{Mn}\ddddot{Mn}$.

" This discrepancy, which, occurring *in such a case*, appeared to me very startling on the first perusal of your paper, I propose to show, may be removed, by regarding the salts in question, as we may reasonably do, notwithstanding our preconceived notions to the contrary, to be as much alike in constitution as you have proved them to be in form."

After some further observations Professor Clark proceeds: " While abiding by this your doctrine, and proceeding on a like principle to what has just now been illustrated in the case of the oxymanganate and the oxychlorate of potash, it is possible, I conceive, to remove that unlikeness of constitution, so apparent in the following salts of like form:

The oxymanganate of barytes . . . $\dot{Ba}\dddot{Mn}\ddddot{Mn}$

The waterless sulphate of soda . . $\dot{Na}\dddot{S}$

These two salts we may better compare, unswayed by any theory, by regarding them in their ultimate components, thus:

$$\dot{Ba}\dddot{Mn}\ddddot{Mn} = Ba + 8O + 2Mn$$

$$\dot{Na}\dddot{S} = Na + 4O + S$$

But as manganese is isomorphous with sulphur, we can, better still, compare the two salts by taking the ultimate components of two atoms of sulphate of soda, thus:

$\dot{Ba}\dddot{Mn}.\ddddot{Mn} = Ba + 8O + 2Mn$ oxymanganate of barytes.

$2\,\dot{Na}\dddot{S} = 2Na + 8O + 2S$ sulphate of soda.

Instead whereof I suggest,

$$So + 8O + 2S"$$

(where So stands for the atom of sodium, being in weight double the received atom, which is represented by Na.)

" Comparing together, as we here do, so much of each of the salts as contains eight atoms of oxygen, we find two atoms of manganese substituted, without affecting the form of the compound, by two atoms of sulphur."

This proposal, as Professor Clark says of the difficulty which it is intended to overcome is "very startling"; but this is not all, for a theory still more destructive of what has been considered as settled is propounded by the Professor, in order that one difficulty in isomorphism may be got over, where so many exist.

After other hypothetical observations Professor Clark observes, "But partiality is unlike a law of nature, and indeed the partiality disappears when we regard the oxygen salts as having metals and not oxides for their bases." "According to this view, when oil of vitriol, regarded as an hydrogen acid $(H\,2\overset{::}{S})$ acts on an oxide, it is not a simple combination that takes place, but a double decomposition, resulting in a neutral salt and water, precisely as takes place when hydrochloric acid acts on oxides;" and the Professor adds, "if observation, which must be the final arbiter, shall determine one coincidence more to accord with the doubled atoms of sodium and silver, then, for aught I can see, the doctrine of oxygen salts having oxides for their bases must be at once abandoned."

Thus then because two salts are isomorphous which ought not so to be, according to the received atomic weights of certain bodies, these are not only to be altered, but we are on the brink of losing altogether the numerous class of oxygen salts.

It is however some consolation to observe that the champions of isomorphism differ so widely in their opinions, that some chance remains of the permanence of the received doctrines as to atomic weights and the constitution of oxygen salts.

In the last Number of the Philosophical Magazine, p. 324, Prof. Johnston has published a paper, the object of which is to prove that the atomic weights of soda and silver, &c., instead of being *doubled* to reconcile certain isomorphous discrepancies, ought to be *halved* for the same cause. In other words, the opinions of the Professors are as 4 to 1, on a subject in which though fancy has much to perform, observation and facts ought to decide.

Professor Johnston observes, "The isomorphism of two compounds generally implies an analogy in their atomic constitution,—that they are both sulphurets of the same order. If the compound of copper be a disulphuret $\underline{Cu}$, that of silver is most probably a disulphuret $\underline{Ag}$, and if so the atomic weight of silver must be reduced one half, or to 6·75. We have therefore an argument in favour of the old result of Dulong and Petit."

This however is not all: "the introduction of this change, however," continues Professor Johnston, "would render necessary a like change in the received atomic weight of sodium, with the oxide of which in anhydrous sulphate of soda (Thenardite) that of silver in sulphate of silver is isomorphous. Soda, common salt, and sulphuret of sodium must be represented $\underline{\dot{\mathrm{N}}\mathrm{a}}$, NaCl, $\dot{\mathrm{N}}\mathrm{a}\,\underline{\dot{\mathrm{N}}}$." "Nor," says Professor Johnston, "can the change stop here." Indeed I wish he could point out where it will. "Several years," he continues, "have elapsed since Mitscherlich announced the very interesting fact, that the nitrates of potash and soda were isomorphous respectively with arragonite and calc spar, and that they presented the same cleavages (Pogg., *An.*, xviii. p. 173); to which Marx afterwards added that the rhomboids of nitrate of soda possessed the doubly refracting structure in a higher degree even than calc spar. (*Jahrbuch der Chim. und Phys.*, xix. p. 166.) From these observations it was natural to infer that some relation existed between the two alkaline nitrates analogous to the relation between the two forms of carbonate of lime; that like carbonate of lime the nitrates of potash and soda might each be capable of assuming two forms isomorphous each with each, though in ordinary circumstances of temperature, &c. the form preferred by each did not correspond; the nitrate of potash *generally* affecting the right rhombic prism, the nitrate of soda the rhomboidal form. The probability of such a relation was strengthened by a comparison of the analysis of chabasie from different localities and by different chemists, from which there appeared strong reason for believing that potash and soda were capable of replacing each other in equivalent proportions."

I wish Professor Johnston had favoured us with the analyses from which he has arrived at the fore-mentioned conclusion.

I have copied all I can find; they amount to seven.

	*1.	2.	3.	4.	5.	6.	7.
Silica	43·33	50·65	49·17	49·07	48·756	48·988	46·184
Alumina	22·66	17·00	18·90	18·90	17·440	19·774	18·423
Lime........	3·34	9·73	...	...	10·468	4·068	7·029
Potash	9·34	1·70	...	12·19	1·458		
Soda........	...	...	12·19	...	...	6·066	5·967
Oxide of iron	...	...	...	...	...	0·404	0·397
Water	21·	19·50	19·73	19·73	21·720	20·700	22·
	99·67	98·58	99·99	99·89	99·842	100·	100·

I confess I am entirely at a loss to conjecture on what principle Professor Johnston finds "strong reason" for believing, upon the evidence of these analyses, that potash and soda are capable of replacing each other in equivalent proportions; but I admit there is abundant evidence to prove that they replace each other in proportions which are quite indefinite.

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* 1. Vauquelin.
2. Berzelius.
3. Ditto.
4. Arfwedson.
5. Dr. Thomson.
6. Lehunt.
7. Lehunt.

The only results which are at all favourable to Professor Johnston's position, are those of Vauquelin and Lehunt; in these I admit that the potash is to the soda nearly as 3 to 2, which are proportional to their atomic weights; but in one of Lehunt's analyses the quantity of lime is more than twice as great as that in Vauquelin's.

According to Berzelius the potash in one specimen amounts to 1·7, and the soda in the other to 12·19; according therefore to the rule which I have admitted, the soda should be only ⅔rds the weight of the potash, or 1·12 instead of 12·19; so that here we have one equivalent of potash isomorphized by about 10 equivalents of soda. This is quite at variance with the rule which I have quoted from Professor Johnston, viz. "that binary compounds which replace each other, contain not only the same absolute number of atoms, but also the atoms of the two elements in the same relative proportion."

After alluding to an anomaly formerly presented by the alums, but now supposed to be explained, respecting the isomorphism of potash and soda, Professor Johnston continues: "But all doubt has at length been removed from the relation between the forms of potash and soda by a beautiful observation of Frankenheim (Pogg., xi. p. 447). He has found that when a saturated solution of pure nitrate of potash is left in small quantity to spontaneous evaporation, two sets of crystals are formed, one in prisms, the other in rhomboids; the former the common arragonitic form generally assumed by nitrate of potash, the latter that of calc spar, commonly assumed by soda. The rhomboidal crystals are microscopic and pass into the prismatic form by friction, by pressure, or by contact with a prismatic crystal, and hence when the salt is crystallized in large masses they entirely disappear. It may therefore be considered as demonstrated that the nitrates of potash and soda are at once isomorphous and dimorphous, or isodimorphous; and since the potash and soda replace each other in certain mineral compounds (chabasie for example) the alkalies also, perhaps the metallic radicals themselves, may be considered isodimorphous."

I know not whether in the original paper cited by Professor Johnston it is stated that the "microscopic" crystals were submitted to measurement; if not I confess that I am not ready to admit the case as proved, for it might be easy to confound "microscopic" rhomboids and shortened prismatic crystals.

Professor Johnston, in alluding to the researches of Dulong and Petit into the specific heats of metals, observes that they have rendered it extremely probable that the atoms or equivalents of these elementary bodies have the same specific heat; and that the usually received atomic weights of silver, gold, and mercury should be halved. The table in which this comparison is exhibited contains sulphur and 13 metals; and with respect to it the late Dr. Turner remarks, "It will be observed on inspecting the last column of the table, that the product of the specific heat into the atomic weight is very nearly 3 for the first eight substances. Platinum deviates visibly from the law, and bismuth and cobalt strikingly. The three last metals (mercury, silver and gold) would nearly coincide with the law, were their respective atomic weight estimated at half the number given in the table." Waiving the objections which Dr. Dalton has made to the results of these experiments, it is, I think, requiring too much to ask chemists to reduce the atomic weights of three metals to one half, when of 3 others one deviates *visibly* and two *strikingly* from the law.

The difficulty with respect to mercury, gold, and silver may, however, I think, be got over in a way which I wonder did not suggest itself to Professor Johnston. If heat combines with bodies in definite proportions, it may in some cases like other elements combine in double proportions; instead therefore of halving the equivalents of these metals, let us suppose that they are combined with two equivalents of heat, and the revolution of atoms with which we are threatened will be rescued from this source of discrepancy.

It is an old observation that when things get to the worst they will mend; I hope therefore that two more Professors may attempt to remove the difficulties of isomorphism, one by multiplying the received atomic weights by 19, or any equally convenient number, and the other by dividing them by the same.

LXVIII. *Observations on Urinary Calculi, with a Descriptive Account of the Collection in the Museum of St. Bartholomew's Hospital. By* THOMAS TAYLOR, *Esq., M.R.C.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

HAVING completed the examination of the valuable collection of urinary calculi, of which the specimens of cystic oxide described in your April Number, p. 337, form a part, may I request the insertion of the following observations, should you deem them of sufficient interest to merit a place in your pages? The entire collection consists of 129 specimens, of which about one third were unexamined; the composition of the others had been pointed out some few years ago by Dr. Hue. It was however necessary to re-examine many of these, as from their not having been divided their internal structure had not been described. In this as in most other collections chemical composition has been taken as the basis of arrangement. This plan would be sufficiently simple and accurate if calculi were always homogeneous, but as by far the greater number consist of layers differing in composition, some additional method is necessary. In the present instance the alternating calculi have been classed according to the number of layers which are present, and these are subdivided with reference to the composition of the nucleus. I am aware that many objections might be urged against this method, and it would no doubt have been more scientific to have grouped together all those in which the layers observe a similar order of succession; but it was found that such an arrangement would have introduced so many subdivisions as completely to destroy that simplicity which in a museum continually subject to increase it was necessary to preserve.

The following table exhibits the relative frequency of each species, together with the order of succession of the layers in the alternating calculi.

Uric acid, nearly pure	11.
Urate of ammonia intimately mixed with variable proportions of oxalate of lime and the phosphates	8.
Oxalate of lime nearly pure	8.
Phosphate of lime	4.
Phosphate of ammonia and magnesia	1.
Mixed phosphates	10.
Ditto deposited on foreign bodies	3.
Cystic oxide	2.

Alternating Calculi.

Uric acid:	Urate of ammonia	4.
———	Oxalate of lime	3.
———	Phosphates	6.
Urate of ammonia:	Uric acid	2.
———	Oxalate of lime	7.
———	Phosphates	13.
Oxalate of lime:	Uric acid	3.
———	Urate of ammonia	1.
———	Phosphates	13.

REESE [illegible] UNI[illegible] CAL[illegible]

Uric acid:	Urate of ammonia:	Phosphates ...	3.
Ditto	Oxalate of lime:	Ditto	1.
Urate of ammonia:	Uric acid:	Phosphates ...	1.
————	Oxalate of lime:	Ditto ...	13.
————	Ditto	Uric acid ...	1.
————	————	Urate of ammonia	1.
Oxalate of lime:	Uric acid:	Ditto	1.
————	————	Oxalate of lime	1.
Calculi consisting of several layers			8.

Uric Acid.—Of these calculi, the greater number has been taken from adults; the finest specimen, which is remarkably compact and crystalline, was extracted by Mr. Lawrence from the bladder of a man aged 72: some few contain a little urate of ammonia, and in these a minute quantity of oxalate and phosphate of lime may be detected.

Urate of Ammonia.—In no table that has been given by writers on this subject has urate of ammonia been regarded as forming an independent concretion, and its existence as such has been much disputed. This difference in opinion has arisen from the difficulty of separating this salt, in an unexceptionable manner, from the triple phosphate with which it is frequently mixed.

The chief chemical evidence adduced in favour of its presence, is the evolution of ammonia when these calculi are treated with a solution of caustic potash. It has been contended, on the other hand, that the ammonia is derived from the decomposition of the phosphate of ammonia and magnesia, or of urea accidentally present. When the former is the case I am not aware of any method by which this objection can be set aside, for every plan which I have hitherto tried of separating these salts so as to avoid their reaction on each other has failed. There are, however, many calculi in which the presence of urate of ammonia can be satisfactorily shown, and which contain none of the triple phosphate, or which contain it in so small a quantity as to be inadequate to account for the quantity of ammonia combined with the uric acid; and as these calculi possess the same external appearances, and in their general chemical characters correspond exactly with those containing the phosphate of ammonia and magnesia, I think it fair to infer that their composition is similar. Notwithstanding therefore the deservedly high authorities by which the contrary opinion has been maintained, I must fully concur in the observation of Dr. Prout, that all those calculi which present when broken an amorphous and earthy-looking fracture, consist *essentially* of urate of ammonia. That the ammonia in these calculi is combined with uric acid may be shown in the following manner. Let a small quantity of boiling water be poured over a few grains of the calculus placed in a small paper filter; the solution will on cooling deposit a copious flocculent white precipitate of urate of ammonia, which from its appearance alone, may be easily distinguished from the scanty crystalline precipitate which takes place when uric acid calculi are similarly treated: should too much water have been added, it will be necessary to evaporate the solution a little before precipitation of the urate of ammonia will take place. Free uric acid is very frequently present in these calculi, and may be observed in the form of minute crystals mixed with the amorphous precipitate of urate of ammonia, or adhering to the sides of the vessel; when however the triple phosphate is present this test is of no value, for the mere affusion of hot water over a mixture of pure uric acid and phosphate of ammonia and magnesia will give rise to the formation of urate of ammonia, and thus vitiate the result. This fact appears to add considerable weight to the opinion of Dr. Prout, that uric acid is seldom or ever deposited in a free state together with the phosphates. There is another distinctive character by which these calculi may always be recognised, viz. their decrepitating on the application of heat: in no well-marked specimen has this property been wanting, and I consider it as perfectly characteristic of the species. To what this peculiarity is owing it is not easy to determine, for pure urate of ammonia does not decrepitate when heated. It has been generally referred to the small quantity of oxalate of lime contained in these calculi; but this is hardly probable, as oxalate of lime calculi undergo combustion silently, and the same property is possessed by those specimens in which the phosphates form the predominating admixture. It may possibly arise from the sudden extrication of ammonia, and its degree of force depend upon the compactness of the body, for in those calculi which from the predominance of the earthy phosphates are porous and friable, this property is considerably impaired or altogether lost. As far as my observation has gone the urate of ammonia in these calculi is never in a state of purity, all of them containing variable quantities of oxalate of lime, the phosphates, uric acid, and in some few instances urate of lime.

The quantity of earthy matter however in the compact varieties is very small, seldom exceeding a few parts per cent. 15 grs. of a specimen which was rather disposed to crumble, and in which the characters of urate of ammonia began to pass into those of the phosphates, gave on analysis:

Uric acid	9·1
Phosphate of lime	1·5
Phosphate of ammonia and magnesia	3·1
Animal matter, ammonia, and loss ...	1·3
	15·0

The ash which is left when these calculi are burnt is almost always alkaline and infusible: in three cases only were the phosphates present in such proportions as to render it fusible. By reference to the table it will be seen that urate of ammonia, so far from being rare, as is generally stated, forms in the present collection the most frequent primary deposit; as out of 82 calculi which have been divided, the proportion as to nuclei is as follows: uric acid 18, urate of ammonia 41, oxalate of lime 23; and in those which are homogeneous the proportion of urate of ammonia though less is still very considerable. It has been remarked by Dr. Prout, that this species of calculus generally occurs in children, and the accuracy of this observation is fully borne out by the histories attached to these calculi; for although unfortunately they are not so perfect as to enable me to institute a strict comparison of the relative frequency of each variety at the different periods of age, yet in the present case by far the greater number are expressly stated to have been taken from persons under puberty.

Phosphate of Lime.—Under this head are arranged some small calculi from the prostate gland, and three large irregular concretions from the kidney; two of these contained carbonate of lime and some urate of ammonia, the latter being apparently in separate layers. In one a small quantity of the phosphate of ammonia and magnesia was likewise present. Whether the phosphate of lime in these calculi was primarily secreted, or merely coated a nucleus of some other substance, is uncertain, as on account of their figure it was not considered advisable to divide them. The other specimen was examined by Dr. Hue, and consisted of phosphate of lime with a large quantity of animal matter.

The term bone earth which is frequently applied to these calculi is faulty, as it conveys the idea that the lime and phosphoric acid are in the same relative proportions as in the earthy matter of bones; whereas it has been shown by Dr. Wollaston that the calculi from the prostate gland contain a much larger proportion of acid, forming what is usually termed the neutral phosphate, or more correctly speaking the diphosphate. From several facts which I have observed, I am, however, convinced that the relative proportions of acid and base in the phosphate of lime surrounding other calculi, whether alone or mixed with the phosphate of ammonia and magnesia, varies considerably: and whether this arises from a mixture of two or more of the already known compounds of lime and phosphoric acid, or whether they are definite compounds of which we have at present no knowledge, I am unable to decide, although I believe the latter may occasionally be the case. In a calculus which consisted of urate of ammonia and oxalate of lime surrounded by the mixed phosphate, was observed among the latter a layer which had an imperfectly fibrous structure and was much harder in texture and more compact than the rest: on digesting a portion of this in dilute acetic acid effervescence took place and some lime was dissolved; the insoluble matter left had a crystalline appearance, and was found to be phosphate of lime. When dissolved in stronger acid and the solution neutralized by ammonia a gelatinous precipitate fell, which after standing about four and twenty hours was wholly converted into a number of small crystals, having undergone similar changes to freshly precipitated uric acid. If these crystals are left for a few days in the solution from which they have been thrown down, they gradually disappear, and are reconverted into an amorphous precipitate, differing only from the former in not being quite so gelatinous. The nature of the changes which take place I am unable at present to explain, although I find that when the diphosphate of lime (prepared by dropping a solution of phosphate of soda into one of muriate of lime, the latter being in excess,) is precipitated from its acetic solution, the same appearances present themselves: the conversion is, however, only partial. The calculi which contain this phosphate usually partake more or less of the external characters before mentioned; in some of them it appeared to be mixed with the bone-earth phosphate, properly so called. If it be identical in composition with the diphosphate, which I believe to be the case, the property alluded to is not noticed in any of the chemical works I have consulted. I am informed by Dr. Prout, that he has remarked the same. Only in one instance have I seen the radiated structure noticed by Dr. Wollaston; it formed a thin layer among the mixed phosphates, surrounding a calculus of oxalate of lime.

Phosphate of Magnesia and Ammonia.—This specimen has not been divided; it probably contains a nucleus of uric acid, and should therefore have been arranged among the alternating calculi.

Mixed Phosphates.—The calculi arranged under this division are composed throughout of the phosphate of lime and phosphate of magnesia and ammonia mixed in variable proportions. Some of these contain thin layers of urate of ammonia, and this salt is frequently present in the fusible calculus.

The number of calculi of this description is rather above the usual average, as the phosphates seldom form the primary deposit.

It is probable that some of these calculi were formed by the decomposition of urine, which from some cause or other could not escape from the bladder; such appears to have been the case in two of these specimens; one having been extracted by Mr. Stanley, from a cyst which communicated with a fistulous passage leading from the bladder to the perinæum; and the other having occurred in a patient in whom, on account of an enlarged prostate gland, lithotomy had been performed above the pubes, and through which opening the urine was subsequently expelled.

Cystic Oxide.—Of these calculi a full description has been given in the Phil. Mag. for April.

Carbonate of Lime.—This salt rarely forms the principal constituent in calculi from the human subject, and no specimen of the kind exists in the museum; it is however very frequently present in small quantities, and generally mixed with the phosphates.

Purpurate of Ammonia.—Of this singular substance it is not easy to obtain decisive chemical evidence, partly on account of the small quantities in which it occurs, and partly on that of the facility with which it undergoes changes by which its colour is destroyed. I believe I am correct, however, in stating that I have detected it in three instances. In one it formed flesh-coloured layers alternating with the phosphates, in the others it merely coated the calculus. In all of them it was mixed with urate of ammonia.

With regard to the alternating calculi the table that has been given expresses nearly all that is worthy of particular notice. It may be observed that in no one instance have the phosphates either pure or in a state of mixture formed the nucleus; indeed this circumstance is so extremely rare, that it has been laid down as a general law by the highest authority on the subject, "that a decided deposition of the mixed phosphates is not followed by other depositions." There is, however, one specimen in the museum which must be regarded as presenting an exception to this statement.

The calculus in question consists at its centre of urate of ammonia containing a little oxalate of lime, around this is oxalate of lime nearly pure, a white layer three eighths of an inch in thickness follows, and is followed by a thin stratum of oxalate of lime of a very dark colour; upon this is deposited crystalline uric acid marked with the irregular concentric lines peculiar to oxalate of lime calculi, although it contains but a mere trace of that substance: the whole was coated by urate of ammonia, uric acid, and oxalate of lime irregularly deposited. As in the museum catalogue the white layer was merely described as fusible, and as Dr. Prout (to whom, with the permission of Mr. Stanley, I had the pleasure of showing this specimen,) suggested that it might contain urate of soda, it was carefully examined for that substance, and the result was, that in addition to the mixed phosphates with some carbonate of lime, a small quantity of uric acid and soda was present. The quantity of the latter was, however, very minute; oxalate of lime could not be detected.

It is highly probable that in this case the deposition of the phosphates had been caused by the use of alkaline remedies, and that on the discontinuance of these, the former diathesis had returned. If this were the case it can hardly be considered as a fair exception to the law above mentioned.

By most writers on this subject, a species of calculus has been noticed, consisting of the different ingredients mixed indiscriminately together, from which circumstance it has been termed mixed or compound. The only specimens which appear to me to deserve this appellation are the mixed phosphates and the less pure varieties of urate of ammonia. As however there is no calculus which is *absolutely* pure, and it would be exceedingly difficult to decide what proportion of the dissimilar ingredients should constitute a mixed calculus, this class has not been included in the arrangement. I may, however, remark that with the exception of those layers which occasionally intervene between two different deposits, and which, as has been remarked by Dr. Prout, usually consist of a mixture of the old and new layers, only two specimens have come under my notice at all approximating to the so-called mixed calculus, or in which the slightest hesitation occurred in assigning their proper place: of one of these I have given the analysis under the head of urate of ammonia: the other contained a much larger relative proportion of the mixed phosphates and surrounded a nucleus of uric acid, it was therefore arranged among the alternating calculi. Although in the foregoing observations I have endeavoured to confine myself to points of general interest or on which a difference of opinion existed, yet I am afraid they have already extended to too great a length, and I shall therefore no longer intrude upon your notice, merely requesting their insertion at your earliest convenience.

I am, Gentlemen, yours, &c.
New Bridge Street, April 13, 1838. THOMAS TAYLOR, *M.R.C.S.*

LXIX. *On the Luminosity of the Human Subject after Death, with Remarks and Details of Experiments made with a view of determining the nature of the fact.* By Mr. DANIEL COOPER, *A.L.S., Curator to the Botanical Society of London, &c.,* and Mr. ROBERT COOPER.*

ON the 14th day of February, 1838, the body of William Tomkins, aged 88, shoemaker by trade, was received at the Webb-street School of Anatomy and Medicine, Borough, having died of age and debility; and on the 3rd of March that of Robert Boreham, aged 45, was also received with the following history. It appeared that this individual, previous to his death, had been observed in the street in a state of extreme poverty, and was accordingly conveyed by a police officer on duty to the Station House, where, from extreme fatigue and exhaustion, the man died on the 26th of February. Being an unclaimed corpse, the parish authorities (according to the regulations of the Act of Parliament for supplying the Anatomical Schools with subjects for dissection) sent it to Webb-street. Previous to the reception of the latter, nearly the whole of the first subject had been dissected, and the only part which exhibited the luminous property was the left leg, which had been removed, according to custom, at the upper third of the thigh. Not having been informed of this phænomenon until it had been despatched for interment, we had not the opportunity of making experiments with regard to the cause.

We were, however, more fortunate with the latter subject, which presented the same appearance, but in a greater degree. Upon examination it was evident that the man had been a muscular and likewise a hard-working individual, if we might be allowed to judge from the appearance of the skin of the palms of the hands.

The phænomenon was first observed by Mr. J. Appleton, (the Curator to the above establishment,) on Saturday the 3rd March, upon taking his accustomed round of an evening to every part of the building previous to retiring to rest. He was greatly surprised at perceiving the extremity before mentioned to be *luminous*; never having heard or witnessed in the whole course of his experience, commencing in 1812, a similar occurrence.

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* Communicated by the Authors: see the Intelligence and Miscellaneous Articles in a future page.

A few nights after the introduction of Boreham, Mr. Appleton observed this subject to be similarly affected, and the following morning communicated this fact to the Professors of the School. The circumstance having become generally known to the Pupils, several assembled the next evening for the purpose of observing this singular and novel phænomenon, when it was remarked by Mr. Appleton that the luminosity had considerably increased since its first appearance.

This novel fact we consider deserving the attention of physiologists, for in no work can we find recorded any notice of the phosphorescent or luminous appearance of the *human subject.* We are fully aware of its occurrence in many of the lower and even the higher classes of animals; but we are not aware of the present fact having been heretofore recorded.

Development of Light in the Lower Animals, and certain Substances.—Müller, the celebrated German physiologist, in a late edition of his work on the Elements of Physiology, (translated by W. Baly) makes some observations with respect to animals &c., which possess the power of emitting a phosphorescent light. The following is but a brief abstract from the work. Müller commences with a description of the animals which produce the phosphorescence of the sea, and enumerates some of the *Infusoria*, *Polipifera*, *Medusæ*, *Annelides*, *Planariæ*, and *Mollusca*, mentioning occasionally some of the leading genera in each group. He then notices some of the leading phosphorescent *crustaceans* and *insects*, and mentions the opinion of Treviranus with respect to the light emitted from insects, viz. that the internal parts of generation are the source of light. He then further alludes to the opinion of Treviranus, viz. that light is derived from matter containing *phosphorus*, which is formed under the influence of light, but once formed is in some measure independent of light. He then brings forward the opinion of Carradori, Beccaria, and Monti as to the power of certain bodies of absorbing light during the day and emitting it during the evening, as is evidenced by several mineral substances, such as sulphate of barytes mixed with sulphuret of barium, oyster shells heated to redness with sulphur, &c., and also by several organic substances when dried, such as seeds, flour, starch, acacia gum, quills, cheese, yolk of egg, *muscle*, *tendon*, isinglass, glue, and horn.

We are informed, according to the observations of Mr. White, that he has repeatedly witnessed the luminous appearance of birds when they have been hanging for some time. Whilst we ourselves are aware of the fact, that many animals, such as dogs, cats, &c., which have been killed, and left exposed to the atmosphere in ditches, &c. have emitted a phosphorescent light; we have also been informed by Mr. Nazer that he has distinctly observed this luminous property in *veal*. In order further to verify the fact that it has been observed in several Mammalia, we have made diligent inquiries of several purveyors of meat in the metropolis, who have the opportunity of seeing meat in all its stages of decomposition, and we are informed, that they have at times perceived it on a dark night to be slightly luminous; but such a phænomenon is of rare occurrence. The common opinion with these individuals with respect to the cause, is, that the meat had been struck by lightning, it having been generally observed in the summer months.

On the parts of the Body most affected.—When the luminosity was discovered in Boreham, it was observed to occupy both the interior and exterior of the thorax, and gradually extended to other parts of the body, more especially the bones, tendons and fasciæ, and also to the muscles, but in a slighter degree. On Saturday, March 10th, we observed the cartilages and bones of the ribs extending from the fourth to the seventh on the right side, and on the back from the fifth to the ninth dorsal near their point of attachment to their vertebræ: the light in the interior corresponded in situation to the light on the exterior of the thorax; there was no phosphorescence observed on the viscera of the chest or abdomen. It likewise extended over the right and slightly over the left lumbar, sacral, and iliac regions as far down as the insertion of the tensor vaginæ femoris between the two laminæ of the fascia lata, from which fascia we were enabled to remove it with our fingers, and to them it gave a luminous appearance. On the evening of the following Monday we continued our researches. On entering the room we observed it greatly diminished in intensity; and upon examining the body we perceived the right knee (the integument of which had that day been removed) to be very luminous. Upon taking a scalpel and scraping the bone, we were surprised to find that the luminosity in no way diminished: although we could remove it by continual scrapings, it seemed to extend into the substance of the bones.

Power of increase.—In order to ascertain whether Boreham had been inoculated with the matter from Tomkins, we placed a portion of the luminous matter taken from the former on the chest of another subject, situated on the opposite side and further end of the room, on Monday the 12th; and accordingly on Wednesday the 14th, as we anticipated, we discovered the trunk of the inoculated subject to be luminous to a very great extent. This occurrence clearly proves to our minds, that the matter from Tomkins had inoculated the body of Boreham. In order to ascertain whether the luminosity was situated in the moist or dry parts, we noted in the dark the situation of the luminosity; which upon examination in the light showed it to be the moistened parts.

Microscopic Observations.—With a view of elucidating this phænomenon we submitted a portion of the luminous matter scraped carefully from the subject to microscopic examination, in the first place, with the idea that from its exceedingly rapid augmentation it was due to an animal very low in the scale of organization. Our first examination led us to suppose, from the peculiar motion of some of the molecules in the fluid, that an animal of extreme minuteness was present: but upon further examination, with the assistance of Mr. Bowerbank's microscope and experience in these matters, we were convinced that no such animal as the *Monas* existed in the matter. It was not until we had an opportunity of witnessing the various but similar currents in a weak solution of gamboge, that we could reconcile ourselves to the appearance; for at times we observed small globules starting from one side to the other, and occasionally stemming the current for a considerable distance. Until we had observed the similar motion in the gamboge, we did not feel perfectly satisfied that no living being there existed. In the course of the examination, Mr. Bowerbank observed a small threadlike body dart across the field of the microscope, which he immediately recognised as one of those bodies (*Vibriones*) which are so abundantly seen upon macerating animal matter, such as a mouse, in water for a length of time. The power of the lens under which we observed the above was about 900; and speaking in general terms of the size of the molecules before mentioned, as viewed under the above power, they were as near as could be ascertained the 100,000th of an inch in size; so small indeed were they, that it was totally impossible to measure them with the finest micrometer as yet constructed. And according to the measurement of certain animals which among others produce the luminosity of the sea, as measured by Mr. Bowerbank at about 100th of an inch, these molecules, for we will not give them any higher title, were at least 1000 times smaller. Although not exactly in connection with the present subject we have given the rough estimate of the molecules observed, as compared with the minute animals which are known to give the sea the beautiful phosphorescent appearance so frequently observed.

A portion of luminous matter having been placed under the microscope, the light evolved was sufficient to illuminate the field in patches. The luminosity appeared to be emitted from an *oily matter.*

Experiments with gases.—Having been led to suppose from the microscopic examinations that there were no traces of animals, we resolved to repeat the experiments of Macartney and Murray as regards the non-disappearance of the phosphorescent light emitted from animals in the different gases. For this purpose we prepared in well-stoppered phials the following gases, viz. oxygen, hydrogen, nitrogen, chlorine, carbonic acid, carbonic oxide, sulphuretted and phosphuretted hydrogen; and into a phial filled with each of these gases we introduced a portion of luminous muscle, tendon, or fascia for the space of 40 minutes, and the following were the results of the experiments:—

No effect observed in	Slight effect produced in	Total extinction in
Oxygen Hydrogen Nitrogen } remained luminous for five days.	Carbonic acid.	Chlorine. Sulphuretted hydrogen.
Carbonic oxide.		
Phosphuretted Hydrogen.		

From the above experiments, we are compelled to disagree with the conclusions of Macartney and Murray as regards the non-disappearance of the phosphorescent light emitted from animals when immersed in the different gases. By perusing the above table, it will be observed that a total extinction of the light takes place when immersed in chlorine and sulphuretted hydrogen: this took place within the space of two minutes.

Appearance in vacuo.—On this point, we cannot coincide with the opinions of Macartney and Murray, having introduced an exceedingly luminous portion of flesh under the receiver of an air pump; and upon exhausting the vessel, the phosphorescence almost entirely disappeared, after having been *in vacuo* for the space of 15 minutes; but upon the readmission of the atmosphere, it immediately regained its former brilliancy, which is contrary to the opinions of the above experimentalists. Upon removing the portion of flesh from the phial containing carbonic acid, and placing it beneath the receiver of the air pump, and on first exhausting, it appeared to regain its luminosity in a slight degree; further exhaustion however diminished it. Oxygen having been admitted in the place of air, it soon regained its original brilliancy; this effect was in like manner produced by the admission of the atmosphere as before, and also by some of the different gases above-mentioned.

Effect in condensed air.—From the result of the foregoing experiment, viz. the diminution of the brilliancy upon withdrawing the atmosphere, we were led to suppose that a contrary effect would be produced upon condensing the air: to effect this, we procured a Cavendish bottle, in which a portion of luminous matter was placed. Upon using the condensing syringe, a visible increase of brilliancy occurred.

Luminous appearance under water.—Upon taking a portion of luminous flesh, and placing it in a glass of distilled water, it retained its luminosity for the space of from 10 to 15 minutes; and upon carefully removing the luminous matter from another portion of flesh with a knife, and agitating the water with the instrument, small globules of luminous matter were observed dispersed throughout the fluid, which remained for the space of 1 minute and a half.

Appearance in milk.—Upon treating the matter as in the preceding experiment, it gives to the fluid a very luminous appearance, which lasts from 15 to 20 minutes; the brilliancy dependent upon the quantity of matter introduced.

In Oil.—The luminous appearance remains in this medium for the space of three or four days. Upon rubbing the immersed flesh against the sides of the glass, it became more vivid.

In Alcohol.—Upon immersion in this fluid, it is extinguished in the space of two minutes. It does not impart to the alcohol the same appearance as is observed in the water or milk.

Heat.—Immediately extinguished, upon being placed in boiling water and heated air.

Cold.—Upon placing a portion in a glass, and suspending the glass in a freezing mixture, no effect was observed after the lapse of 30 minutes.

Effects in the Diluted Mineral Acids.—Strength of solution 6 fluid drachms of acid to 2 fluid ounces of water.

Sulphuric Acid.—Extinguished almost immediately.

Nitric Acid.—Effect not so immediate as the preceding.

Muriatic Acid.—Not so immediate as the nitric.

Diluted Vegetable Acids.—Solution same proportions as above.

Acetic Acid.—Soon out after immersion.

Tartaric Acid.—Not so immediate as preceding.

Oxalic Acid.—Requires a longer period than tartaric.

Diluted Alkalies.—Ammonia.—Extinguished on immersion.

Potassa.—In this medium some time is required to extinguish the luminosity.

Muriate of Soda.—A strong solution of this substance extinguishes it almost immediately.

It would be difficult to state the true nature of the cause of this phænomenon. From our own observations, and the results of the above experiments, we are inclined to believe that it is the effect of a *peculiar state* of decomposition, totally independent of atmospheric causes, the luminosity residing (to the best of our belief,) in the *oily matter*, which we observed upon submitting it to microscopic examination: we hope however, that at some future period, we may have an opportunity of observing the same phænomenon, and continuing our researches.

82, Blackfriars Road, London.

LXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 368.]

Feb. 15, 1838. A Paper was in part read, entitled "Experimental Researches in Electricity," Twelfth Series, by Michael Faraday, Esq., D.C.L., F.R.S., &c.

February 22.—The reading of a paper, entitled, "Experimental Researches in Electricity," Twelfth Series, by M. Faraday, Esq., D.C.L., F.R.S., was resumed.

March 1.—The reading of a paper, entitled "Experimental Researches in Electricity," Twelfth Series, by Michael Faraday, Esq., D.C.L., F.R.S., &c., was resumed and concluded.

Experimental Researches in Electricity: Twelfth Series. By Michael Faraday, Esq., D.C.L., F.R.S., Fullerian Professor of Chemistry in the Royal Institution of Great Britain.

The object of the present series of researches is to examine how far the principal general facts in electricity are explicable on the theory adopted by the author, and detailed in his last memoir *, relative to the nature of inductive action. The operation of a body charged with electricity, of either the positive or negative kind, on other bodies in its vicinity, as long as it retains the whole of its charge, may be regarded as *simple induction*, in contradistinction to the effects which follow the destruction of this statical equilibrium, and imply a transit of the electrical forces from the charged body to those at a distance, and which comprehend the phenomena of the *electric discharge*. Having considered, in the preceding paper, the process by which the former condition is established, and which consists in the successive polarization of series of contiguous particles of the interposed insulating dielectric; the author here proceeds to trace the process, which, taking place consequently on simple induction, terminates in that sudden, and often violent interchange of electric forces constituting *disruption*, or the electric discharge. He investigates, by the application of his theory, the gradual steps of transition which may be traced between perfect insulation on the one hand, and perfect conduction on the other, derived from the varied degrees of specific electric relations subsisting among the particular substances interposed in the circuit: and from this train of reasoning he deduces the conclusion that *induction* and *conduction* not only depend essentially on the same principles, but that they may be regarded as being of the same nature, and as differing merely in degree.

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* See our present volume, p. 358.

The fact ascertained by Professor Wheatstone, that electric conduction, even in the most perfect conductors, as the metals, requires for its completion a certain appreciable time*, is adduced in corroboration of these views; for any retardation, however small, in the transmission of electric forces can result only from induction; the degree of retardation, and, of course, the time employed, being proportional to the capacity of the particles of the conducting body for retaining a given intensity of inductive charge. The more perfect insulators, as lac, glass and sulphur, are capable of retaining electricity of high intensity; while, on the contrary, the metals and other excellent conductors, possess no power of retention when the intensity of the charge exceeds the lowest degrees. It would appear, however, that gases possess a power of perfect insulation, and that the effects generally referred to their capacity of conduction, are only the results of the carrying power of the charged particles either of the gas, or of minute particles of dust which may be present in them: and they perhaps owe their character of perfect insulators to their peculiar physical state, and to the condition of separation under which their particles are placed. The changes produced by heat on the conducting power of different bodies is not uniform; for in some, as sulphuret of silver and fluoride of lead, it is increased; while in others, as in the metals and the gases, it is diminished by an augmentation of temperature.

One peculiar form of electric discharge is that which attends *electrolyzation*, an effect involving previous induction; which induction has been shown to take place throughout linear series of polarized particles, in perfect accordance with the views entertained by the author of the general theory of inductive action. The peculiar feature of this mode of discharge, however, is in its consisting, not in a mere interchange of electric forces at the adjacent poles of contiguous particles, but in their actual separation into their two constituent particles; those of each kind travelling onwards in contrary directions, and retaining the whole amount of the force they had acquired during the previous polarization. The lines of inductive action which occur in fluid electrolytes are exemplified by employing for that purpose clean rectified oil of turpentine, containing a few minute fibres of very clean dry white silk; for when the voltaic circuit is made by the introduction into the fluid of wires, passing through glass tubes, the particles of silk are seen to gather together from all parts, and to form bands of considerable tenacity, extending between the ends of the wires, and presenting a striking analogy to the arrangement and adhesion of the particles of iron filings between the poles of a horse-shoe magnet.

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* See Lond. and Edinb. Phil. Mag. vol. vi. p. 61.

The fact that water acquires greater power of electrolytic induction by the addition of sulphuric acid, which not being itself decomposed, can act only by giving increased facility of conduction, is adduced as confirming the views of the author.

The phenomena of the disruptive electric discharge are next examined with reference to this theory: the series of inductive actions which invariably precede it are minutely investigated: and reference is made to the accurate results obtained by Mr. Harris, as to the law of relation between the intensity of a charge, and the distance at which a discharge takes place through the air.

The theory of Biot and others, which ascribes the retention of a charge of electricity in an insulated body to the pressure of the surrounding atmosphere, is shown to be inconsistent with various phenomena, which are readily explained by the theory adopted by the author.

The author then enters into an inquiry relative to the specific conducting capacities of different dielectrics.

With a view of determining the degrees of resistance to the transit of electricity excited by different kinds of gases, he constructed an apparatus, in which an electric discharge could be made along either of two separate channels; the one passing through a receiver filled with the gas, which was to be the subject of experiment, and the other having atmospheric air interposed. By varying the length of the passage through the latter, until it was found that the discharge occurred with equal facility through either channel, a measure was afforded of the relative resistances in those two lines of transit, and a determination consequently obtained of the specific insulating power of the gas employed.

The circumstances attending the diversified forms of the disruptive discharge, such as the vivid flash or spark, the brush or pencil of light, and the lucid point or star, which severally represent different conditions of the sudden transit of electrical forces through an intervening dielectric, are minutely investigated in their various modifications. The spark is the discharge, or reduction of the polarized inductive state of many dielectric particles, by the particular action of a few of those particles occupying but a small and limited space, leaving the others to return to their original or normal condition in the inverse order in which they had become polarized: and its path is determined by the superior tension which certain particles have acquired, compared with others, and along which the action is accordingly conducted in preference to other lines of transit. The variety in the appearance of the electric spark taken in different gases may be ascribed partly to different degrees of heat evolved, but chiefly to specific properties of the gas itself with relation to the electric forces. These properties appear also to give occasion to diversities in the form of the pencil or brush, which takes place when the discharge is incomplete, and is repeated at short intervals, according to the shape of the conductor on either side, and according to the species of electricity conveyed. The diverging, converging, bent and ramified lines presented in these different forms of electric discharge, strikingly illustrate the deflexions and curvilinear courses taken by the inductive actions which precede the disruption; these lines being not unlike the magnetic curves in which iron filings arrange themselves when under the action of opposite magnetic polarities.

March 8.—A paper was read, entitled, "Proposal for a new method of determining the Longitude, by an absolute Altitude of the Moon," by John Christian Bowring, Esq. Communicated by John George Children, Esq., F.R.S.

The method employed by the author for determining the longitude by the observation of an absolute altitude of the moon, was proposed, many years ago by Pingré and Lemmonier; and the principal difficulty which stood in the way of its adoption, was its requiring the exact determination of the moon's declination reduced to the place of observation. This difficulty the author professes to have removed by supposing two meridians for which the altitudes are to be calculated: and the only remaining requisite is the accurate determination of the latitude, which presents no great difficulty, either on land or at sea. Examples are given of the practical working of this method; showing that if the latitude of a place of observation be obtained within a few seconds, the longitude will be found by means of a single observation of the altitude of the moon.

A paper was also read, entitled, "An Inquiry into a new Theory of earthy Bases of Vegetable Tissues," by the Rev. J. B. Reade, M.A., F.R.S.

The author, after briefly noticing the results of some of his experiments described in two papers which appeared in the Philosophical Magazine for July and November, 1837, and also those of Mr. Robert Rigg in a paper read to the Royal Society*, next adverts to the theory of M. Raspail, detailed in his *Tableau Synoptique*, and *Nouveau Système de Chimie*. In opposition to some of the views entertained by the latter, he finds that in the bark of the bamboo and the epidermis of straw the silica incrusting these tissues is not crystallized, but, on the contrary, exhibits, both before and after incineration, the most beautiful and elaborate organization, consisting of an arranged series of cells and tubes, and differing in its character in different species of the same tribe, and in different parts of the same plant.

The observations of Mr. Golding Bird, contained in the 14th number of the Magazine of Natural History, New Series, are then referred to; and the author states in confirmation, that, by employing caustic potash, the siliceous columns may be removed from the leaf of a stalk of wheat, while the spiral vessels and ducts, which form the principal ribs of the leaf, as well as the apparently metallic cups which are arranged on its surface, remain undisturbed. He proposes, therefore, to substitute, in the description of vegetable tissues, the term *skeleton*, instead of that of *bases*, whether saline or siliceous, of those tissues.

March 15.—The reading of a paper, entitled, "Experimental

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* See Lond. and Edinb. Phil. Mag., vol. ix. p. 535.

Researches in Electricity," Thirteenth Series, by Michael Faraday, Esq., D.C.L., F.R.S., &c., was commenced.

March 22.—A paper was read, entitled, "Description of a new Tide-Gauge, constructed by T. G. Bunt, and erected on the Eastern bank of the River Avon, in front of the Hotwell House, Bristol, in 1837." Communicated by the Rev. William Whewell, M.A., F.R.S.

The principal parts of the machine here described, are an eight-day clock, which turns a vertical cylinder, revolving once in twenty-four hours; a wheel, to which an alternate motion is communicated by a float rising and falling with the tide, and connected by a wire with the wheel which is kept constantly strained by a counterpoise; and a small drum on the same axis with the wheel, which by a suspending wire communicates one 18th of the vertical motion of the float to a bar carrying a pencil which marks a curve on the cylinder, or on a sheet of paper wrapped round it, exhibiting the rise and fall of the tide at each moment of time. The details of the mechanism, illustrated by drawings, occupy the whole of this paper.

A paper was also read, entitled, "On the Régar or Black Cotton Soil of India," by Capt. Newbold, Aide-de-Camp to Brigadier-General Wilson. Communicated by S. H. Christie, Esq., M.A., Sec. R.S.

The author states that the Régar of India is found, by chemical analysis, to consist of silica, in a minute state of division, together with lime, alumina, oxide of iron, and minute portions of vegetable and animal *débris*. Hence it is usually considered as having been formed by the disintegration of trap rocks: the author, however, after examining its numerous trap dykes traversing the formation of the ceded districts, which he found invariably to decompose into a ferruginous red soil, perfectly distinct from the stratum of black régar through which the trap protrudes, was led to regard this opinion of its origin as erroneous: and from the circumstance of its forming an extensive stratum of soil covering a large portion of the peninsula of India, he believes it to be a sedimentary deposit from waters in a state of repose.

Specimens of basaltic trap and of the Régar soil were transmitted to the Society by the author, for the purpose of analysis.

The reading of a paper, entitled, "Experimental Researches in Electricity," Thirteenth Series, by Michael Faraday, Esq,, D.C.L., F.R.S., &c., was resumed but not concluded.

March 29, 1838.—The reading of a paper, entitled, "Experimental Researches in Electricity," Thirteenth Series, by Michael Faraday, Esq., D.C.L., F.R.S., was resumed but not concluded.

April 5, 1838.—The reading of a paper, entitled, "Experimental Researches in Electricity," Thirteenth Series, by Michael Faraday, Esq., D.C.L., F.R.S., was resumed and concluded.

The author, in this paper, pursues the inquiry into the general differences observable in the luminous phenomena of the electric discharge, according as they proceed from bodies in the positive or the negative states, with a view to discover the cause of those differences. For the convenience of description he employs the term *inductric*, to designate those bodies from which the induction originates, and *inducteous* to denote those whose electric state is disturbed by this inductive action. He finds that an electric spark, passing from a small ball, rendered positively *inducteous*, to another ball of larger diameter, is considerably longer than when the same ball is rendered positively *inductric*; and that a similar difference, though to a less extent, is observable, when the smaller ball is rendered negative. The smaller ball, rendered positive, gives also a much longer spark than when it is rendered negative; in which latter case, however, it affords, at equal distances, a luminous brush of greater size, and gives it much more readily than when positive. In order to ascertain the relative degrees of charge which the balls acquire before the occurrence of the discharge, the author employed an apparatus attached to the insulated conductor of the electrical machine, and also to the conductor connected with the discharging train, and consequently uninsulated, consisting, on each side, of a rod branching out in the form of a fork, and terminating, at one of its extremities in a large ball, and at the other in a small one; the position of the forks being capable of adjustment, so that the large ball of each rod might be brought exactly opposite to the small one of the other: and the distances between each pair admitted of being regulated at pleasure, until the discharges through each interval were rendered apparently equal to one another. From numerous experiments made with this instrument, the author concludes that when two conducting surfaces of small but equal size, are placed in air, and electrified, the one positively and the other negatively, a discharge takes place at a lower tension from the latter than from the former; but that, when a discharge does occur, a greater quantity of electricity passes at each discharge from the positive, than from the negative surface. Experiments of a similar nature were made in gases of different kinds, by enclosing them in an apparatus constructed on the same plan as the former one, but capable of acting in a receiver, from which the air could be exhausted, and the particular gas, whose powers in modifying the electric discharges were to be ascertained, could be introduced in its place. The results of various trials are given in a table, from which it appears that different gases restrain the discharge in very different degrees. The discharge from the small ball, through nitrogen and hydrogen gases, most readily takes place when the charge is positive; and through oxygen, carbonic acid, and coal gas, when it is negative.

The author next directs his attention to the peculiar luminous phenomena attending the disruptive electrical discharge, which he terms *a glow*, and which appears to depend on a quick, and almost instantaneous charge given to the air in the immediate vicinity, and in contact with the charged conductor; and he enters into a detailed account of the circumstances by which it is influenced, and its production favoured; such as diminution of the charging surface, increase in the power of the machine, rarefaction of the surrounding air, and the particular species of electricity concerned. The relations which the glow, the brush, and the spark bear to one another, as well as the steps of transition between each are minutely investigated; and the conclusion is deduced that the glow is in its nature exactly the same as the luminous part of a brush or ramification, namely, a charge of air; the only difference being that the glow has a continuous appearance from the constant renewal of the same action in the same place, whereas the ramification is occasioned by a momentary and independent action of the same kind. The disruptive discharge may take place at degrees of tension so low as not to give rise to any luminous appearance; so that a dark space may intervene in the line of actual discharge, as is frequently observable between the brush on one side, and the glow on the other. Thus it is inferred that electric light is merely a consequence of the quantity of electricity which, after a discharge has commenced, flows and converges towards the spot where it finds the readiest passage: and these conclusions are further confirmed by the phenomena which take place in other gases, besides atmospheric air, and which are specifically detailed by the author.

The last kind of discharge which is here considered is the *convective* or *carrying discharge*, namely, that effected by the translation of charged particles from one place to another. The phenomena attending this mode of transference are examined under various aspects as they occur in air, in liquids of various kinds, in flame, and as they are exhibited in the case of particles of dust, which perform the office of carriers of the electricity; and also in that of solids terminated by liquids. Thus all these apparently isolated phenomena comprised under the heads of the electric currents which characterize electrolyzation, of transference through dielectrics by disruptive discharges of various kinds, or by the actual motion of charged particles, and of conduction through conductors of various degrees of power, are assimilated to one another by their being shown to be essentially the result of actions of contiguous particles of matter assuming particular states of polarization.

The author lastly considers electric currents, not only in their effects on the bodies they traverse, but also in their collateral influences as producing inductive and magnetic phenomena. The analogies, which connect electrolytic discharge with that by conduction, are pointed out, as tending to show that they are essentially the same in kind, and that when producing different kinds of motion in the particles of matter, their mode of operation may be regarded as identical. An attempt is made to connect with these views the lateral or transverse actions of currents, which are most distinctly manifested in their magnetic effects; these effects being produced equally by the disruptive, the conductive, and the electrolytic discharges, and probably depending on the transverse condition of the lines of ordinary induction. This transverse power has the character of polarity impressed upon it, and, in its simplest form, appears as attractive or repulsive, according as the currents themselves are in the same, or in opposite directions. In the current and in the magnet it assumes the condition of tangential force; and in magnets and their particles it produces poles.

The author announces that he intends shortly to develop, in another series of these researches, some further views which he entertains concerning the nature of electric forces and electric excitation in connexion with the theory he has here advanced.

The Society then adjourned over the Easter Recess to meet again on the 26th of April.

GEOLOGICAL SOCIETY.

Annual General Meeting, Feb. 16*.

After the reading of the usual reports, the President presented the Wollaston Medal to Mr. Richard Owen, and, on doing so, said,

MR. OWEN,

I have peculiar pleasure in presenting to you this Medal, awarded to you by this Society for your services to Fossil Zoology in general, and, in particular, for the description of the Fossil Mammalia collected by Mr. Darwin. I trust it will be a satisfaction to you to receive this our testimony of the success with which you have cultivated that great science of comparative zoology, to which you have devoted your powers. I trust it will add to your satisfaction to consider that the subject which we more peculiarly wish to mark on this occasion, in the study of Fossil Zoology, is one to which the resources of your science were applied, while the subject was yet new, by that great man, John Hunter, whose Museum and whose reputation are so worthily assigned to your care. I trust also that this Medal thus awarded to you at the outset, if I may so say, of an enlarged series of investigations, will convey to you the assurance that, in your progress in such researches, you carry with you our strong interest in your endeavours, and our high esteem of your powers and your objects; and will convince you that in all your successes, you may reckon upon our most cordial sympathy in the pleasure which your discoveries give.

Mr. OWEN acknowledged his sense of the distinction conferred upon him in the following terms:—

I wish, Sir, that I had words adequately to express my deep sense of the honour I have now received; but I feel assured that you will grant to me the sincerity of my brief acknowledgments. The study of the animal organization has always abundantly repaid me by the pleasure which naturally flows from the contemplation of the marvellous skill with which, in the complete frame of existing species, structures are modified and designed in relation to particular ends; and from the perception of a subordination of the various instruments to one general plan. But since I have pursued anatomical investigations in connection with fossil remains, I have been rewarded by new and extrinsic pleasures. I trace to this source my connexion with the Geological Society, and the possession of some most valued friendships; and now, Sir, my obligations to the Society, and to Palæontology are increased ten-fold by the unexpected honour I have this day received at your hands. I receive this testimony of your good opinion as a strong stimulus to future endeavours. I cannot permit myself to regard it as a reward for the inadequate contributions which I have hitherto been able to make to the history of lost species; and I pledge myself to lose no available time or opportunity which may be applied to further that branch of geological science which has the extinct animals of this planet for its immediate object.

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* The papers read previously to the Anniversary will be noticed in future numbers.

The following gentlemen were elected the Officers and Council for the ensuing year.

President.—Rev. William Whewell, M.A. F.R.S. *Vice-Presidents.*—William Henry Fitton, M.D. F.R.S. & L.S.; Charles Lyell, Jun. Esq. F.R.S. & L.S.; Roderick Impey Murchison, Esq. F.R.S. & L.S.; Rev. Adam Sedgwick, F.R.S. & L.S. *Secretaries.*—Charles Darwin, Esq.; William John Hamilton, Esq. *Foreign Secretary.*—H. T. De la Beche, Esq. F.R.S. & L.S. *Treasurer.*—John Taylor, Esq. F.R.S. & L.S. *Council.*—Henry Boase, M.D. F.R.S.; Rev. William Buckland, D.D. F.R.S. L.S.; Viscount Cole, M.P. D.C.L. F.R.S.; Charles Giles Bridle Daubeny, M.D. F.R.S. L.S.; Sir P. Grey Egerton, Bart. M.P. F.R.S.; G. B. Greenough, Esq. F.R.S. & L.S.; Leonard Horner, Esq. F.R.S. L. & E.; Robert Hutton, Esq. M.P. M.R.I.A.; Sir Charles Lemon, Bart. M.P. F.R.S.; Marquis of Northampton, F.R.S.; Richard Owen, Esq. F.R.S.; Sir Woodbine Parish, K.C.H. F.R.S.; John Forbes Royle, M.D. F.R.S. & L.S.; T. Weaver, Esq. F.R.S. M.R.I.A.

Address delivered by the REV. WILLIAM WHEWELL, M.A. F.R.S. *President.*

GENTLEMEN,

You have heard in the Reports just read, statements which show that the Society is in a state of healthy progress both in respect to its numbers and its funds. The total number of Fellows of the Society, exclusive of Honorary and Foreign Members, at the close of the year 1836, was 709. At the close of the last year it was 738, the increase being 29, after deducting 18 Members deceased or resigned.

A Part of the Transactions has recently been published, which is worthy of its predecessors in the interest of its matter, and which is not inferior to them in its appearance and illustrations. I believe it will be found that improvements have been introduced, especially in the colouring of the maps.

Our collections have also gone on increasing, and have, as in previous years, derived great additional value from the labour and knowledge bestowed upon them by our excellent Curator. But your Council has found itself compelled to attend to the great, and I may say intolerable amount of labour which has fallen upon Mr. Lonsdale, and certain alterations in the Society's arrangements, directed to the object of remedying this evil, are now in progress or in contemplation. When they are completed I shall have the satisfaction of announcing them to the Society.

The Council have awarded the Wollaston Medal, as you have already been informed, to Mr. Richard Owen, for his general services to Fossil Zoology, and especially for his labours employed upon the fossil mammalia collected by Mr. Darwin in the voyage of Captain Fitz Roy. I need not remind you, Gentlemen, how close are the ties which connect the study of living and of fossil animals; how much light the progress of comparative anatomy throws upon the interpretation of geological characters; and what important steps in our knowledge of the past condition of the earth are restorations of the animal forms which peopled its surface in former times, but have long vanished away. Since the immortal Cuvier breathed into our science a new principle of life, the value of such researches has ever been duly appreciated; and the award of the Wollaston Medal last year is an evidence how gladly your Council take that method of congratulating the successful cultivators of such studies. I am sure that all who are acquainted with Mr. Owen's labours will rejoice that we have in this manner marked our sense of his success. His earlier researches, those for instance on the Nautilus, have been of exceeding use and interest to geologists. And the first part of his description of the fossil mammalia, collected by Mr. Darwin in South America, contains matters of the most striking novelty, interest, and importance. We have there the restoration, performed with a consummate skill, such as fitly marks the worthy successor of Hunter and the disciple of Cuvier, of two animals, not only of new genera, but occupying places in the series of animal forms, which are peculiarly instructive. For the one, the Toxodon, connects the Rodentia with the Pachydermata by manifest links, and with the Cetacea by more remote resemblances; and thus contributes to the completion of the zoological scale just in the parts where it is weakest and most imperfect: while the other animal, the Macrauchenia, the determination of which is considered by anatomists as an admirable example of the solution of such a problem, appears to be exactly intermediate between the horse and the camel. But this creature is also interesting in another way, since it closely resembles, although on a gigantic scale, an animal still existing in that country and peculiar to it, the Llama. Thus, in this as in some other instances, the types of animal forms which distinguish a certain region on the earth's surface are clearly reflected to our eyes as we gaze into the past ages of the earth's history, while yet they are magnified so as to assume what almost appear supernatural dimensions. The Llama, the Capybara, and the Armadillo of South America are seen in colossal forms in the Macrauchenia, the Toxodon, and the Megatherium. I will not omit this occasion of stating that the profound and enlarged speculations on the diffusion, preservation, and extinction of races of animals to which Mr. Darwin has been led by the remains which he has brought home, give great additional value to the treasures which he has collected, and make it proper to offer our congratulations to him, along with Mr. Owen, on the splendid results to which his expedition has led and is likely to lead. Mr. Owen and Mr. Darwin are engaged in the restoration of other animals from the South American remains in their possession, and I am able to announce that two or three other new genera have already been detected. I am sure I am conveying your feeling, Gentlemen, as well as my own, when I express a cordial hope that these two naturalists, so fitted by their endowments and character to advance the progress of science, may long go on achieving new triumphs; and may have the satisfaction—higher even than that which they derive from the honours we so willingly bestow—of finding the great principles which it is given to them to wield, becoming every year more powerful instruments of discovery; and of seeing, as they pursue their researches, light thrown upon the darkest and widest of the vast problems which they have proposed to themselves.

I will now say a few words concerning a few of the most conspicuous of the names which have been obliterated by death from our list during the year.

Among the members of our body, whom we have lost there is one whom we cannot but mention with more than common emotion, endeared as he was to many of us by private friendship, and admired by all for his talents, his knowledge, and his services. Dr. Edward Turner, Professor of Chemistry in the London University, filled the office of our Secretary for five years, and subsequently was two years Vice-President, which situation he held at the time of his death in February 1837. Several of you may remember, Gentlemen, that our last anniversary meeting was in some measure clouded by the recollection of this then recent calamity; and that many of the Fellows of the Society, on that occasion, expressed their intention of testifying their respect and regard for the departed by attending his funeral. Of Dr. Turner's private virtues, and of the charm of his society, I must not here speak. I will not allow myself to dwell upon the admirable clearness and precision of his thoughts as expressed in conversation,—upon the delightful openness and candour of his character,—upon the kind and gentle cheerfulness of his demeanour, the genuine fruit of a deep habitual religious feeling. But I may take this occasion to say, that in him chemistry suffered a loss, not only great,—for that all would at once say,—but much greater and more difficult to repair than may at first sight appear. Dr. Turner entertained a conviction (I am stating the result of many interesting conversations which I have held with him) that the time was come when the chemist could not hope to follow out the fortunes of his science, and to read in her discoveries their full meaning, without being acquainted with the language, and master of the resources of mathematics. Acting upon this enlightened view, he did not hesitate to encounter the great labour and exertion of a course of study in the higher mathematics; and he succeeded entirely in making himself a good mathematician. And he was one of the very few who, in our country, labour at a branch of chemistry which is of the highest importance to us as geologists; but which,—we may suppose from its laborious and intricate nature,—appears to repel our most active chemists; I mean that portion of chemistry which is connected with mineralogy.

Yet this department is, in truth, more inviting than it may at first appear. No doubt in it clear mathematical conceptions are necessary, and perhaps some little training in mathematics; but there is good promise that the labour which this line of investigation demands will be rewarded. I am fully persuaded that there is no portion of the frontier line of our knowledge of which we can so certainly say, "Here we are on the brink of great discoveries." Had Dr. Turner been spared to us some years longer, I know no one who was more likely to have had a principal share in such discoveries. Two papers of his, in the Philosophical Transactions,* show that he was able to deal with the atomic theory in a mode which combines the resources of the skilful analytical chemist with the rigour of the mathematical reasoner; a combination which the right prosecution of that theory requires, but which has not always been found in its cultivators.

Dr. Turner lectured on chemistry at the London University from its first foundation in 1828; he was there surrounded by students, whose affection he gained by his kindness, as well as their admiration by the clearness of his teaching. He also gave a course of lectures on geology, in conjunction with Dr. Grant and Mr. Lindley, each of those gentlemen taking a division of the subject with which he was most familiar. Dr. Turner was snatched from science at the early age of thirty-nine, having been born in the island of Jamaica in 1796. He studied anatomy at Edinburgh, and chemistry at Göttingen, under the able chemist Friedrich Von Stromeyer, to whom he dedicated his Elements of Chemistry; a work which has had, as it well deserves, a very wide circulation among students†.

In William Farish, B.D., Jacksonian Professor of Natural and Experimental Philosophy in the University of Cambridge, the Society has lost an honorary member, elected as such soon after its original foundation, namely in November 1808, and one of a number of our countrymen who were at that period placed upon the honorary list. Professor Farish never employed himself peculiarly in geological pursuits as we now understand the term; but it is to be recollected, that within a few years of the date of his election, which I have mentioned, the investigation of the earth's structure made a rapid progress, and, in consequence, assumed a more fixed and technical form. Professor Farish's scientific studies were mainly directed to the arts, manufactures, and machinery of the empire; on these subjects he delivered courses of lectures full of interest and instruction; and he was thus led to describe our mines, and the mode of working them.

But no reference to particular portions of Professor Farish's labours can convey a just notion of the impulse which he gave to the progress of scientific knowledge within his own sphere of influence, by the habit of seizing, with an active and vivid apprehension, upon prominent parts of modern science, and conveying them, in a manner singularly clear and simple, to his audience. For a long course of years his lectures were more efficacious than any other circumstance in stimulating the minds of men in his university to philosophical thought on physical subjects; and to this day these lectures are never mentioned by those who attended them at that period, without admiration and pleasure. His merit was well recognised by the university in which he spent his life. He received the highest mathematical honours of that body on taking his degree of B.A. in 1778, was elected Professor of Chemistry in 1794, and Jacksonian Professor in 1813; and at the institution of the Cambridge Philosophical Society in Nov. 1819, he was its first president.

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* On the Composition of Chloride of Barium, 1829; Researches on Atomic Weights, 1833. [See Phil. Mag. and Annals, N.S., vol. viii. p. 180; and L. and E. Phil. Mag. vol. i. p. 109; iii. p. 448.—Edit.

† [See also present volume, p. 275.]

I cannot refrain from adding, that although I have here to speak of him principally as a man of science, such pursuits were in his case little more than episodes, in a life the main action of which was directed to the ends of religion and benevolence. In his duties, as a minister of Christianity, he was most zealous and indefatigable; and every attempt to relieve the misery, the ignorance, the unjust restraints of any portion of mankind, found in him a strenuous advocate and ready agent. His childlike simplicity, genuine kindness of heart, and untiring religious earnestness were such as well suited his kindred with Bernard Gilpin, "the Apostle of the North," from whom, through his mother, he derived his descent. He was born at Carlisle in 1759, and died at the age of 78.

Henry Thomas Colebrooke, member of the Supreme Council of Calcutta, was one of those extraordinary men whom our Indian empire has produced; and who show the animating effects of the great scene in which they are there placed, by the variety of subjects to which they extend their attention, and by the vigour with which they combine speculative and practical employment. Mr. Colebrooke went to India as a writer in 1782, and about 1792 began to attend peculiarly to Sanscrit literature. A little later we find him beginning to enrich the Asiatic Researches with a series of memoirs on the religion, the literature, and, above all, the science of the Hindoos. In this department his labours on the Zodiac of the Indians*, and on their notions of the Precession of the Equinoxes and the motions of the Planets†, are highly deserving of notice; as were at a later period the account of the Indian Algebra, given in his translations of the Lilawati and Vijaganita. But Mr. Colebrooke was also ready to contribute a share in sciences with which we are more nearly concerned, He took a lively interest in the correction of errors respecting the physical geography of India, and was one of the first to declare (in 1815) his opinion that the Himalaya mountains were higher than the Andes, an opinion soon afterwards fully confirmed. He also was one of the first to enter upon a subject, to which we may now look with the greatest hope. The first part of vol. i. of our New Series of Transactions (published in 1822) contains two papers by him, one upon the geology of the valley of the Sutledge, which had been explored by Lieut. Gerard; the other upon the north-east of Bengal, where Mr. D. Scott had noticed various rocks, and, among the rest, a deposit which contained fossils, resembling, as he conceived, those of the London clay. I shall have occasion, in the course of this address, to refer to a recent repetition of this observation of an identity between the fossils of the east of India and those of the London and Paris basin. I may observe that these, and other contributions to Indian geology by other writers, contained in the volume of which I spoke, and a preceding one, induced the Secretaries of that time to insert a map, on which the localities of these observations were indicated; and to express in the volume a hope, that these were merely an earnest of the information which might be expected from the activity of British subjects in that quarter*.

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* Asiat. Res., vol. ix.

† Ibid., vol. xii.

Among our foreign members deceased within the year, I regret much to have to mention one, to whom is due, in no small degree, a revolution in the mode of treating the subject of geology, which has taken place in our own times, and the formation of a new branch of geology. This revolution consists in the endeavour, now so familiar to us, to identify geological with recent changes, instead of classifying the great past changes in the surface of the earth which its structure discloses to us, as separate from the newer and slighter modifications of which history and tradition gives us evidence; and the study of the discernible causes of change to which we are thus led, I shall have occasion to speak of under the name of Geological Dynamics. You are well aware that Mr. Lyell is the person who has, with a bold and vigorous hand, moulded the whole scheme of geology upon this idea; but the power which he had of doing this was derived in no small degree from Von Hoff's admirable survey of the evidence of those changes which can be proved by tradition. The extent and universality of the facts thus brought into notice, might well forcibly strike a philosopher already seeking to apply such a principle to geology; and Mr. Lyell has always been forward to acknowledge his obligations to M. Von Hoff. Indeed the idea of such an identification of geological with historical changes was by no means new; it had been both expressed and acted on by Deluc; and must have been present to the minds of those persons who framed the question which gave rise to Von Hoff's book. This question was proposed in 1818 by the Royal Academy of Science of Göttingen. "Considering," they said, "that we have, in the crust of the earth, evidence of great revolutions, which have happened at different times, in different portions, and of which the period and duration are unknown, we are led to ask whether certain more partial alterations may not lie within the domain of tradition, and give us the means of knowing at what period they took place, and what time the formation of certain portions of the earth's crust required; whereby some light may be thrown on those changes which lie beyond the limits of history."

M. Von Hoff's work,—"The history of those natural changes in the earth's surface which are proved by tradition"—appeared (the first part) in 1822, and had the Academy's prize assigned it. This part of the work contained an account of the changes due to the agency of water; and by the wide range of reading and study which it included, and the philosophical manner in which its copious materials were arranged, well justified the distinction which it received. The view presented in it of the great changes which have gone on from the beginning of historical times,—the yielding or advancing of coasts, the disappearing of islands, the union of seas,—appear to give a new face to the globe. But the portion of the judgement of the Academy which the author most valued was, that in which they said that he had used the sources of his information *conscientiously*. In 1824 appeared the second part, containing the history of volcanos and earthquakes; and, although the previous labours of Humboldt and Von Buch had done much to connect and generalise facts of this kind, Von Hoff's labours were an important step: "At least," he himself says, "he was not aware that any one before him had endeavoured to combine so large a mass of facts with the general ideas of the natural philosopher, so as to form a whole." Among other large views, we may see much which, as to kind of change supposed, agrees with the opinions of Mr. Darwin, of which I shall have to speak; for instance, Von Hoff conceives that the island of Otaheite is undergoing a gradual elevation out of the sea.* Finally, the third volume of this work appeared after an interval of ten years, in 1834; in which he considers other causes of change; as rising and sinking of the land; alterations of rivers and seas; the operations of snow and ice; and also the geological results to which the whole survey had led him. In this volume he expresses his pleasure at the appearance of Mr. Lyell's work, which had taken place in the intervening period, and by which he had found much new light thrown upon his own speculations.

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* [Another notice of Mr. Colebrooke has appeared in our present volume, p. 272.—Edit.]

In the interval of time between the publication of the second and third volumes, M. Von Hoff published "Geological Observations on Carlsbad," (1825) and "Measures of Heights in and near Thuringia" (1833). In this last work he not only gave a great number of his own barometrical measurements, but discussed all extant measures of the heights of points in Thuringia, to the amount of above 1100. He also employed himself in meteorological observations.

Karl Ernest Adolph Von Hoff, Knight of the order of the White Falcon, and invested with several offices of honour and dignity at the Ducal Court of Gotha, died at Gotha the 24th of May last. He was 66 years of age, having been born in the same city Nov. 1, 1771.

Besides the history I have mentioned, which must always continue to be a classical work on the subject of which it treats, he was at the time of his death employed in compiling a continuation of his Notices of Earthquakes and Volcanic Eruptions; and also a new work, which was considered to be an important one, and was to be entitled "Germany according to its Natural Conditions and Political Relations."

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* Part II. Pref. p. xiv.

[To be continued.]

ZOOLOGICAL SOCIETY.

[Continued from p. 216.]

January 24, 1837 (*continued*).—Mr. Martin described a species of *Fox* brought by Mr. Darwin from the island of Chiloe, respecting which he made some remarks which will be found in No. xlix. of the Proceedings. He characterized it under the specific title *fulvipes**.

The Secretary read a communication from J. O. Westwood, Esq., describing several new species of Insects belonging to the family of the *Sacred Beetles*.

After noticing the interest which is attached to the family of the *Scarabæidæ*, not only on account of their curious habits, whence they were raised to the rank of objects of worship by the Egyptians, but also from having led to the publication of the *Horæ Entomologicæ* by Mr. MacLeay, in which an analysis of the Linnæan *Scarabæi* was given; the author gives an abstract of the classifications of this family respectively proposed by MacLeay, Latreille, (*Règne An.*, 2nd edition), and Serville and Saint Fargeau (*Encyclop. Méthod.* vol. x.), with a notice of the genera more recently proposed by various authors referrible or allied thereto. From a review of these distributions in conjunction with the natural economy of the insects of which the family is composed, the author is disposed to consider the family as divisible into two natural groups, those with long hind legs and those which have their legs short and conical; and also that the characters of the genus *Scarabæus* and subgenus *Heliocantharus* must either be modified so as to exclude the species which are destitute of a distinct spur at the extremity of the intermediate *tibiæ*, or that the *Ateuchus Adamastor* (*Enc. Méth.*) and the insects subsequently described must be regarded as referrible to the genus *Scarabæus*, although possessing two spurs at the extremity of the intermediate *tibiæ*, agreeing in all other material respects with the true *Scarabæi*.

The following is an abstract of the characters of the insects, the descriptions of which were accompanied by figures exhibiting the various essential organs in detail, and by observations upon the structural peculiarities of the two groups.

Typus SCELIAGES.

Corpus latum, subdepressum. *Caput* subtrigonum clypeo trilobato, lobo intermedio valdè emarginato. *Antennæ* clavâ subglobosâ, articulo 7^{mo} magno infernè producto, articulos duos terminales in sinu ejus includente, ultimo 8vo minori. *Palpi* maxillares breves subfiliformes, labiales abbreviati 3-articulati, articulis magnitudine decrescentibus. *Thorax* abdomine paullo latior. *Tibiæ* anticæ magnæ, pone medium intus curvatæ. *Tibiæ* intermediæ bicalcaratæ.

Species, SCELIAGES *Iopas*, from South Africa.

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* In order to avoid repetition we will here state that in all cases in which new species or groups are stated to have been *characterized*, in our reports of the meetings of the Zoological Society, their *characters* will be found in the "Proceedings" of the Society for the meeting in question.—EDIT.

Typus Anomiopsis.

Pedes elongati, *tibiæ* intermediæ curvatæ bicalcaratæ, calcaribus mobilibus interno, elongato acuto, externo breviori spatuliformi, *tarsi* pedum anticorum obsoleti, quatuor posticorum depressi setosi, unguibus nullis; *palpi* maxillares filiformibus, articulis tribus ultimis longitudine fere æqualibus; labiales difformes, articulo 2do maximo transverso-ovato, ultimo minutissimo internè et obliquè inserto.

Species, Anomiopsis *Dioscorides*, from Patagonia, and *Aniom. Sterquilinus*, of unknown locality.

Mr. Martin called the attention of the meeting to a specimen of the *Dasypus hybridus*, in the collection presented to the Society by C. Darwin, Esq. This animal, the *tatou mulet* of Azara, has been characterised in all systematic works, as closely related to *Dasypus Peba*, and as having large ears; whereas the ears are much smaller than in *D. Peba*, and but little larger than those of *D. minutus*. In reference to this species, which he at first was unable satisfactorily to identify, he observed that the vague and unsatisfactory account given in systematic works would, he conceived, justify him in laying before the meeting a more complete and definite description of the animal than he had been able to meet with, the want of which he had himself experienced, which he thus ventured to supply. The description appears in No. xlix. of the Proceedings.

Mr. James Reid exhibited to the Meeting, and characterized as new, under the name of *Obscurus*, a dark-coloured monkey, from the Society's collection, belonging to the genus *Semnopithecus*. The locality of the particular specimen before the Meeting was unknown.

February 14, 1837.—A letter was read from C. R. Read, Esq., a corresponding member, dated Singapore, September 2nd, 1836, announcing a present of 56 skins of birds, and the skin of an alligator of large size, which have been received.

At the request of the Chairman, Mr. Waterhouse brought under the notice of the Meeting numerous species of the genus *Mus*, forming part of the collection presented to this Society by Charles Darwin, Esq., a Corresponding Member. The specimens placed on the table had been collected at various parts of the Southern Coast of South America, viz. Coquimbo, Valparaiso, Port Desire, Maldonado, Bahia Blanca, &c.

Most of these numerous species were considered by Mr. Waterhouse as hitherto undescribed, and drawings were exhibited by him illustrative of the modifications observable in their dentition.

The characters, dimensions, and particular habitats of the species above referred to are given in No. l. of the Society's Proceedings, under the following specific names; viz.

Mus *tumidus*, *nasutus*, *obscurus*, *longipilis*, *olivaceus*, *micropus*, *brachyotis*, *xanthorhinus*, *canescens*, *arenicola*, *bimaculatus*, *elegans*, *gracilipes*, *flavescens*, *brevirostris*, and *Maurus*.

After giving the characters, &c., of the above new species of *Mus*, Mr. Waterhouse proceeds as follows:

"Though in the foregoing description I have retained the generic title *Mus*, I have here to state that the above species naturally divide themselves into several subordinate groups, the characters of which are sufficiently evident, not only between themselves, but also between each group and that to which the term *Mus* ought, I conceive, to be restricted, and of which our common mouse (*Mus musculus*) may be regarded as the type. To these groups I shall here assign subgeneric titles, and at the same time point out their chief distinguishing characters without entering into any minute details respecting them, as I shall shortly have an opportunity of illustrating my views by means of drawings both of the teeth and of the animals, without which it is impossible to convey a clear idea of the subject."

Subgenus 1. SCAPTEROMYS[*].

Molars with enamel deeply indented in the crown. In the front molar of the lower jaw the enamel is indented twice on the outer margin and three times on the inner; in the second molar the enamel is indented once on the outer margin and twice on the inner; and in the last molar once on the outer, and twice on the inner. Fur long and soft. Tail moderate, well clothed with hair. Claws long, but slightly curved and formed for burrowing. Fore-feet moderately large. Thumb furnished with a distinct claw. Ears moderate, well clothed with hairs.

Species *Mus* (*Scapteromys*) *tumidus*.

Subgenus 2. OXYMYCTERUS[†].

Molars with the folds of enamel penetrating deeply into the body of the tooth. Front molar of the lower jaw with three indentations on the inner side and two on the outer; second molar with two on the outer side and the same number on the inner; the last molar with one indentation of the enamel on each side. Fur long and soft. Claws long, but slightly curved, and formed for burrowing. A distinct claw on the thumb. Tail short, moderately furnished with hair. Nose much elongated and pointed.

Species *Mus* (*Oxymycterus*) *nasutus*.

Subgenus 3. ABROTHRIX[‡].

Folds of enamel penetrating deeply into the sides of the molars. The front molar of the lower jaw has three folds of enamel on the inner side and two on the outer; the second molar has two on the inner side and one on the outer; and the last molar has one on each side. Fur long and soft. Tail short, well furnished with hair. Thumb with a short rounded nail. Ears well furnished with hair.

Type *Mus* (*Abrothrix*) *longipilis*.
Species 2 *Mus* (*Ab.*) *obscurus*.
——— 3 ——— *olivaceus*.
——— 4 ——— *micropus*.
——— 5 ——— *brachyotis*.
——— 6 ——— *xanthorhinus*.
——— 7 ——— *canescens*.
——— 8 ——— *arenicola*.

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[*] *Scapteromys*, from Σκαπτηρ, a digger, and Μυς.

[†] *Oxymycterus*, from Οξυς, sharp, and Μυκτηρ, nose.

[‡] *Abrothrix*, from Ἁβρος, soft or delicate, and Θριξ, hair.

In general appearance these animals resemble *Arvicolæ*.

Subgenus 4. Calomys*.

Fur moderate, soft. *Tarsus* almost entirely clothed beneath with hair. Front molar with three indentations of enamel on the inner side and two on the outer; second molar with two on the inner and two on the outer; and the last molar with one on each side.

Type *Mus* (*Calomys*) *bimaculatus*.
Species 2 *Mus* (*Cal.*) *elegans*.
——— 3 ———— *gracilipes*.

Mus maurus and *M. brevirostris* I regard as belonging to the restricted genus *Mus*. In *Mus flavescens* the dentition differs *slightly* from that of the ordinary mice.

Mr. Gould exhibited, in continuation, the *Fissirostral Birds* of Mr. Darwin's collection recently presented to the Society, and characterized from among them the following new species: viz. Caprimulgus *bifasciatus*, and *Caprim. parvulus*; Hirundo *frontalis* and *Hirund. Concolor*; and Halcyon *Erythrorhynchus*: the characters, dimensions, and habits of all which will be found in No. I. ol Society's Proceedings.

February 28, 1837.—A notice by T. C. Eyton, Esq. of some osteological peculiarities in different skeletons of the genus *Sus* was read; which has appeared in No. I. of the Proceedings.

A letter was read from Thomas Keir Short, Esq., dated Launceston, Van Diemen's Land, August 10th, 1836, containing some remarks upon the *Apteryx*, two living specimens of which had been seen by the writer. The general correctness of the description published by Mr. Yarrell† of this bird is confirmed by the observations of Mr. Short, with the exception of its progressive powers, which are stated to be remarkably great. The natives employ two methods of capturing it; one by hunting it down with very swift dogs, the other by imitating its call at night, and when by this means the bird is decoyed within a short distance, it is suddenly exposed to a strong light, which so confuses it that it is then readily taken. The usual position is standing, with the head drawn back between the shoulders, and the bill pointing to the ground. The food is stated to be principally worms and insects, and these birds are strictly nocturnal in their habits, feeding only during the night. Mr. Short remarks, that he has not been able to learn the place in which the *Apteryx* builds its nest, or the number of eggs which it lays. In conclusion, he promises to use his utmost endeavours to procure specimens for the Society.

Mr. Gould resumed the exhibition of his collection of Australian Birds, as also several species, from the same country, forming portions of the collections of the United Service Museum, and of King's College, London. Among his own birds Mr. Gould characterized two new species of *Meliphagidæ*, constituting a subdivision of that family, including *Meliphaga tenuirostris* of authors. For this new group he proposed the generic title of *Acanthorhynchus*, and for the two new species the names of *A. superciliosus* and *A. dubius*.

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* *Calomys*, from Καλος, beautiful, and Μυς.
† See Lond. and Edinb. Phil. Mag. vol. iii. p. 299.

ACANTHORHYNCHUS. (Gen. char.) *Rostrum* elongatum gracile et acutum ; ad latera compressum ; tomiis incurvatis ; culmine acuto et elevato.

Nares basales elongatæ et operculo tectæ.

Lingua ut in Gen. *Meliphaga*.

Alæ mediocres et sub-rotundatæ, remigibus primis et quintis ferè æqualibus ; tertiis et quartis intensè æqualibus et longissimis.

Cauda mediocris, et paululùm furcata.

Tarsi elongati, fortes ; halluce digito medio longiore et robustiore ; digito externo medium superante.

Ungues curvati.

Typus, *Certhia tenuirostris*, auct.

The following species, also in Mr. Gould's collection, were named and characterized ; viz.

PARDALOTUS *affinis*, NANODES *elegans*, PLATYCERCUS *flaveolus*, and HIMANTOPUS *leucocephalus*.

Mr. Gould also characterized under the following names two new species of the genus *Sterna*, from the collection in King's College, a species of *Cormorant* in the United Service Museum, and three species of the genus *Orpheus*, from the Galapagos, in the collection of Mr. Darwin.

STERNA *poliocerca* and STERN. *macrotarsa* ; PHALACROCORAX *brevirostris* ; and ORPHEUS *trifasciatus*, *O. melanotis* and *O. parvulus*.

Mr. Waterhouse resumed the exhibition of the small *Rodents*, belonging to the collection presented by Mr. Darwin to the Society. Among them were three species allied to the genus *Mus*, but offering some slight modification, not only in the external form, but in the structure of the teeth. They have the fur soft and silky ; the head large, and the fore legs very small and delicate ; the *tarsus* moderately long and bare beneath ; in the number and proportion of the toes they agree with the true rats ; the tail is moderately long, and more thickly clothed with hair than in the typical rats. The ears are large, and clothed with hair. Like the true rats, they have twelve rooted molars ; the folds of enamel, however, penetrate more deeply into the body of each tooth, and enter in such a way that the crowns of the teeth are divided into transverse and somewhat lozenge-shaped lobes, or in some instances into lobes of a triangular form. In the front molar of the upper jaw the enamel enters the body of the tooth twice, both on the outer and inner sides ; and in the second and posterior molars, both of the upper and under jaws, the enamel penetrates but once externally and internally in each. In the front molar of the lower jaw the enamel enters the body of the tooth three times internally, and twice externally.

As the above-mentioned characters, in Mr. Waterhouse's opinion, evidently indicated an aberrant form of the Muridæ, he suggested the propriety of constituting a subgenus under the name of Phyllotis* for the reception of the species.

They were characterized as Mus (Phyllotis) *Darwinii*, *xanthopygus*, and *griseo-flavus*. For their characters and dimensions, &c. see Proceedings.

Two species of small Rodents were next characterized as constituting examples of a new genus, for which Mr. Waterhouse proposed the name of

REITHRODON.†

Dentes primores $\frac{2}{2}$; inferioribus acutis, gracilibus, et anticè lævibus; superioribus gracilibus, anticè longitudinalitèr sulcatis.

Molares utrinque $\frac{3}{3}$ radicati; primo maximo, ultimo minimo: primo superiore plicas vitreas duas externè et internè alternatìm exhibente; secundo, et tertio, plicas duas externè, internè unam: primo inferiore plicas vitreas tres externè, duas internè; secundo, plicas duas externè, unam internè; tertio unam externè et internè, exhibentibus.

Artus inæquales: *antipedes* 4-dactyli, cum pollice exiguo unguiculato: *pedes postici* 5-dactyli, digitis externis et internis brevissimis.

Ungues parvuli et debiles. *Tarsi* subtùs pilosi.

Cauda mediocris, pilis brevibus adpressis instructa.

Caput magnum, fronte convexo: oculis magnis: auribus mediocribus.

"In the present genus, the incisors, compared with those of the true rats, are rather smaller in proportion, and those of the upper jaw also differ in having a longitudinal groove, a character which exists in *Euryotis* (Brants), *Gerbillus*, *Otomys* (Smith), *Dendromys*, and some other genera, but not combined with molars similar in structure to those above described, nor yet with similar external characters. In other respects the incisors resemble those of the genus *Mus*; that is to say, those of the lower jaw are long, slender, and pointed, and those of the upper are deep from front to back, and somewhat flattened at the sides and in front. The molars gradually decrease in size from the front to the last posterior tooth. The folds of enamel penetrate deeply into the crowns of these teeth, so that those from one side are in contact with those of the other; these folds of enamel are each nearly opposed to the salient angles of the opposite side.

"In the two species of this genus with which I am acquainted the fur is long, very soft, and consists of hairs of two lengths. The arched form of the head and the large eyes produce in these animals a slight resemblance to young rabbits; their affinity, however, is with the *Muridæ*."

Mr. Waterhouse then gave the characters of Reithrodon *typicus* and Reith. *cuniculoïdes*.

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* *Phyllotis*, from Φυλλον, a leaf, and Ους, ωτος, an ear.

† Ρειθρον, a channel; Οδον, a tooth.

In conclusion, two other new Rodents were characterized under the generic name of

ABROCOMA.*

Dentes primores $\frac{2}{2}$ acuti, eradicati, anticè læves: *molares* utrinque $\frac{4}{4}$ subæquales, illis maxillæ superioris in areas duas transversales ob plicas vitreas acutè indentatas divisis; plicis utriusque lateris vix æquè profundis; illis mandibulæ inferioris in tres partes divisis, plicis vitreis bis釋 internè, semel externè indentatis, areâ primâ sagittæ cuspidem fingente, cæteris acutè triangularibus.

Artus subæquales.

Antipedes 4-dactyli, externo brevissimo, intermediis longissimis et ferè æqualibus.

Pedes postici 5-dactyli; digito interno brevissimo. *Ungues* breves et debiles, illo digiti secundi lato et lamellari; omnibus setis rigidis obtectis.

Caput mediocre, auribus magnis, membranaceis; oculis mediocribus.

Cauda breviuscula.

Vellus perlongum, et molle.

"The genus Abrocoma is evidently allied on the one hand to *Octodon*, *Ctenomys*, and *Pœphagomys*, and it appears to me almost as evidently allied on the other hand to the *Chinchillidæ*. The dentition, however, differs considerably from either of the above-mentioned genera, or from either of those of the family *Chinchillidæ*, and in fact indicates a new generic form†. From *Ctenomys* and *Pœphagomys* the present genus is readily distinguished, by the comparatively large size of the ears, the small delicate claws, and smaller size of the incisors; and from *Octodon* by the uniform length of the hairs on the tail.

"In the structure of the feet the genus *Abrocoma* approaches very nearly to *Octodon*, not only in the form but in having the soles both of the fore and hind feet (which are devoid of hair) covered with minute round fleshy tubercles. In *Octodon*, however, the toes have on their under side transverse incisions as observed in the *Muridæ*, a character, however, not found in *Abrocoma*; here the under side of the toes is, like the sole of the foot, covered with tubercles.

"The extreme softness of the fur of the animals about to be described, suggested for them the generic name of *Abrocoma*. The fur consists of hairs of two lengths, and the longer hairs are so extremely slender that they might almost be compared to the web of the spider. The specific names (*Abrocoma Bennettii* and *A. Cuvieri*, applied are those of the distinguished naturalists who first made us acquainted with the two genera *Octodon* and *Pœphagomys*, these being very nearly allied to *Abrocoma*."

March 14, 1837.—A paper was read, "On the habits of the *Vultur aura*," by Mr. W. Sells, with notes of dissections of the heads of two specimens, by Mr. R. Owen.

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* ῾Αβρος, soft; Κομη, hair.

† "I may here mention that the folds of enamel in the dentition of the lower jaw very much resemble those in the teeth of the genus *Arvicola*."

The writer states that this bird is found in great abundance in the Island of Jamaica, where it is known by the name of *John Crow*; and so valuable are its services in the removal of carrion and animal filth, that the legislature have imposed a fine of £5 upon any one destroying it within a stated distance of the principal towns. Its ordinary food is carrion, but when hard pressed with hunger it will seize upon young fowls, rats, and snakes. After noticing the highly offensive odour emitted from the eggs of this bird when broken, Mr. Sells relates the following instances which have come under his own personal observation, for the purpose of proving, that the *Vultur aura* possesses the sense of smell in a very acute degree.

"It has been questioned whether the vulture discovers its food by means of the organ of smell or that of sight. I apprehend that its powers of vision are very considerable, and of most important use to the bird in that point of view; but that it is principally from highly organized olfactories that it so speedily receives intelligence of where the savory morsel is to be found will plainly appear by the following facts. In hot climates the burial of the dead commonly takes place in about twenty-four hours after death, and that necessarily, so rapidly does decomposition take place. On one occasion I had to make a post-mortem examination of a body within twenty hours after death, in a mill-house, completely concealed, and while so engaged the roof of the mill-house was thickly studded with these birds. Another instance was that of an old patient and much-valued friend who died at midnight: the family had to send for necessaries for the funeral to Spanish Town, distant thirty miles, so that the interment could not take place until noon of the second day, or thirty-six hours after his decease, long before which time, and a most painful sight it was, the ridge of the shingled roof of his house, a large mansion of but one floor, had a number of these melancholy-looking heralds of death perched thereon, beside many more which had settled in trees in its immediate vicinity. In these cases the birds must have been directed by smell alone as sight was totally out of the question.

"In opposition to the above opinion, it has been stated by Mr. Audubon that vultures and other birds of prey possess the sense of smell in a very inferior degree to carnivorous quadrupeds, and that so far from guiding them to their prey from a distance, it affords them no indication of its presence, even when close at hand. In confirmation of this opinion he relates that he stuffed the skin of a deer full of hay and placed it in a field; in a few minutes a vulture alighted near it and directly proceeded to attack it, but finding no eatable food he at length quitted it. And he further relates that a dead dog was concealed in a narrow ravine twenty feet below the surface of the earth around it and filled with briers and high canes; that many vultures were seen sailing in all directions over the spot but none discovered it. I may remark upon the above experiments that in the first case the stag was doubtless *seen* by the birds, but it does not follow that they might not also have smelt the hide, although inodorous to the human nose; in the second case, the birds had undoubtedly been attracted by *smell*, however embarrassed they might have been by the concealment of the object which caused it. I have in many hundred instances seen the vulture feeding upon small objects under rocks, bushes, and in other situations where it was utterly impossible that the bird could have discovered it but through the sense of smell; and we are to recollect that the habit of the vulture is that of soaring aloft in the air, and not that of foraging upon the ground."

Mr. Sells's communication was accompanied by the following letter from Mr. Owen, addressed to the Secretary, W. Yarrell, Esq.

"Dear Sir,—I received the heads of the *John Crow*, which I suppose to be the *Vultur aura* or *Turkey Buzzard*, and have dissected the olfactory nerves in both; as also in a *Turkey* which seemed to me to be a good subject for comparison, being of the same size, and one in which the olfactory sense may be supposed to be as low as in the *Vulture*, on the supposition that this bird is as independent of assistance from smell in finding his food as the experiments of Audubon appear to show. There is, however, a striking difference between the *Turkey Vulture* and the *Turkey* in this part of their organization. The olfactory nerves in the *Vulture* arise by two oval ganglions at the anterior apices of the hemispheres from which they are continued 1½ line in transverse diameter, and 2 lines in vertical diameter, and are distributed over well-developed superior and middle spongy bones, the latter being twice the dimensions of the former. The nose is also supplied by a large division of the supraorbital branch of the 5th pair, which ascends from the orbit, passes into the nose crossing obliquely over the outer side of the olfactory nerve, extending between the superior spongy bone and the membrane covering the middle spongy bone, then descending, and after supplying the inferior and anterior spongy bone escaping from the nasal cavity to supply the parts covering the upper mandible. This olfactory branch of the 5th pair is about ¼th the size of the true olfactory nerve.

"In the *Turkey* the olfactory branch of the 5th nerve is about the same size as in the *Vulture*, and is superior in size to the true olfactory nerve, which is only about ⅓th the size of that in the *Vulture*. The olfactory nerve does not form a ganglion at its commencement, but is continued as a small round chord from the anterior apex of each hemisphere, and is ramified on a small middle spongy bone, there being no extension of the pituitary membrane over a superior turbinated bone as in the *Vulture*. Indeed the difference in the development of the nasal cavity is well marked in the different forms of the head in these two species. In the *Vulture* there is a space between the upper parts of the orbits in which the olfactory ganglions and nerves are situated, and the nasal cavity anterior to these is of a much greater breadth and also longer, as well as exhibiting internally a greater extent of pituitary surface, than in the *Turkey*. In this bird the olfactory nerves are compressed within a narrow interorbital space, which would not admit of the lodgement of ganglions; the olfactory nerves after passing through this space then diverge to the nasal cavity.

"In the *Goose* the olfactory nerves are developed to the same size as in the *Vulture*, and expand upon superior spongy bones of similar form, but placed wider apart, and these supply the middle spongy bones which are longer but not so broad as in the *Turkey*. The olfactory branch of the 5th pair is double the size of that in the *Vulture* or *Turkey*; it gives, however, not a greater proportion of filament to the nose than in those birds, but is mainly expended upon the membrane covering the upper mandible.

"The above notes show that the *Vulture* has a well-developed organ of smell, but whether he finds his prey by that sense alone, or in what degree it assists, anatomy is not so well calculated to explain as experiment.

"I will bring my preparations showing the above at next meeting, and am truly yours,

"Royal College of Surgeons, March 7th." "R. Owen."

Mr. Gould brought before the notice of the meeting, from the collection of Mr. Darwin, a new species of *Rhea* from Patagonia, and after offering some observations upon the distribution of the *Struthionidæ*, and upon the great interest attending this addition to that family, he remarked that the new species is distinguished from *Rhea Americana* of authors, in being one-fifth less in size, in having the *bill* shorter than the head, and the *tarsi* reticulated in front instead of scutellated, and in being plumed below the knee for several inches. It has also a more densely plumed wing, the feathers of which are broader, and all terminated by a band of white.

Mr. Gould, in conclusion, adverted to the important accessions to science resulting from the exertions of Mr. Darwin, and to his liberality in presenting the Society with his valuable Zoological Collection; to commemorate which he proposed to designate this interesting species by the name of *Rhea Darwinii*.

Mr. Darwin then read some notes upon the *Rhea Americana*, and upon the newly described species, but principally referring to the former.

This bird abounds over the plains of Northern Patagonia and the United Provinces of La Plata; and though fleet in its paces and shy in its nature, it yet falls an easy prey to the hunters, who confound it by approaching on horseback in a semicircle. When pursued it generally prefers running against the wind, expanding its wings to the full extent. It is not generally known that the *Rhea* is in the habit of swimming, but on two occasions Mr. Darwin witnessed their crossing the Santa Cruz river, where its course was about 400 yards wide and the stream rapid. They make but slow progress, their necks are extended slightly forwards, but little of the body appears above water. At Bahia Blanca, in the months of October and September, an extraordinary number of eggs are found all over the country. The eggs either lie scattered about, or are collected together in a shallow excavation or nest; in the former case they are never hatched, and are termed by the Spaniards *Huachos*. The Gauchos unanimously affirm that the male bird alone hatches the eggs, and for some time afterwards accompanies the young. Mr. Darwin does not doubt the accuracy of this fact, and states that the cock bird sits so closely that he has almost ridden over one in the nest. Mr. Darwin has also been positively informed that several females lay in one nest, and although the fact at first appears strange, he considers the cause sufficiently obvious, for as the number of eggs varies from 20 to 50, and, according to Azara, even 70 or 80, if each hen were obliged to hatch her own before the last was laid, the first probably would have been addled; but if each laid a few eggs at successive periods in different nests, and several hens, as is stated to be the case, combine together, then the eggs in one collection would be nearly of the same age. Mr. Burchell mentions that in Africa two ostriches are believed to lay in one nest.

Mr. Darwin then proceeds to notice the other species of *Rhea*, which he first heard described by the Gauchos, at River Negro, in Northern Patagonia, as a very rare bird, under the name of *Avestruz Petise*. The eggs were smaller than those of the common *Rhea*, of more elongated form, and with a tinge of pale blue. This species is tolerably abundant about a degree and a half south of the Rio Negro, and the specimen presented to the Society was shot by Mr. Martens at Port Desire in Patagonia, (in latitude 48). It does not expand its wings when running at full speed, and Mr. Darwin learned from a Patagonian Indian that the nest contains fifteen eggs, which are deposited by more than one female. It is stated in conclusion that the *Rhea Americana* inhabits the country of La Plata as far as a little south of the Rio Negro, in lat. 41°, and that the *Petise* takes its place in Southern Patagonia.

Mr. Chambers then brought before the notice of the Society a simple process for taking impressions from feathers, which is effected by placing the feathers between two sheets of paper, the lower one being previously well damped, and the upper covered with printers' ink; both are then passed through the rolling press of a copper plate printer, and on removing the upper sheet perfect figures of the feathers will be left, which may be coloured when dry, and will then have the resemblance of feathers placed on paper.

FRIDAY EVENING MEETINGS AT THE ROYAL INSTITUTION.

Jan. 19, 1838.—Mr. Faraday on Electrical Induction.

Jan. 26.—Mr. Brande on the nature of fatty bodies, and on the application of stearine to the manufacture of candles.

Feb. 2.—Mr. Goadby on the skeleton of insects.

Feb. 9.—Mr. Gray on the formation and structure of shells.

Feb. 16.—Dr. Ainsworth on the progress of the alluviums of Babylonia.

Feb. 23.—Mr. Faraday on the atmosphere of this and other planets.

March 2.—Mr. Carpmael on the manufacture of welded iron tubing.

March 9.—Mr. Griffiths on the philosophy and manufacture of the various means for obtaining instantaneous lights.

March 16.—Mr. Pereira on the relation between the external form and the optical and other characters of crystals.

March 23.—Mr. Cowper on the manufacture of lace by machinery.

March 30.—Dr. Grant on the metamorphosis of the Amphibia.

April 6.—Mr. Faraday on Mr. Ward's plan of preserving and growing plants in closed vessels and places.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

A meeting of this Society was held on Monday evening, February 26th, the Rev. L. Jenyns, Vice-President, in the chair. Various presents of books were announced, and the following papers were read:—On some new genera of fossil multilocular shells in the slate rocks of Cornwall, by Mr. Ansted, of Jesus College; On a question in the Theory of Probabilities, by Mr. De Morgan; On the Quadrature of the Circle, by Dr. Cresswell.

A meeting of this Society was held on Monday evening, March 12th, the Rev. the Master of Christ's College, the President, being in the chair. Mr. Kelland, of Queen's College, read the first part of a paper On Molecular Attraction. Afterwards Professor Henslow gave an account of the plants brought by Mr. Darwin from the Keeling Islands. These are coral islets of recent formation, lying to the south of Sumatra. They are of the form called lagoon islands, the average height of the land above the water not being more than six feet. These islands have only recently been inhabited by man. The indigenous vegetable species from them are 24 in number, and Mr. Darwin has brought home 22 of these, belonging to 21 genera, and 18 different families.

March 26th.—The Rev. Dr. Graham, the President, in the chair. Professor Challis read a paper On the Proper Motions of the Stars. Mr. Airy read the termination of a paper "On the Intensity of Light in the neighbourhood of a Caustic," of which the following is an abstract. Taking V to represent the length of the path from a source of light to *any point* of a reflecting surface (or, *mutatis mutandis*, of a refracting surface), and thence to a point at which the intensity of light is to be estimated, and putting x for the ordinate of the point of the reflecting surface, $\frac{d(V)}{dx}$ has a finite value at all points, except when the point whose ordinate is x is the same with the point which, on the ordinary laws of reflexion, would reflect light to the point under consideration; for that point $\frac{d(V)}{dx} = 0$. From this the author deduced that, if the point under consideration were a conjugate focus, receiving the rays reflected from the whole surface, V must be constant, or the whole series of differential coefficients must vanish; but if the point under consideration is the focus only for a very small pencil reflected from the point whose ordinate is x, and from neighbouring points, then $\frac{d(V)}{dx} = 0$, and $\frac{d^2(V)}{dx^2} = 0$, without any condition for the remaining differential coefficients. From the nature of the caustic it is evident that these equations must apply to every point of the caustic, provided that x have the value corresponding to the point of reflection on the ordinary law; but it may be shown also that, in general, $\frac{d^3(V)}{dx^3}$ is finite, and admits of being expressed in terms of the radius of curvature of the caustic and other lines. Having found therefore, that in the caustic $\frac{d(V)}{dx} = 0$, $\frac{d^2(V)}{dx^2} = 0$, $\frac{d^3(V)}{dx^3} = C$, the proper value being given to x, the author infers that, for a point at the distance δp from the caustic, $\frac{d(V)}{dx}$ will $= A . \delta p$, $\frac{d^2(V)}{dx^2} = B\,\delta p$, $\frac{d^3(V)}{dx^3} = C + D . \delta p$. The values of A and C are easily found. Consequently the value of V, for a point at the distance δp from the caustic, when measured through a point of the reflecting surface, whose ordinate is $x + z$, is of the form

$$V' + A . \delta p . \frac{z}{1} + B\,\delta p . \frac{z^2}{1.2} + (C + D . \delta p)\frac{z^3}{1.2.3}.$$

Rejecting the unimportant parts of the coefficients, and altering z so as to take away the second power, this becomes $V' + A\,\delta p . z' + \frac{C}{6} z'^3$: the expression for the disturbance of ether is

$$\int_{z'} \sin \frac{2\pi}{\lambda}\left\{vt - V' - A\,\delta p . z' - \frac{C}{6} z'^3\right\}$$

which, observing that $\int_{z'} \sin \frac{2\pi}{\lambda}\left(A\,\delta p . z' + \frac{C}{6} z'^3\right)$ between − infinity and + infinity = 0, may in all cases be shown to be proportional to $\sin \frac{2\pi}{\lambda}\left(vt - V\right) \times \int_w \cos \frac{\pi}{2}\left(w^3 - m . w\right)$ the integral being taken from $w = 0$ to $w =$ infinity, and m being expressed in terms of A, C, δp, and λ. Putting S for the definite integral from $w = 0$ to $w =$ infinity, the intensity of light therefore is proportional to

$$\left[S^w \cos \frac{\pi}{2}\,(w^3 - m . w)\right]^2.$$

The author then considers especially the case of the rainbow (included in the general case of the deviation of rays having a maximum or minimum), and shows that it depends on the same expression.

An account was then given of the way in which the value of the definite integral had been found for forty-one different values of m (beginning with $m = -4{\cdot}0$, $-3{\cdot}8$, &c. and ending with $m = +4{\cdot}0$). As far as $w = 2{\cdot}0$, it was found by summation; after that, by series. The series possessed the property of diverging indefinitely after some assignable term, yet of having a sum always finite. The process was one of considerable labour.

From the result of these calculations it appeared that the place of greatest intensity was not at the caustic but on its convex side, or (for the rainbow) within the primary bow: that the intensity was no where infinite; that it diminished most rapidly on the concave side of the caustic; that on the convex side, after having increased to a maximum, it diminished to 0, and then increased to a second maximum, whose value was about $\frac{3}{5}$ths of the first. The calculations did not extend to the next evanescence of light. The following rule was given for ascertaining the place of the geometrical rainbow (on the theory of emission): measure the distance between the bright bow and the first spurious bow; the geometrical bow is exterior to the bright bow by $\frac{11}{24}$ths of this quantity.

LXXI. *Intelligence and Miscellaneous Articles.*

GRESHAM COLLEGE.

[As some of our correspondents have formerly endeavoured to call attention to the subject of Gresham College, we have given the following brief notes of Professor Pullen's admirable Lectures, and have much satisfaction in pointing out to those who are desirous of attending to astronomical and physical studies a means of improvement which is freely open to the public.

It is greatly to be wished that the Gresham trustees would second and encourage the efforts of the Professors, by affording the lectures the advantage of the requisite illustrations. The usefulness of Mr. Pullen's would have been very much increased had he been supplied with diagrams large enough to be seen by the audience.]

ON Friday, April 20th, Mr. Pullen the Professor of Astronomy, commenced a course of three lectures on the tides. In the first lecture he explained the general phænomena of tides according to Bernoulli's, or the Equilibrium Theory. The earth being supposed a sphere covered with water to a certain depth, the attraction of the moon would have the effect of throwing that water into the form of a prolate spheroid, of which the upper pole is raised by the excess of the moon's attraction on the waters immediately subjected to her influence over that on the general mass of the earth; the lower results from a similar *relative* effect, the greater attraction of the moon on the mass of the earth *subtracting* it from the water. This spheroid will follow the moon at a certain interval; and a spectator on the earth's surface in the course of $24^h 48^m$ (a lunar day,) will be sensible of two tides, one by a passage through the upper, another by a passage through the lower pole. An effect similar to that of the moon is produced by the sun, but in a less degree. There will, therefore, be a tidal spheroid at a certain distance from the moon, and another tidal spheroid of greater dimensions following the moon. These effects in their combinations give rise to the semimenstrual inequality as well in the height of the tide as in the lunitidal interval. At the moon's conjunction and opposition the two effects are combined, and the result is spring tide; when the moon is in quadratures, the elevation due to one spheroid is partly counteracted by the depression due to the other, and the result is the phænomena of neap tides. The lecturer proceeded to show the effect which the monthly variation of the moon's distances from the earth, as evidenced by the variation of her horizontal parallax from 54′ to 61′, is calculated to produce both in the height of the tide and in the lunitidal interval; as well as the smaller effect attributable to the annual variation of the moon's distances. The highest possible spring tide is that which follows a new or full moon in the month of January, the moon being at the same time in perigee. The first lecture concluded with an explanation of the diurnal inequality, or difference in the heights of the superior and inferior tides depending upon the moon's declination, and the consequent inclination of the axis of the tidal spheroid to the plane of the equator. The moon being in the equator the superior and inferior tides are equal; but when the moon is in north declination the superior tide is greater than the inferior in north latitudes, and less than the inferior in south latitudes. The contrary effect is produced when the moon has south declination.

In the second lecture, the lecturer explained the mode in which tide observations had been conducted, and the manner in which they may be made to exhibit the different inequalities which theory leads us to expect. He particularly instanced the observations made at the London Docks from the year 1808 to the year 1826, from which tables were calculated by Mr. Lubbock, and published in the Philosophical Transactions for 1831. Grouping together observations made in different years, but in the same month, and corresponding to the same half hour of the moon's transit, we may eliminate the effects of winds and errors of observation by taking their mean; and thus a table of lunitidal intervals is constructed for every month of the year, and every half hour of the moon's transit.

The variation of the intervals in this table is called the calendar month inequality, and this is eliminated by taking the mean of the horizontal columns. The mean intervals are given under the head "mean," in the last vertical column. Two inequalities explained by theory are involved in this table: 1. the annual inequality depending upon the position of the sun in his orbit; and 2. the inequality depending upon the moon's declination; for the sun, occupying a definite place in his orbit, and the moon following the sun at a given time, she must necessarily have a certain specific declination. Neglecting, therefore, the annual inequality, the same result will be obtained, whether we compute the times of high water by a calendar month table, or by the column of mean transits and a declination table. This table, however, does not exhibit the effects of parallax; it is therefore necessary to group observations according to the values of the moon's horizontal parallax; and then by subtracting these quantities from the corresponding mean values such effects are exhibited. Tables thus constructed show that the lunitidal interval is diminished, and the height of the tide increased by the increase of the moon's parallax; a result to which theory also leads us. The greatest variation in the lunitidal interval at the London Docks is about 38^m, the greatest variation in height about 1¼ feet. The lecturer then proved that the diurnal inequality is also to be discovered by observation. At the London Docks it is small; but at Liverpool it makes nearly one foot of difference between the superior and inferior tides. This inequality may be seen by inspection of the tables, but it is exhibited more clearly in the zigzag form of the curve traced out by laying down the successive heights of high water as ordinates, and taking the corresponding times as abscissæ. A very remarkable peculiarity in the diurnal inequality is, that while the semimenstrual and other inequalities correspond very accurately to the *fourth* transit of the moon preceding the tide, this inequality corresponds as uniformly to the *fifth*, a fact which can only be accounted for by supposing (what also appears from other considerations,) that all the circumstances of the tides as observed in the northern hemisphere, depend upon conditions existing in the southern hemisphere, and that they are propagated from thence northwards, simply by the mechanical laws of undulations in fluids.

The third lecture was on the comparison of tidal observations in different parts of the world made with a view of tracing the progress of the tide. This theory is entirely of recent origin, being solely attributable to Mr. Whewell, who has justly remarked in his "Essay towards a first approximation to a Map of Cotidal lines," contained in the Philosophical Transactions for 1833*, that no attempt had till then been made to answer decisively the inquiry which Bacon suggested to the philosophers of his time, "whether the high water extends across the Atlantic, so as to affect contemporaneously the shores of America and Africa, or whether it is high on one side of this ocean when it is low on the other." The lecturer first showed what would be the motion of the tide wave on a sphere covered with water, and then considered the manner in which it would be affected by continents and islands, inland seas and bays. He also showed how the tide would be obliterated by the propagation of a series of undulations in opposite directions, differing in their epoch by six hours; and the modifying effects they would mutually produce when they differed by other intervals. The tides of different ports are compared by a comparison of their *establishments*. The "*vulgar establishment*" is the time of high water immediately following the new or full moon. This involving the semimenstrual inequality as well as the longitudinal difference of the port, a more correct form is obtained by Mr. Whewell in his second essay (Phil. Trans. 1836, p. 293,) by taking the mean of the greatest and least lunitidal intervals, correcting for the moon's parallax and declination, and her motion in Æ, and referring the whole to Greenwich time. The establishment thus corrected is called the cotidal hour of the port.

At the instance of Mr. Whewell, a request was made by the British Association for the Advancement of Science, to the British Government to procure a series of simultaneous tide observations to be made on the shores of Europe and America, in order to ascertain accurately the places of contemporaneous high water. In pursuance of this object, simultaneous observations were made during twenty days of the month of June, 1835, at about 500 places, extending from Florida to Nova Scotia on one shore of the Atlantic, and from Gibraltar to the North Cape of Norway on the other. At the same time similar observations were made at the several coast guard stations round the British Islands. The lecturer exhibited maps on which cotidal lines, or lines of contemporaneous high water, were laid down. These maps to a great extent verify the conclusions at which we arrive by theory. The tide wave raised in the southern ocean is propagated up the Atlantic northward; it then pursues its course round the northern coast of Scotland, and along the eastern shores of England, till it finally arrives at the mouth of the Thames. At the same time derivative tides are propagated through St. George's and the British Channels; so that these tides meeting with those which arrive from the north of Ireland in the one case, and the north of Scotland in the other, produce *interferences* of various degrees and forms. It appears from observation that the tide coming up the British Channel turns off to the Dutch side of the German Ocean, while the tide coming from the northward is flowing on the coast of England in the opposite direction. On the coast of Jutland it appears that the tides are almost entirely obliterated by interference.

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* See p. 354 of the present volume.

A remarkable fact exhibited by the accurate tide observations of 1835, is the great retardation of the tidal wave produced by the shores along which it is propagated. In some places the cotidal line instead of being inclined to the coast at a considerable angle is almost parallel to it; and so it happens that while it is high water at two promontories bounding a bay at a certain time, it will be high water considerably later in the bay itself. Thus in the British Channel the 10 o'clock cotidal line runs from the eastern shore of the Isle of Wight, touching Beachy Head, Dungeness, the headlands between Dover and Ramsgate, and extends into the mid-channel off the North Foreland, while on the other side it is pretty nearly parallel to the French coast, touching Cape Blanc Nez, and the promontory of St. Valery westward of Dieppe. Hence, it is high water at Brighton later than at Beachy Head, at Folkstone later than at Dover, at Dieppe and Boulogne later than at Calais. The same effect is exhibited in numerous instances as the tidal wave travels northward along the coasts of America and Spain.

At the end of the lecture the lecturer announced his intention of taking the figure of the earth as the subject of his next course, which will commence on the 4th day of next term, May 26.

ON THE INFLUENCE OF HEAT, ETC. ON THE CIRCULATION OF THE CHARA.

M. Dutrochet read a paper before the Royal Academy of Sciences at Paris, on the circulation of the Chara; giving an account of his experiments, showing the influence of temperature and mechanical irritation; and the action of salts, acids, alkalis, narcotics and alcohol upon the circulation of the Chara flexilis.

1st. *Influence of temperature.*—The circulation exists at the freezing point of water, but is slow. If the water in which the plant is placed be gradually heated, the circulation is accelerated as the temperature increases; at 65° to 66° Fahrenheit it becomes very rapid. It then diminishes, and at 80° it is very much slackened. If the temperature of 80° is continued the circulation after some time increases in quickness and soon becomes very rapid. If the temperature be then increased first to 94° and then to 104°, the same effect takes place, that is, the circulation after a diminution in quickness is by degrees accelerated; at 113° the circulation is stopped, and does not return.

It may be observed therefore that the plant which has been exposed to a temperature below 113° first experiences a torpor, but that this torpor disappears by degrees. Whenever the plant is submitted to a sudden change of temperature of about 77° the rotary motion is completely stopped, but begins again some time afterwards. In general a depression of temperature diminishes the quickness of the circulation, while the elevation of temperature, provided it does not exceed certain limits, augments it; beyond that temperature a slackening takes place. Cold produces the same phænomena: it tends to slacken the circulation, but the vital reaction restores to this circulation a quickness which is far from attaining that which it acquires under the influence of the action of increase of temperature.

2nd.—*Influence of Light.*—Light only acts upon the circulation of Chara in its quality of agent to determine its chemical actions of nutrition and respiration; but in regard to its action upon the existence, and upon the quickness of the circulation, it has no influence; the temperature being the same, there is no difference in the quickness of the circulation either during the day or the night.

3rd. *Influence of mechanical irritation.*—Compression by means of ligatures has a primitive and direct effect, producing a suspension, or simply a diminution of the motive action of the circulating fluid; but this action is soon re-established by the vital reaction. Incisions produce the same effect: if the verticillated leaves are cut, situated on the two opposite joints of a stem, the circulation in the central tube is stopped, and does not begin again for some minutes. Punctures produce also the same effects, provided they do not penetrate into the cavity of the central tube; in this case the circulation is entirely stopped.

4th. *Influence of chemical agents.*—A stem of Chara placed in water containing one-thousandth part of its weight of caustic potash or soda in solution stops the circulation after two or three minutes, without return. With a solution containing but one two-thousandth of alkali, the circulation at the expiration of five minutes becomes extremely slow; five minutes afterwards reaction begins, and the movement becomes very rapid. After 25 minutes the circulation again becomes very slow, and at the end of 35 minutes it entirely ceases, without returning. Lime water destroys the circulation in two or three minutes. A solution containing 50 parts of crystallized tartaric acid entirely destroys it in ten or twelve minutes. If the solution contains only one part of tartaric acid in 1000 water, at the end of three minutes the circulation is very much retarded, but after five minutes it is accelerated; at the end of three quarters of an hour it is again retarded, and after one hour's immersion it altogether ceases.

The circulation is immediately destroyed by water holding $\frac{1}{30}$th of its weight of marine salts in solution; the liquid makes a disorderly movement, the range of green globules are dissociated and become confusedly dispersed.

In a solution containing $\frac{1}{90}$th of its weight of marine salt the circulation is stopped at the end of four minutes, and slight convulsive movements are manifested; after eight minutes the circulation is reestablished and accelerates gradually, continues for eight days and then definitively ceases. A solution of one part of a watery extract of opium in 144 parts of water destroys the circulation in six minutes. In a solution of one part in 288 parts of water the circulation is suspended at the end of eight minutes, but ceasing for ten minutes, it begins again and becomes more rapid than it is naturally; it lasts thus during eighteen hours, then diminishes its quickness, and after twenty two hours it ceases altogether. Water containing $\frac{1}{20}$th of its volume of alcohol, of 36 degrees strength, considerably diminishes the quickness of its circulation at the end of five minutes; then after ten minutes the movement recommences, accelerated by the vital reaction, and becomes very rapid; it ceases altogether at the end of 42 hours, after gradually diminishing in quickness.—*L'Institut*, No. 223, January 1838.

OXALO-NITRATE OF LEAD.

M. Dujardin describes a new double salt, formed of two acids united to one base, which he has obtained, and calls an oxalo-nitrate of lead. It may be formed by dissolving, by the aid of heat, oxalate of lead in weak nitric acid; the liquid on cooling deposits brilliant white crystals in the form of rhomboidal plates, which appear to be derived from the right prism. It remains unchanged upon exposure to the air, is decomposed by heat, which drives off two atoms of water of crystallization, and disengages afterwards a mixture of nitrous and carbonic acids in red fumes. Water decomposes it, dissolving the nitrate of lead, and leaving the oxalate in the state of a white powder; but if nitric acid is added and slightly heated the oxalate is redissolved and the double salt reproduced. This salt is composed of one atom of nitrate of lead, one atom of oxalate of lead, and two atoms of water.

This is the only double salt of this kind which has been obtained; the other insoluble oxalates do not form combinations with the corresponding nitrates. Oxalate of manganese is even entirely decomposed by warm nitric acid, whilst oxalate of cerium simply dissolves and crystallizes on cooling; oxalate of copper is neither dissolved or decomposed.

M. Dujardin also remarks that the double salts described in chemical treatises, as composed of phosphate and nitrate of lead, cannot be obtained by dissolving the phosphate in nitric acid. The crystals thus obtained are octahedrons of the nitrate, modified in appearance by an excessive elongation of four opposite faces, which [therefore] might be mistaken for prismatic crystals.—*L'Institut*, January 1838, No. 223.

ACTION OF IRON AT A HIGH TEMPERATURE ON BENZOIC ACID: PRODUCTION OF BENZIN.

M. Felix D'Arcet passed the vapour of benzoic acid over red hot iron: by this he obtained a yellowish fluid oil which had an empyreumatic odour mixed with that of bitter almonds.

This oil was rectified on a salt water bath, and left a pitchy residue; the distilled product was very fluid and colourless; it had a peculiar odour, boiled at 185° Fahr., and congealed at 21°. By decomposition with oxide of copper it was found to consist of

Hydrogen	7·935
Carbon	92·065
	100·

This liquid is therefore benzin, composed of 6 equivalents of hydrogen and 12 equivalents of carbon; its production is attended with the formation of carbonic acid, and supposing hydrated benzoic acid to have been used, the action must have been thus:

	Hydrogen.		Carbon.		Oxygen.
Benzoic acid	6		14		4
Carbonic acid separated			2	+	4
Benzin obtained	6	+	12		

When the temperature is too high, then oxide of carbon is obtained; but when it is merely low red, then only carbonic acid is produced.

Benzin may also be obtained, according to M. D'Arcet, by distilling a mixture of benzoic acid and arsenious acid.—*Ann. de Chim. et de Phys.*, lxvi. p. 99.

ACTION OF IRON AT A HIGH TEMPERATURE ON CAMPHOR.

M. F. D'Arcet also passed the vapour of camphor over iron heated to redness; he obtained in the receiver a very fluid, yellowish, oleaginous liquor. When subjected to the heat of a salt-water bath no portion of it came over; but when the temperature was raised to about 293°, a slightly yellow coloured liquid distilled; it was lighter than water, had a peculiar aromatic odour, not at all resembling that of camphor, if the operation was slowly conducted.

Analysed by means of oxide of copper, it gave

Hydrogen.............	7·65
Carbon...............	92·35
	100·

This product, therefore, like the preceding, is equivalent to a compound of 12 hydrogen + 6 carbon, and therefore resembles benzin in composition; but its properties are very different, for it boils at 284° Fahr. instead of 185°. When the operation is conducted at a high temperature, then, besides the above-described product, naphthalin is also obtained.—*Ibid.*, p. 110.

ON CHLORIDE OF TUNGSTEN. BY M. HENRI ROSE.

This chloride was discovered by Woehler, who prepared it by heating oxide of tungsten in a current of chlorine gas. The oxide is then converted into chloride of tungsten, which is volatilized and separately obtained, and tungstic acid, which remains in the apparatus after the operation is over. The chloride of tungsten has the property of being decomposed by water into hydrochloric acid and tungstic acid; and it is this property which induced me to consider it as a chloride of tungsten corresponding in composition with tungstic acid. M. Malaguti supposed that he had confirmed this composition by analysis, having found, by quantitative experiments, that chloride of tungsten, obtained by the action of chlorine on oxide of tungsten, was composed of 47·4 of tungsten, and 54·89 of chlorine. I obtained the oxide of tungsten, which I used for the preparation of the chloride, from tungstic acid, by means of hydrogen; managing the heat so as to avoid the complete reduction of any part of the acid into metal.

If a current of dry chlorine gas be passed over the oxide thus prepared, chloride of tungsten is obtained mixed with the red chloride of tungsten which corresponds to the oxide of this metal; at the upper part of the glass bulb in which the oxide of tungsten is heated, and whilst a current of chlorine was passing through it, there was deposited a substance which could not be volatilized, even by heating the glass bulb as strongly as it could bear the heat. By heating slightly the chloride obtained, it is separated from the red chloride, which is much less volatile. If the chloride be too strongly and suddenly heated, red chloride is formed, and a first residue remains, similar to that obtained during the preparation of the chloride. These two products are tungstic acid.

This decomposition caused a suspicion, that the chloride is not entirely composed of chlorine and tungsten, but that it must contain some oxygen. It will afterwards be seen that it is impossible to obtain the chloride free from all admixture of tungstic acid, by attempting to free it from the chloride which accompanies it, with a gentle heat. Lastly, small quantities only of the chloride can be prepared, especially if the glass tubes, which are welted to the bulb in which the oxide of tungsten is heated, are not of sufficient diameter; for the chloride of tungsten formed, which collects near the opening of the tube, in the bulb heated by the spirit-lamp, soon deposits, by its decomposition, so much tungstic acid as to choke the tube, and causes the bulb to burst.

Two hundred and thirty-seven and a half parts of the chloride were dissolved in solution of ammonia, the tungstic acid which was mixed with the chloride remained insoluble; the solution evaporated nearly to dryness, and the mass dried and calcined gave 198·5 parts of tungstic acid, which corresponds to 66·67 per cent. of tungsten in the compound analysed. But as the chloride evidently contains oxygen, besides a small quantity of tungstic acid, formed during the preparation, and principally during the purification of the chloride, upon freeing it from the red chloride which accompanies it, the quantity of tungsten obtained evidently belongs to a combination formed (English equivalents) of 1 atom of tungsten = 100, 1 atom of chlorine = 36, two atoms of oxygen = 16.

This compound is named by M. Rose tungstate of chloride of tungsten; he considers it as analogous to the chromate of chloride of chromium; but he observes that it is a remarkable compound, because tungstic acid, which is one of the most fixed substances, is rendered volatile. When this compound is suddenly heated, it is decomposed into tungstic acid, red chloride of tungsten, and chlorine. The tungstic acid is deposited in the state of a bright yellow mass, which has sometimes a greenish tint. The apparent sublimate which is formed in the upper part of the glass bulb when strongly heated, arises from the partial decomposition of the tungstate of chloride of tungsten, which is then deposited at the moment of the action of the chlorine on the oxide of tungsten. This tungstic acid is very difficultly soluble, or rather, it is insoluble in ammonia. It is possible that the tungstic acid which remains insoluble in the solution of ammonia, when the volatile compound of chlorine is dissolved in it, derives this property and its origin from a decomposition occasioned by a high temperature.

Oxide of tungsten was prepared by M. Rose by heating a mixture of tungstate of soda and hydrochlorate of ammonia. This oxide when subjected to the action of chlorine, furnished a greater quantity of red chloride than the oxide obtained by reduction with hydrogen, probably because it contained metallic tungsten. As it was necessary to heat this tungstate of chloride of tungsten for a longer time than that obtained by the process above described, in order to free it from the red chloride, it contained a greater admixture of tungstic acid; it amounting to 68·92 per cent. instead of 66·67, as already noticed.—*Ann. de Chim. et de Phys.*, vol. lxvi. p. 13.

FALL OF METEORIC STONES IN BRAZIL.

On the 11th of December, 1836, about half past 11 o'clock in the evening, with a clear sky and a south-west wind, a meteor of uncommon size and brilliancy appeared over the village of Macao, at the entrance of the river Assu; it immediately burst with a loud crackling noise, and a shower of stones fell within a circle of 10 leagues. They came into several houses and buried themselves some feet deep in the sand, but they did not occasion any further damage than killing and wounding a few oxen. The weight of those picked up varied from 1 to 80 pounds. Specimens which have been sent to the Parisian Academy are to be analysed by Berthier.—*Compt. Rend.* tom. v. p. 211.—Poggendorff's *Annalen*, No. 12. 1837.

ON THE ADULTERATION OF CARMINE. BY C. G. EHRENBERG.

There occurs in commerce a kind of very fine coloured and very expensive carmine in the form of cakes, which owes its fine colour to an adulteration. Upon being made use of for ordinary painting no difference has been observed, but by the microscope it may be discovered that half of it consists of starch (wheat starch) which imparts to the finely divided carmine a clear ground and a brilliancy highly increasing the appearance of the colour. When such carmine is mixed with much water, it diffuses itself throughout, and is for a long time suspended; but upon pouring off the water a white sediment remains similar to white lead. This sediment is starch. Besides this distinct form and size of an amilaceous body when it is examined by its reaction upon tincture of sodium, it produces the well-known blue colour.. This sediment when heated with water forms a paste. The addition of white lead is detected by its weight, but the addition of starch is not so easily discovered; by means of the microscope the adulteration may be with certainty recognised, and confirmed by chemical examination. It may be perhaps interesting for the artist to know that few colours of this description mixed with an organic body, although generally pretty permanent, yet in a damp atmosphere are very liable to decomposition. In regard to its covering properties starch differs considerably from white lead. It covers less on account of its transparency.—Poggendorff's *Annalen*, No. 12. 1837.

METEOROLGICAL OBSERVATIONS FOR MARCH 1838.

Chiswick.—March 1, 2. Rain. 3. Fine: rain. 4. Rain: foggy. 5. Overcast. 6. Clear: cloudy: clear at night. 7, 8. Fine. 9. Frosty: fine. 10—12. Very fine. 13. Rain. 14. Hazy: fine. 15. Fine. 16. Fine: stormy showers at night. 17. Clear and cold: showery. 18. Cloudy and fine. 19. Drizzly. 20. Boisterous, with showers. 21. Clear, cold and dry. 22. Hazy: rain. 23. Bleak and cold, with slight snow-showers. 24. Fine: rain. 25. Fine. 26. Frosty and hazy. 27, 28. Fine. 29. Foggy: very fine. 30. Fine. 31. Hazy and cold.

Boston.—March 1, 2. Cloudy: rain early A.M. and P.M. 3. Cloudy. 4. Cloudy: rain A.M. and P. M. 5. Cloudy. 6. Fine: rain early A.M.: hurricane with rain P.M. 7. Fine: stormy with rain P.M. 8—12. Fine. 13. Rain. 14—16. Cloudy. 17. Fine: snow early A.M. 18. Fine. 19. Cloudy. 20. Stormy: rain early A.M. 21. Stormy. 22. Cloudy. 23. Snow A.M.: rain P.M. 24—27. Fine. 28. Cloudy. 29. Fine. 30, 31. Cloudy.

Applegarth Manse, Dumfriesshire.—March 1. Soft weather: dull and cloudy. 2. Rain: soon ceased. 3. Rain and sleet: cleared in the evening. 4. Fair and mild: chill in the evening. 5. Fine and clear: wet in the evening. 6. Storm of wind and rain. 7. Showery, with wind. 8. Clear, but cold: morning frosty: sun shone out. 9. Frosty: cloudy: raw in the evening. 10. Cloudy: wet afternoon. 11. Soft rain. 12. Fine day, but frosty: sun shone out. 13. Fine rain, but soon ceased. 14. Soft and genial shower. 15. Brisk wind and dry: sun shone out. 16. Hail showers: sleet: wind. 17. Showers of snow: high wind. 18. Frosty: clear: sun shone out. 19. Soft: cloudy: watery. 20. Stormy: wind and rain. 21. Dry and cold. 22. Sprinkling of snow: cold: sun shone out. 23. Frosty: slight snow: sun shone out. 24. Frosty: fine day: sun shone out. 25. Fine day: snow on the hills: sun shone. 26. Drizzling day, but cleared and sun shone. 27. Fine spring day: sun shone. 28. Fog in the morning: cleared: sun shone. 29. Fine and clear: sun shone out. 30. Fog: sunshine for half an hour. 31. Dull and cold: sun shone for a little.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfriesshire.*

Days of Month. 1838. March.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8½ A.M.	Dumfries.		London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	Dumfries.		London: Roy. Soc. 9 A.M.	Chiswick 1 P.M.	Bost.	Dumfries.	London: Roy. Soc. 9 A.M.	Chiswick.	Boston.	Dumfries.	Lond.: Roy. Soc. 9 A.M.
		Max.	Min.		9 a.m.	9 p.m.	Fahr. 9 A.M.	Self-register. Max.	Self-register. Min.	Max.	Min.		9 a.m.	9 p.m.									
1.	29·124	29·142	29·140	28·83	29·06	29·04	43·7	49·2	41·8	49	38	41	36	36	S.	S.	calm	NE.	·133	·11	·10		39
2.	29·094	29·225	29·116	28·82	29·05	29·10	41·7	48·0	40·9	44	36	39	36½	36½	NE.	E.	calm	E.	·088	·08	·07		40
☽ 3.	29·246	29·266	29·256	28·93	29·12	29·10	42·0	45·0	41·4	51	28	37	34½	34	W.	SE.	calm	E.	·063	·22	·03		39
4.	29·030	29·221	29·010	28·82	29·07	29·19	38·5	45·3	37·6	48	37	37	35½	35½	NE.	SE.	W.	E.	·105	·02	...		38
5.	29·614	29·826	29·624	29·23	29·50	29·42	41·2	47·6	38·4	46	39	40	36	35	NW.	NE.	calm	E.	·108	·06	...		37
6.	29·796	29·974	29·766	29·30	29·25	29·30	46·6	47·0	40·7	53	31	44	37	38	WSW. V.	SW.	calm	SW.	·161	...	·15		40
7.	29·912	29·966	29·927	29·44	29·47	29·61	41·9	51·3	37·0	53	34	40	38½	35	W.	W.	W.	SW.	...	...	·17		37
8.	30·116	30·329	30·118	29·64	29·86	30·13	42·2	50·0	37·8	51	25	38·5	36	35	NW.	NW.	calm	NW.	...	·02	·02		36
9.	30·302	30·336	30·187	29·87	30·13	29·90	37·3	47·8	35·2	51	28	36	32½	37	NW.	E.	calm	NW.	...	...	·05		37
10.	30·000	30·033	29·896	29·64	29·71	29·57	40·3	44·9	35·0	51	30	36	36	39	SE.	S.	calm	E.	...	...	...		38
○ 11.	29·778	29·929	29·802	29·44	29·50	29·72	39·8	46·4	36·0	52	29	40	38½	38	E.	S.	calm	SE.	...	...	...		36
12.	30·120	30·277	30·154	29·72	29·98	29·98	38·8	46·0	35·9	52	27	36·5	36	37½	N.	E.	calm	SSW.	...	...	...		35
13.	30·232	30·255	30·118	29·70	29·81	29·73	41·8	46·8	37·0	52	45	40	41	46	SE.	SW.	calm	W.	...	·04	...		37
14.	30·062	30·048	29·962	29·57	29·71	29·50	50·6	51·4	41·8	60	43	51	44	42½	W.	SW.	calm	S.	...	...	...		43
15.	29·948	30·016	29·965	29·46	29·84	29·73	46·5	58·3	44·6	50	24	42·5	36	36	NW.	NE.	N.	W.	...	...	·06		43
16.	29·900	29·927	29·636	29·39	29·37	29·29	41·8	48·4	37·2	52	32	39	38	35	W.	SW.	calm	W.	...	·18	...		39
17.	29·480	29·507	29·453	29·00	29·20	29·29	41·3	50·0	36·3	49	32	34·5	34½	32	NW.	NW.	NW.	W.	·180	·02	...		36
18.	29·618	29·649	29·635	29·24	29·40	29·43	41·0	47·0	36·2	48	36	37	32½	37	W.	NW.	calm	SW.	...	...	...		36
☾ 19.	29·610	29·640	29·565	29·22	29·42	29·22	41·2	46·0	39·8	50	36	39	38	38	S.	SW.	calm	SW.	...	...	...		38
20.	29·222	29·424	29·248	28·70	28·62	28·68	50·2	51·7	41·0	54	33	51	40½	35	SW. var.	W.	W.	WNW.	...	·02	...		42
21.	29·394	29·454	29·384	28·83	28·97	29·30	43·7	53·3	36·0	53	31	41	36½	34	SSW.	W.	W.	NW.	·055	·02	·27		38
22.	29·522	29·574	29·538	29·13	29·55	29·57	40·0	49·3	35·8	40	28	37	34	30	NW.	N.	N.	NW.	...	·01	...		37
23.	29·580	29·628	29·563	29·24	29·49	29·47	36·3	41·8	31·0	40	37	33	32	32	NW.	NW.	N.	NE.	...	·01	...		31
24.	29·566	29·584	29·432	29·20	29·37	29·30	41·0	42·6	35·4	48	35	35·5	34	34	SSW.	S.	calm	ESE.	...	·05	...		35
● 25.	29·820	30·098	29·849	29·28	29·48	29·83	44·8	47·2	36·4	53	25	42	36	33½	WSW.	W.	W.	W.	·036	...	·08		37
26.	30·130	30·212	30·139	29·71	29·88	29·93	42·7	50·8	35·2	55	27	38	38	37	NW.	S.	calm	SE.	...	...	·09		35
27.	30·312	30·409	30·313	29·83	30·11	30·25	43·9	51·4	38·4	57	29	44	44	41	NE.	NE.	calm	W.	...	...	...		38
28.	30·524	30·535	30·525	30·07	30·34	30·38	43·8	54·4	40·0	59	30	40	40	36	N.	N.	calm	SW.	...	...	...		40
29.	30·530	30·556	30·521	30·04	30·40	30·38	40·3	52·5	38·7	62	46	48	37	42	NE.	NE.	calm	SE.	...	...	...		40
30.	30·488	30·503	30·278	29·83	30·30	30·15	51·8	57·0	40·2	60	38	52	40	38	NNW.	NW.	calm	SE.	...	...	...		46
31.	30·180	30·209	30·162	29·71	30·10	30·10	45·8	56·4	41·8	52	26	43	33½	29	NNW.	N.	NE.	ENE.	...	...	...		43
Mean.	29·815	29·895	29·783	29·38	29·58	29·59	42·7	49·2	38·1	51·45	32·74	40·3	36¾	36¼					Sum. ·929	·86	1·09	Two Inch.	Mean. 38·2

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LXXII. *On the Formation of Calc Spar and Arragonite. By* Gustave Rose.*

THAT calc spar and arragonite, notwithstanding their different crystalline form, have the same chemical composition, and are therefore *heteromorphous* or *isomeric* bodies, has long since been acknowledged, and will now, by the following experiments, be placed beyond all doubt; but the conditions under which these substances are formed were hitherto quite unknown. From all former observations it even appeared as if both originated under very similar circumstances, as they are both apparently formed in the humid as well as in the dry way†. The circumstance, however, that the stalactite which is still deposited in calcareous caverns, is calc spar, while the Sprudelstein of Karlsbad is arragonite, brought to my mind the thought that the origin of calc spar and arragonite was owing to an action of the different temperatures at which the crystals of carbonate of lime are formed, and caused me to make a few experiments in order to explain this subject.

1. *Crystalline Form of Carbonate of Lime in the humid way.*

If we allow a solution of carbonate of lime in carbonated water to stand several weeks in an open glass at the common temperature, all the carbonic acid gradually escapes, and the carbonate of lime is deposited in minute microscopic crystals, partly on the sides of the vessel, and partly on the surface of the solution*. These latter are always the most distinct, and become, if the carbonic acid is allowed to escape slowly by covering the vessel with a glass plate, at times so large, that their forms may not only be easily recognised by the naked eye, but even their angles may be measured by means of the reflective goniometer. The crystals are *calc spar*, and have always the form of the primary rhombohedron of calc spar; they are generally obtuse at their ends from the superposed terminal surfaces.

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* Read before the Physico-Mathematical Class of the Academy of Sciences of Berlin, October 16, 1837: Translated by Mr. William Francis, from Poggendorff's *Annalen*, vol. xlii. p. 353.

† See Poggendorff's *Annalen*, vol. xxi. p. 57.

In the same way we also obtain calc spar if we mix a solution of chloride of calcium with carbonate of ammonia or with other alkaline carbonates at the common temperature of the air. The precipitate thus caused is in the beginning very voluminous and flocculent, and even retains this property if it is filtered soon after precipitation, washed and dried: if however it is allowed to stand still for some time, it gradually falls together and becomes granular. If we observe the loose precipitate under the microscope, we only perceive small opake grains, which on being highly magnified often appear exactly similar to those rings which Ehrenberg has described in chalk†; on the other hand, the precipitate which has become granular appears to consist of small sharply defined diaphanous rhombohedrons, perfectly distinct, and which, like the crystals obtained by evaporation, are evidently primary rhombohedrons of calc spar.

The specific gravity of the flocky precipitate I found to be 2·716, that of chalk 2·720, that of the granular precipitate 2·719‡. The specific gravity of the flocky precipitate is according to this somewhat less than that of the granular precipitate and of chalk; this circumstance may however only arise from the specific gravity of the flocky precipitate having been taken after it had already been dried, and it may in this way have come out rather too low§. At all events, the diversity is too small to be explained by a difference in the chemical composition, which, however, has been supposed, in this flocky precipitate. We may infer from this, that the specific gravity of all these bodies, and consequently also their chemical composition, is the same. They differ only in this respect, that the carbonate of lime in the flocky precipitate, as in chalk, with which compared under the microscope it exactly agrees, is in a very imperfect crystalline state, while the granular precipitate, which is subsequently formed from the imperfect crystalline, is in an obviously crystalline state. That chalk is in the same condition as the carbonate of lime immediately after precipitation, is a result which is probably not without interest in a geological point of view.

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* As it would be difficult without very large apparatus to procure such a solution of carbonate of lime, I applied to M. Soltmann, who with his usual kindness caused to be prepared for me in his manufactory of artificial mineral waters such a solution, and placed at my disposal several bottles of it, with which the following experiments have been made.

† Poggendorff's *Annalen*, vol. xxxix. p. 105, and Phil. Mag. vol. x. p. 318.

‡ Beudant fixes the specific gravity of the pulverulent calc spar rather higher, namely 2·723. (Poggendorff's *Annalen*, vol. xiv. p. 485.)

§ The method which I employed in the weighing of these, as also of other pulverulent bodies which are mentioned in this memoir, is the following: The powder was first mixed with water in a large goblet, and then poured into a small cylindrical glass of about an inch in size, which has reverted borders at the top, so that it could be suspended by means of a hair to a hook situated under the scale. The glass with powder was now weighed in water, then placed in a water bath, and the powder evaporated to dryness, and

According to the above method, we therefore obtain only calc spar; if, on the contrary, the solution of carbonate of lime in carbonated water is evaporated in a water bath to dryness, a loose crystalline powder is obtained, which under the microscope appears for the greatest part as an aggregation of distinct crystals, which manifestly have the form of arragonite. They generally appear as six-sided columns somewhat dilated, or as very acute six-sided double pyramids, like many sapphire crystals; at times, however, they appear as single pyramids, so that they are therefore crystallized differently at the two ends.

In the same manner we may also obtain arragonite by precipitating a boiling solution of chloride of calcium with hot carbonate of ammonia. The crystals, of which such a precipitate consists, examined under the microscope, are smaller than those obtained by evaporation; they are very distinct, however. In this case they very often occur as triad-crystals;

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then weighed again. In this manner nothing is lost of the powder, while on the other hand it would be difficult to avoid a loss if the experiment had been made in a reverse manner, the powder being first weighed dry and then in water. Yet notwithstanding this, I have constantly found the specific gravity of the precipitate which was immediately carried, after being precipitated and washed, to the water, rather higher than when the precipitate was first dried and then mixed with water, even when I boiled it in the water, which probably arises from the powder in the latter case, on being mixed with the water, not being freed from small air-bubbles. The specific gravity of the immediate precipitate I have always found to coincide with that which is obtained if we make use of small crystals for the determination; hence it appears necessary always to determine the specific gravity of pulverulent bodies in this manner if it be possible. Beudant also found (in the above-mentioned memoir) the specific gravity of a body in a pulverulent state always rather lower than in small crystals, which probably is to be ascribed to the above-mentioned circumstance: however Beudant has not stated in what manner he determined the specific gravity of the pulverulent body.

for from the middle of a columnar crystal two others diverge.

According to either method, however, it is difficult to obtain the arragonite pure, for generally a greater or less quantity of rhombohedrons of calc spar are found mingled with the columnar crystals. This intermixture of calc spar occurs especially when the arragonite is prepared by evaporation, and in this case it is easily explained, for the carbonic acid escapes even before the solution has attained that temperature at which only arragonite can form, and the precipitate thus originated must necessarily take the form of calc spar. And it is for this reason also that the specific gravity of arragonite so obtained is lower than that of the pure mineral. I evaporated twelve quarts of a solution of carbonate of lime to dryness in a large water bath, neglecting, however, to skim off at the beginning the saline scum which formed first, and probably consisted entirely of calc spar; but after all the liquid had evaporated I scraped the whole of the deposit together with a card from the sides of the vessel, by which it was all mixed together. The specific gravity of this arragonite amounted only to 2·803. In order to obtain the arragonite pure, I took a solution of carbonate of lime, which had previously stood exposed to the air for some time, and in which, therefore, a precipitate had been formed. The solution was filtered, and then gradually poured in small quantities into a vessel containing boiling water, and afterwards evaporated to dryness. The specific gravity of this deposit amounted to 2·836. According to this, therefore, it was rather purer than the former; however, it was still mixed with much calc spar.

A similar mixture of arragonite and calc spar may also be obtained by precipitating from a hot solution of chloride of calcium with carbonate of ammonia, although in this case the precipitate is generally purer arragonite than by the method of evaporation. I have varied in several ways the preparation by precipitation; I have taken more or less concentrated or weak solutions, mixed larger or smaller quantities of carbonate of ammonia, or chloride of sodium. I also obtained several times in experiments with small quantities, a precipitate, which under the microscope, consisted entirely of pure arragonite; if, however, I wished to prepare a greater quantity in the same way in order to obtain a sufficient quantity to determine the specific gravity, I always found in this intermixtures of calc spar.

There is, however, a very simple method *of obtaining arragonite quite pure*, which I accidentally tried after having put all the others aside; and it consists in this; not merely, as I had previously done, pouring the hot solution of carbonate of ammonia into the hot solution of chloride of calcium, but the reverse, by pouring the latter into the first. In this manner a completely loose precipitate is obtained, which, observed under the microscope, consists of still smaller crystals than those obtained by former precipitations, but entirely free from any intermixture of rhombohedrons of calc spar. This is also proved by its higher specific gravity, which, according to many experiments I performed, amounts to 2·949. I found the specific gravity of a single transparent crystal of arragonite from Bilin in Bohemia to be 2·945*. Beudant gives that of pulverulent arragonite at 2·9466.

In order to preserve unchanged the arragonite obtained by precipitation, it is necessary to wash and dry it immediately. If it is allowed to stand after precipitation for any length of time in the fluid, it is in a very curious manner gradually converted into calc spar. Eight days are quite sufficient for that purpose. Minute rhombohedrons are formed, which under the microscope are perfectly distinct, and are often so grouped in series, that their aggregation forms columns similar to those from which they originated. But this change also takes place, although much slowe., " the newly precipitated crystals are kept immersed in pure water. I observed this in the precipitate of entirely pure arragonite, the specific gravity of which had previously been ascertained. I had preserved the remainder in a vessel, into which I had conveyed it by a jet immediately off the filter on which it had been washed, with the intention of again determining the specific gravity of a portion of it, but which accidentally could not be done till after the lapse of eight days. The specific gravity which I now found amounted however to only 2·909. As I presumed that in this weighing some error might probably have occurred, I determined on the following day the specific gravity of a second quantity, and on the third day that of a third; but I now found still lower numbers, namely, on the second day 2·883, and on the third 2·881. Having then examined the arragonite under the microscope, I found perfect rhombohedrons among the minute crystals of arragonite, by which the cause of the lower specific gravity was now explained.

Although this metamorphosis of arragonite and calc spar takes place so easily, yet it only happens when the arragonite is newly precipitated. If it has been well dried first, it remains quite unchanged, even if it be now re-immersed in water or carbonate of ammonia, and allowed to stand in it for weeks. In the same way, natural arragonite, which I had reduced to a fine powder and treated in the same manner, did not in the least change.

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* Breithaupt (Vollständige Charakteristik des Mineralsystems, p. 65.) gives the specific gravity of a similar crystal of arragonite at from 2·937 to 2·938, which is evidently too low.

2. *Crystallization of Carbonate of Lime in the dry way.*

It is well known that carbonate of lime may under great pressure and by great heat be brought to melt, and then on cooling again it crystallizes. Sir James Hall performed this experiment, and probably a similar process has frequently taken place in the formation of the crust of the earth, since all marble has originated in this manner. The fused lime at its crystallization always forms calc spar, the three cleavage surfaces of which are perfectly visible in the larger grains of the marble. Arragonite is never produced in this way; it occurs however very frequently in the rents and cavities of mountain masses which previously had evidently been in a state of igneous fusion, as in basalt; but here the arragonite has evidently been formed from the infiltration of a solution of carbonate of lime which was heated by the still hot basalt, and therefore, according to what we have stated above, the carbonate of lime deposited itself as arragonite.

Besides, arragonite cannot exist at all at a greater heat; for if pieces of large arragonite crystals are exposed to a moderate red heat, for instance in a glass tube over an alcohol lamp, it puffs up, and falls, as Berzelius has shown, into a white opake coarse-grained powder. No change takes place in the chemical constitution at this merely moderate high temperature at which the arragonite pulverizes; no gas is developed, as Mitscherlich* has proved to be the case; and above all things, it suffers scarcely any change in its weight, of which I have convinced myself by several experiments; for 1·721 grammes of small fragments of crystals of arragonite from Bilin in Bohemia, weighed, after having been heated, 1·717 grammes; and in a second experiment 1·9115 gr. weighed 1·9090. The small loss in weight of 0·23 per cent. in the one, and of 0·13 per cent. in the other case, arises only from some water of decrepitation, the escape of which on heating the mineral evinces itself by the flying off of small pieces.

For the explanation of this phænomenon, Haidinger † has already advanced the opinion, that it should be ascribed to a metamorphosis of calc spar into arragonite which takes place at this higher temperature, and that the cause of the pulverizing of the calc spar was this; that the carbonate of lime, in the form of arragonite, possessed a higher specific gravity than calc spar, and therefore occupied a smaller space than the latter. Although this opinion has from that time been pretty commonly admitted, it has never yet been strictly proved, and on this account I found it necessary to make a few experiments with respect to it. For this purpose I heated some crystalline fragments of arragonite from Bilin: after having previously weighed them, I weighed again the fallen powder in order to convince myself that no carbonic acid had escaped, and now took the specific gravity of the powder. I always found it as follows in three different experiments, which were always performed with different quantities: 2·703, 2·704, and 2·709. In the latter experiment the powder had previously been boiled for some time. According to this, therefore, we may suppose with certainty that arragonite at a weak red heat changes into calc spar; for even if the numbers found are somewhat lower than those obtained when the specific gravity of small crystals of calc spar is taken, this without any doubt has its cause in the above-mentioned circumstance.

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* Poggendorff's *Annalen*, vol. xxi. p. 157.

† *Ibid.*, vol. xi. p. 177.

The disintegration of arragonite at a low red heat is very visible in large crystals, and is a very remarkable phænomenon; it occurs, however, only in large crystals, never in small ones or fibrous masses. The Sprudelstein of Karlsbad loses its transparency on being heated, but does not disintegrate; nor do those small arragonite crystals disintegrate which are found upon the branches of the coralloidal arragonite (*Eisenblüthe*) from Steiermark, nor the microscopic crystals of the arragonite artificially prepared.

If we examine the powder obtained by the disintegration of the larger crystals of arragonite, under the microscope, it has the appearance of quite irregular fragments which are perfectly transparent, but full of cracks and rents. The minute crystals of the coralloidal arragonite from Steiermark retain with their transparency their form also; they appear, however, in the interior full of rents, and have on their sides cracks often very wide*. The larger only of the microscopic crystals of the artificially prepared arragonite have some rents; the smaller do not appear at all changed, and possess the same transparency and form. Notwithstanding this, the mass has become completely converted into calc spar, as the examination of the specific gravity proves. The mixture of arragonite and calc spar obtained by evaporation on the water-bath, the specific gravity of which I had found by experiment to be 2·803, was, in order to be certain that all became changed (since it is in this case impossible to see the change as in the large arragonite crystals,) exposed in a platina crucible to a very high temperature. By this a part of the carbonate of lime becomes already caustic: this however may immediately be removed by dissolving it in a large quantity of water, decanting the water, and washing the powder. Examined under the microscope the larger arragonite crystals had become full of flaws, in the manner above indicated. I found the specific gravity of the mass thus treated to amount to 2·700, like that of the heated arragonite. This circumstance proves, that in the small crystals of arragonite the smallest parts are capable of extending and reverting themselves, without the form of the crystal being in any degree lost; they are the most perfect pseudomorphous crystals of calc spar in the form of arragonite. It is probably not impossible to convert larger crystals into such pseudomorphous crystals, if the crystals of arragonite be exposed to a slowly increasing heat.

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* For this reason therefore it would be better to perform the heating in a glass tube blown out at the middle into a globe, and during the heating to pass some carbonic acid over the arragonite. In order to be assured whether at the heating of the arragonite any portion of the mass had become caustic, it is not sufficient after the addition of some water to see whether the red litmus paper turns blue; for undecomposed carbonate of lime always produces the same effect if it be powdered and immersed in water.

The *results* of the experiments above related are as follows:

1. That in the humid way both calc spar and arragonite are formed, the first at a lower, the latter at a higher temperature; but in the dry way calc spar alone is formed.
2. That carbonate of lime immediately after precipitation from a cold solution is in an indistinct crystalline state, which agrees with chalk, from which subsequently the distinct crystalline state proceeds.
3. That arragonite changes very easily into calc spar, in the humid way, if the arragonite obtained by precipitation is allowed to stand under water or in a solution of carbonate of ammonia; in the dry way, if the arragonite is exposed to a low red heat, at which the large crystals fall into a coarse powder, but small crystals retain at the same time their form, and produce pseudomorphous crystals.

A further consequence is that the origin of arragonite can no longer be ascribed, as has frequently been the case, to the small quantity of strontia, which natural arragonite generally contains. This follows indeed, even from the circumstance, that there is arragonite which does not contain any carbonate of strontia; but the case is now completely proved by the fact that arragonite may be prepared artificially with ease without containing any such portion of strontia. I purposely mixed a small quantity of a solution of chloride of strontia with a solution of chloride of lime, but by precipitation at the common temperature with carbonate of ammonia only crystals of calc spar were perceivable.

The form of arragonite is also found in witherite, strontianite, and white lead ore, or the neutral carbonates of barytes and strontia, and carbonate of lead: however, I have not succeeded in producing this substance in the second rhombohedral form as in the carbonate of lime by the above-mentioned method.

If carbonate of barytes be dissolved in hydrochloric acid, and the solution precipitated with carbonate of ammonia, we obtain (the solution may have been hot or cold,) a precipitate, which examined under the microscope consists of very distinct crystals similar to those of arragonite.

If we treat carbonate of strontia in the same manner, we recognise under the microscope, from the hot solution very distinct crystals quite similar to those of arragonite; the precipitate from the cold solution as aggregated globules, which do not allow of our distinguishing any fixed form.

With the carbonate of lead the case was the reverse; here the precipitate from a cold solution produced crystals similar to those of arragonite (which however were much smaller and less distinct than in the carbonates of barytes and strontia); the precipitate from a hot solution gave an opake indistinct mass. It is however probable that we also obtain, from the cold solution of the chloride of strontia and from the hot solution of chloride of lead, crystals which are determinable, if we find out the favourable circumstances under which the crystals are formed, which however from this would probably always have the form of arragonite. This also explains why it is that a small mixture of carbonate of strontia frequently occurs in arragonite, but never in calc spar, (at least has not hitherto been found in it,) not even when calc spar and strontianite occur grown together. Even thus we do not find in calc spar carbonate of barytes, but this forms with calc spar a double salt, baryto-calcite, which is evidently a combination of 1 atom of calc spar with 1 atom of witherite. Carbonate of lead alone is sometimes found in small quantities in calc spar: such a combination Johnston has described under the name of plumbo-calcite.

If I did not succeed in producing in the form of calc spar the latter carbonates which occur only in the form of arragonite, I was so fortunate as to obtain a carbonate in the form of arragonite which occurs only in the form of calc spar and generally in connection with it; this is the neutral carbonate of magnesia. If, namely, we evaporate a solution of this salt in carbonated water to dryness on a water-bath, a crystalline powder is obtained, which under the microscope exhibits crystals similar to those of arragonite, and much larger than those obtained by the evaporation of a solution of carbonate of lime. They are however not found alone in this powder, but in company with the eccentric-radiated globules described by Fritzsche*, and which he proved to belong to *magnesia alba*: I mention the first, however, as it is the first time that the neutral carbonate of magnesia has been prepared in a state free from water.

I have hitherto not been able to pay that attention to these latter bodies which they deserve. There remains a wide field open for new experiments.

LXXIII. *Observations on Sulphurous Æther, and Sulphate of Ætherine (the true Sulphuric Æther). By* R. Hare, *M.D., Professor of Chemistry in the University of Pennsylvania.*†

IT is known that when two parts, by weight, of sulphuric acid are distilled with one of alcohol, a yellow sulphurous liquid is obtained. Berzelius alleges, that when this liquid is exposed in an exhausted receiver over sulphuric acid and hydrate of potash, an oleaginous liquid remains, which he designates as "*oil of wine containing sulphuric acid, or heavy oil of wine.*"

This oil is, by the same author, described as being heavier than water, as having a penetrating aromatic odour, and a cool pungent taste, resembling that of peppermint. It is, in fact, the liquid which Hennel first analysed as oil of wine, without, at the same time, mentioning the process by which it was procured. No doubt the difference between it and that procured by Boullay and Dumas, was, in some degree, the cause of the discordance between his observation and theirs. According to Hennel, the oil of wine consists of an atom of sulphuric acid, and an atom of hydrocarbon: $\overset{\therefore}{S} + 4C + 4H$. By the last-mentioned appellation, this skilful chemist designates a compound consisting of four atoms of carbon, and four of hydrogen.

Serullas represents the oil in question as consisting of two atoms of the acid, two of hydrocarbon or ætherine, and one of water.

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* Poggendorff's *Annalen*, vol. xxvii. p. 304.

† From the Transactions of the American Philosophical Society, N.S., vol. v. p. 347.

To the hydrocarbon of Hennel (4 C H),* as the common base of all the æthers, excepting those lately alleged to have mythelene for a base, the name of ætherine has been given; so that the heavy oil of wine may be called the sulphate of ætherine: or, according to the formula of Serullas, $2\dddot{S}E + H$, it is a hydrous sulphate of ætherine. It is, in fact, the only compound to which the name of sulphuric æther can be applied with propriety. The yellow liquid out of which it is procured, as above stated, may be designated as the æthereal sulphurous sulphate of ætherine.

Another oil, lighter than water, resulting from the distillation of the æthereal sulphurous sulphate of ætherine, from hydrate of lime, or from potash, is described by Berzelius as oil of wine exempt from sulphuric acid. Of this the odour is represented as disagreeable; and, though nothing is said of its taste, it is to be presumed that it differs from the heavy oil of wine in this respect, as well as in its odour and specific gravity.

Thenard alleges, that when the heavy oil of wine is heated with water for some time, a liquid swims on the water, which, if refrigerated by ice, will, within twenty-four hours, deposit crystals. The mother liquid he calls light oil of wine, while to the crystals he gives the name of concrete oil of wine. Hennel mentions his having obtained a similar product by the reaction of oil of wine with water, or an aqueous solution of potash; and treats the crystalline matter as the base of the heavy oil of wine, deprived of its acid; or, in other words, as his "hydrocarbon;" or, as above mentioned, ætherine.

Considering how much has been written on this topic, I am surprised that I have met with no statements respecting the reaction of ammonia with the above-mentioned æthereal sulphurous sulphate of ætherine.

Since the year 1818, I have been accustomed to saturate the acid in that liquid by ammonia. The residue, being rendered very fragrant, and entirely freed from its sulphurous odour, by admixture with about twenty-four parts of alcohol, was found to constitute an anodyne, possessing eminently all the efficacy of that so long distinguished by the name of Hoffman. When the residue, remaining after saturation with ammonia, was distilled in a water-bath, æther came over, and left an oil which I was accustomed to consider as the oil of wine.

I had observed that, in the process above-mentioned, there was a striking evolution of vapour, which seemed irreconcilable with the received opinion of the reagents employed. Since the affinity between the ammonia and sulphurous acid is energetic, it did not appear to be reasonable that a copious escape of the one should be caused by its admixture with the other; and it was no less improbable that the vaporization of hydric æther, in its natural state, could take place at temperatures so much below its boiling point as those at which this phænomenon was noticed. In order to ascertain the truth, I luted a funnel, furnished with a glass cock and an air-tight stopper, into the tubulure of a retort, of which the beak was so recurved downwards as to enter and be luted into the tubulure of another retort. The beak of the latter passed under a bell over water.

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* Mr. Hennel's paper will be found in Phil. Mag. and Annals, N.S., vol. vi. p. 342.—Edit.

Both retorts were about half full of liquid ammonia, and surrounded with ice. The apparatus being thus arranged, about a thousand grains of the æthereal sulphurous sulphate of ætherine were poured into the funnel, and thence gradually allowed to descend into the ammonia in the first retort. Notwithstanding the refrigeration, much heat was perceptible, and a copious evolution of vapour, which, passing into the second retort, was there absorbed or condensed, none being observed to reach the bell glass. At the close of the operation, hydric æther, holding oil of wine in solution, floated upon the ammonia in the first retort, and pure æther, of the same kind, floated on the ammonia in the second.

The ammonia in both retorts gave indications of the presence of sulphurous acid, on the addition of sulphuric acid. From these results, I inferred that a chemical compound of sulphurous acid and hydric æther formed the principal portion of the yellow liquid, and might be separated by distillation. Accordingly, by means of retorts arranged and refrigerated as above described, I procured a portion of sulphurous æther, which boiled at 44°, and which, when agitated with ammonia in a bottle, produced so much heat and consequent vapour, as to expel the whole contents in opposition to the pressure of my thumb. By employing the same distillatory apparatus, I subjected 2150 grains of the æthereal sulphurous sulphate of ætherine to distillation, and obtained 726 grains of sulphurous æther, which boiled as soon as the frigorific mixture was removed from the containing retort. This being redistilled, as in a former experiment, so as to receive the product in ammonia, left in the retort five grains of oil of wine. The resulting ammoniacal liquid, saturated with chloride of barium in solution, gave a precipitate which, agreeably to the table of equivalents, contained 356 grains of sulphurous acid.

The residue of the 2150 grains of æthereal sulphate being subjected to distillation, raising the temperature from 95°, the point at which it had been before discontinued, to 140°, the product obtained by means of a refrigerated receiver weighed 602 grains. This was, of course, inferior in volatility to the first portion distilled; and, when redistilled, it was found to contain a small quantity of oil of wine. In fact, it appears, the boiling point of the æthereal sulphurous sulphate rises, not only as the ratio of the sulphurous acid lessens, but also as the proportion of oil of wine augments.

The residual liquid being exposed to the heat of a water-bath at 212°; a very fragrant, and well-flavoured oil of wine was evolved, and floated upon a quantity of water acidulated by sulphuric or sulphovinic acid.

Agreeably to another experiment, 1750 grains by weight, of the æthereal sulphurous sulphate of ætherine, after washing with ammonia, gave 869 grains of an æthereal solution of oil of wine. This being subjected to distillation by a water-bath raised gradually to 190°, there remained in the retort 148 grains of oil, beneath which there were a few drops of acidulated water. Agreeably to the result of several experiments, the æthereal sulphurous sulphate of ætherine yields about half its weight of the æthereal solution of oil of wine. The quantity is always somewhat less than half when weighed; but the deviation is not greater than might be expected to result from the loss by evaporation, and the diversity of refrigeration employed in the condensation of the æthereal sulphurous sulphate, during the process by which it is evolved.

Under the expectation of procuring a sulphurous æther of a still higher degree of volatility, I associated with the apparatus usually employed in the process for generating hydric æther, a series of tubulated retorts, of which the beaks were recurved downwards in such a manner that the beak of the first communicated with a perpendicular tube, passing through an open-necked cylindrical receiver, so as to enter the tubulure of the second retort, of which the beak was in like manner inserted into a tube passing through a receiver in a third retort, and this communicated in like manner with a fourth retort. The second, third, and fourth retorts, and the tubes entering them, were all refrigerated, the first with ice, the second with ice and salt, and the third with ice and chloride of calcium.

By these means, on subjecting to distillation in the first retort 48 ounces of alcohol of 830, and a like weight of sulphuric acid, besides the æthereal sulphurous sulphate of ætherine usually resulting from the process, and condensing in the first receiver, it was found that in the other retorts severally, there were liquids of various degrees of volatility. That in the last boiled at 28°, but the boiling points rose gradually as the quantity of the residual liquid diminished.

In order to ascertain the nature of the sulph-acids abstracted from the æthereal sulphurous sulphate of ætherine by the ammonia employed, chloride of barium was added in excess to the resulting ammoniacal solution, until no further precipitate would ensue. The liquid having been rendered quite clear by filtration, soon became milky. By evaporation to dryness, and exposure to a red heat, a residuum was obtained which proved partially insoluble in chlorohydric acid, and by ignition with charcoal, yielded sulphide of barium. It appears, therefore, that a hyposulphate of barytes existed in the liquid after it was filtered; as I believe that the hyposulphuric acid is the only oxacid of sulphur which is capable of forming with barytes a *soluble* compound, susceptible, by excess of oxygen, of being converted into an insoluble sulphate, and precipitating in consequence.

It must be evident from the facts which I have narrated, that the yellow liquid obtained by distilling equal measures of sulphuric acid and alcohol, consists of oil of wine held in solution by sulphurous æther, composed of nearly equal volumes or weights of its ingredients; also, that the affinity between the *æther* and the acid is analogous to that which exists between alcohol and water. The apparent detection of sulphuric acid in the ammonia, justifies a surmise, that the ætherine distils in the state of a hyposulphate, which subsequently undergoes a decomposition into sulphurous acid and sulphate of ætherine.

The liquid above alluded to, as resulting from the saturation of the æthereal sulphurous sulphate of ætherine by ammonia, and distillation by means of a water-bath gradually raised to a boiling heat, is a very fragrant variety of oil of wine. It differs from that described by Berzelius as the heavy oil of wine of Hennel and Serullas, in being lighter and containing less sulphuric acid. I have a specimen exactly of the specific gravity of water, and have had one so light as to float on that liquid. The oil of wine obtained by ammonia approximates, in its qualities, to the variety which Thenard describes as light oil of wine. The presence of sulphuric acid in a definite or invariable ratio does not appear requisite to the distinctive flavour or odour of oil of wine.

The heavy oil of wine treated by Hennel as sulphate of hydro-carbon, $2\dddot{S}+4CH$; and by Serullas as a hydrous sulphate of ætherine, $4\,C\,H + 2\dddot{S} + \dot{H}$; I have obtained, as above mentioned, by exposing the æthereal sulphurous sulphate of ætherine, *in vacuo*, over the hydrate of lime, or potash, and sulphuric acid. This variety sinks in water, being of the specific gravity of 1·09 nearly; is of a deeper hue than the other, and of a smell less active, with a taste somewhat more rank. A specimen of oil thus obtained being subjected to the distillatory process, a portion came over undecomposed, leaving in the retort a carbonaceous mass. 14 grains of the oil which had not undergone distillation, and a like portion of the distilled oil, were severally boiled in glass tubes with nitric acid until red fumes ceased to appear; about 28 grains of pure nitre were added to each, some time before the boiling was discontinued. The resulting liquid was in each case poured into a platina dish, boiled dry, and afterwards deflagrated by a red heat. The residual mass being subjected to water, the resulting solution was filtered, an excess of nitric acid added, and then nitrate of barytes in excess.

The precipitate obtained from the distilled oil, weighed, when dry, only nine and five-eighths grains, while that procured from the oil which had not been distilled, amounted, under like circumstances, to fourteen and one-eighth grains. Ten grains of another portion, left for some time over liquid ammonia, yielded only seven-eighths of a grain of sulphate.

About a drachm of Hennel's oil of wine was subjected to distillation with strong liquid ammonia; fourteen and a half grains came over, retaining the appropriate fragrance and flavour. This yielded, by the process above described, only two grains of sulphate of barytes. After all the water and ammonia had distilled, the receiver was changed, and fourteen grains of oil, devoid of the fragrance and flavour of the oil of wine, were obtained. This yielded one and one-eighth grains of sulphate. A carbonaceous mass, replete with sulphuric acid, remained in the retort.

Hennel states, that when oil of wine was heated in a solution of potash, an oil was liberated which floated upon water, having but little fluidity when cold; and which, in some cases, partially crystallized. When gently heated, it became clear, and of an amber colour. The vapour had an agreeable, pungent, aromatic smell. This oil must have been pure ætherine.

It is not improbable, that this oil, which may be considered as devoid of sulphuric acid, is more or less liberated in evolving oil of wine, according to the nature of the process employed; and that the oil alluded to by Thenard, and those procured by me by simple distillation, ebullition, or distillation with ammonia or potassium, are mixtures of the ætherine with its sulphate in various proportions. As it is well known that the odour of the essential oils is rendered more active by dilution, the livelier smell of the solutions may be consistent with a diminished proportion of the odoriferous matter.

Oil of wine cannot be distilled *per se* without partial decomposition, which does not take place below the temperature of 300°. When subjected to the distillatory process, over potassium, at a certain temperature, a brisk reaction ensued, and the oil and metal agglutinated into a gelatinous mass. By raising the temperature the mass liquefied, and a colourless oil came over, which retained the odour of oil of wine. Meanwhile some of the potassium remained unchanged, and appeared within the liquid in the form of pure metallic globules. On pouring into the retort a portion of nitric acid in order to remove the caput mortuum, ignition took place from the presence of the potassium.

LXXIV. *On a supposed Analogy in Atomic Constitution between the Earthy Carbonates and Alkaline Nitrates. By* J. F. W. Johnston, *A.M., F.R.SS., L. & E., F.G.S., Professor of Chemistry and Mineralogy in the University of Durham.*[*]

IF we tabulate the knowledge we possess in regard to those dimorphous compounds, which in the crystalline state assume the two forms of arragonite and calc spar, we shall have the following table.

	In Rhomboids.	Dimensions.	In Right Rhombic Prisms.	Dimensions.
$\dot{Ca}\ddot{C}$	In Calc Spar	105° 5′	in Arragonite......	116°10′
$\dot{Fe}\ddot{C}$	In Brown Spar ...	107 0	in Junckerite......	108 26 118 0 } ?
$\dot{Pb}\ddot{C}$	In Plumbo Calcite	104 53½?	in Native Carbonate.	117 18
$\dot{Mg}\ddot{C}$	In Bitter Spar ...		Artificial †	?
$\dot{K}\overset{\cdot\cdot\cdot\cdot\cdot}{N}$	Rarer form of Nitre in microscopic crystals	106 36	in common crystals of Nitre	118 52

In this table we have a universal analogy in form, but in regard to one substance $\dot{K}\overset{\cdot\cdot\cdot\cdot\cdot}{N}$ a remarkable difference in chemical constitution. How is this difference to be reconciled to the analogy in atomic constitution generally observed among compounds known to assume the same form?

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[*] Communicated by the Author.

† G. Rose in Poggendorff's *Annalen*, xlii. p. 366. [*ante* p. 474.]

Between the formula $\dot{Ca}\,\ddot{C}$ and $\dot{K}\,\overset{\cdot\cdot\cdot\cdot\cdot}{N}$ there is no obvious analogy. If we represent the electro-positive elements in each by R, the formulæ become $\dddot{\underline{R}}$ and $\overset{\vdots\vdots\vdots}{\underline{R}}$, which also belong to very different groups of compounds, analogous to the sesqui-oxide and first acid of manganese $\dddot{Mn}$ and $\dddot{M}$. But if we halve the equivalent of potassium (for the reasons given in a former number of this Journal) and adopt Berzelius's weight for nitrogen, the formula for nitre becomes $\underline{\dot{K}}\,\underline{\overset{\cdot\cdot\cdot\cdot\cdot}{N}}$, or taking the positive elements together $R_4\,O_6$ or $\dddot{\underline{R}}$ the same as the formula for the dimorphous carbonates.

This mode of establishing an analogy in atomic constitution between these isodimorphous compounds is very short and simple, but there are very strong reasons why we ought not at present hastily to adopt it. In all compounds known to crystallize in identical forms an analogy in atomic constitution has been observed to prevail, with few exceptions, not only between the crystalline compounds as a whole, but between the several compounds of two or more elements, by the union of which they are supposed to be directly formed. Thus, $\dot{Ag}\,\dddot{S}$, and $\dot{Na}\,\ddot{Se}$, which crystallize in forms nearly identical, are analogous not only in containing each two equivalents of positive to four of negative elements ($\ddddot{\underline{R}}$), but also in containing an analogous oxide of a metal ($\dot{R}$) united to an analogous acid ($\dddot{R}$). The salts of ammonia present an exception to the first part of this rule, the sulphate or seleniate of this base being represented by a formula in which the oxide $\dot{R}$ or $\dot{\underline{R}}$ of the silver and soda salts is replaced by $N\,H_4\,O$. Still the analogy holds in regard to the acid, and we draw a double inference in both cases, that the entire salts, namely, are isomorphous, and that the acids and bases also are isomorphous each with each, and may mutually replace each other.

The titaniate of the protoxide of iron $\dot{Fe}\,\dddot{Ti}$, in replacing the peroxide of iron $\dddot{Fe}$ in Ilmenite and some allied minerals, presents an example which at first sight would appear to justify the method above suggested for establishing an analogy between the nitrate of potash and the carbonate of lime. But so long as we can represent the constitution of peroxide of iron by $\dot{\mathrm{Fe}} + \dddot{\bar{\mathrm{Fe}}}$, a formula corresponding not only as a whole, but also in each of its members, with that of titaniate of iron, very little confidence can be placed in the support which this case seems to lend to the method proposed.

Still there is something very remarkable in the fact that the earthy carbonates and the alkaline nitrates both crystallize not merely in *one* but in *two* forms wholly different, in each of which forms they are isomorphous. The fact is so striking, that we cannot lightly reject the notion, that a simple relation must exist between the atomic constitution of the two classes of compounds.

In regard to the ultimate form of lime compared with that of potash and soda, it is inferred, *first*, that they are not identical because the analogous salts (the anhydrous sulphates for example) crystallize in forms very different from each other*: *secondly*, that the form of lime with an atom of water is probably identical with that of potash and soda, because with this excess of water it appears to replace the latter in mesotype, chabasie, and other minerals of the zeolite family. We should expect therefore that where the same acid in the same proportion combines with $\dot{\mathrm{K}}$, $\dot{\mathrm{Na}}$, or $\dot{\mathrm{Ca}} + \dot{\mathrm{H}}$, the resulting compounds should be isomorphous; where it combines with $\dot{\mathrm{Ca}}$ only, the form of the salt of lime should be different.

But though lime and potash combined with the same acid A produce unlike forms, there is no reason why lime may not combine with another acid B to produce a form identical with that of potash, in union with A; in other words, there is no known law of crystalline combination in accordance with which a form C can result from the union of two invariable forms only, A and B. There may in all probability exist other pairs or groups of forms D, E, differing to any extent from A and B, yet so related that the result of their union may be the form C. It need not surprise us therefore that instances should occur, in which, though an isomorphic relation be observed to prevail between two compound bodies, no such relation can be detected between the parts or simple compound bodies of which they are made up; on the contrary, the wider our knowledge extends, the more numerous in all probability will such instances become.

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* The difference between the forms of the anhydrous sulphates of lime and soda might be explained by supposing them dimorphous (*Cristallographie* von G. Rose, p. 159.), but if soda be isomorphous with potash, and anhydrite differ in form from both alkaline sulphates, the difference could only be explained by supposing all of them to be trimorphous; a supposition not at present to be entertained. The three anhydrous sulphates of potash, soda, and lime crystallize in right rhombic prisms, in which M on M′ subtends angles of 120° 30′, 125° and 100° 8′ respectively.—(Brooke).

According to this reasoning, an analogy in atomic constitution among the several constituents, simple or compound, which make up any pair of isomorphous bodies, chemically different, is not a condition *necessary* to their existence. A general analogy, however, may be necessary between the isomorphous bodies taken as a whole, whether that of an equal ratio between the positive and negative equivalents present in them, similar to that made out as above between the earthy carbonates and the alkaline nitrates; or some other relation, as that between the several elements in the isomorphous crystals of permanganate of baryta and anhydrous sulphate of soda, to which the attention of chemists has lately been drawn by Dr. Clarke*.

Still as deductions from observation we know but few exceptions to the law of Mitscherlich, that identity of form implies analogy of constitution in compound bodies as well as identity of form in *both*, and analogy of constitution in *one at least* of the parts or members of which they are composed.

It may hereafter be established, that an equality in the number of positive and negative equivalents, or even an equality in the ratio of these, may give the power to crystallize in the same form, or in the same suite of two or more forms; but though such views derived from reasoning may enable us to explain certain anomalies in this and other branches of the science, yet we must carefully distinguish them from that *certain* knowledge which is derived from direct observation, regarding them only as a portion of that dawning light which precedes the establishment of every great principle.

In the above observations therefore I would be understood as desirous rather of bringing the matter more under the attention of chemists, and familiarizing them with the mode in which such relations as that observed between the earthy carbonates and alkaline nitrates may be supposed to take place, than as advocating the reception of the hypothesis I have stated, which further observation may prove to be wholly destitute of foundation.

Durham, Sept. 1837.

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* See Dr. Clarke's letter to Mitscherlich, in the Records of General Science.

LXXV. *A Reply to the Observations of* J. H. Wheeler, Esq. *on the Method of computing the Results of Experiments with the Comparative Photometer.* *By* Richard Potter, *Esq., B.A.*[*]

IN the Number of the Philosophical Magazine and Journal of Science for December 1834, (vol. v. p. 439.) is a paper by J. H. Wheeler, Esq., in which he gives the results he has obtained on repeating certain of my photometrical experiments published in previous Numbers, and his calculations founded upon them.

It is satisfactory to find that my experimental results have been verified in the most complete manner by Mr. Wheeler, who cannot for a moment, from the tenour of his paper, be suspected of having his experiments influenced by any prepossession against the undulatory theory of light. I regret however that we do not agree equally well as to the mode to be pursued in calculating the quantity of light transmitted or reflected, from the data furnished by the experiments. Mr. Wheeler speaks of the possibility of his continuing his researches, which induced me to believe that he would finally perceive the true theory of the comparative photometer, and in some later essay would give a correction of his former views; and from the polite tone of his paper, I should have much wished that it had come from his own pen rather than mine. Another reason for my not replying earlier to Mr. Wheeler was, that I had formed a resolution to avoid all scientific controversies whilst an undergraduate of this University.

The preliminary formulæ which Mr. Wheeler investigates are perfectly correct for the illumination received on a screen from any small luminous surfaces inclined at any angles to that screen; but the comparative photometer acts on a quite different principle. When we compare the reflective powers of two substances, for example, those exhibited by plane polished surfaces of diamond and glass, we view the images which these surfaces give of small portions of a long and narrow parallelogram of pasteboard, which is bent round into a semicircle, and equally illuminated in every part, the reflecting surfaces being placed at the centre of the semicircle. Now, all the parts of the pasteboard being, by supposition, equally illuminated, the images of the parts of it seen by the eye reflected in the surfaces of the glass and diamond, would appear equally bright if the same proportion of the incident rays were reflected by each. We do not regard the reflector, but only the image formed by it, according to the ordinary rules of catoptrics for plane reflectors; therefore it is immaterial at what angles of incidence the light be reflected, provided that the same proportion of the incident light be reflected by both; and conversely this must be clearly the case, when equally bright images are formed of equally bright objects.

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[*] Communicated by the Author.

To find the reflective power of diamond at various angles of incidence, when that of glass is known, we ascertain the angles of incidence in the comparative photometer at which the images appear equally bright. Thus suppose that for any given angle of incidence i_1 on diamond, the image is equally bright with that formed by glass at an angle of incidence i_2, then the parts of the pasteboard themselves being equally bright also, we have clearly as many rays reflected by diamond at an angle of incidence i_1 as by glass at an angle i_2. But since we can obtain large and plane surfaces of glass, we can determine its reflective power for all incidences by independent methods, and therefore when this is known we can by the comparative photometer deduce that of diamond from it, which cannot be obtained in surfaces sufficiently large for those methods. Hence the value of this method of photometry by comparison.

The comparative photometer is also serviceable in other cases, as for instance in determining the relative intensity of the light in Newton's rings seen by transmission, but in all cases the principle on which it acts is the same.

The above is the method I have pursued in the investigation to which Mr. Wheeler objects, and there is clearly no other formula involved in the problem than that by which we calculate the reflection by glass at any given angle of incidence.

The magnitude of the reflecting surfaces cannot in the least affect the brightness of the images formed by them; but the eye may nevertheless err in comparing illuminated surfaces of different magnitudes, as for instance a square inch of white paper laid on a dark substance may appear brighter than a square foot of white cloth similarly placed, on account of the contiguity of the dark substance, or the prejudice of the eye, although equal portions might appear equally bright; hence as a precaution of experiment, it is advisable, if not necessary, to compare areas of the pasteboard surface which are nearly equal in magnitude, and the reflector should therefore be so, or if one is much more inclined to the visual ray it ought to be larger proportionally.

I have been more particular on this point from its having apparently been in some measure the cause of Mr. Wheeler's missing the principle of the photometer, for he has considered the magnitude of the reflecting surface as a fundamental datum in determining the brightness of the image perceived by the eye.

In concluding, I must be allowed to observe, that the experimental part of my photometrical investigations on the relative intensity of the light in Newton's rings seen by transmission, and on the reflective powers of diamond and glass of antimony, having been confirmed by Mr. Wheeler's experiments, the calculated results press upon our attention with full force, the inadequacy of the undulatory theory of light, of which the results predicted are palpably at variance with the fact.

Cambridge, April 18, 1838.

LXXVI. *On the primary Forces of Electricity. By* RICHARD LAMING, *Esq., M.R.C.S.*[*]

1. THE experiments of Coulomb, and the more recent and beautiful manipulations of Mr. Harris, have completely established the fact, that in our atmosphere bodies dissimilarly electrified, attract one another with a force which varies inversely as the square of the distance. That the same ratio obtains in an ordinary vacuum might now be theoretically deduced from the results of experiments on the discharging distances of different electrical accumulations through air of various densities; but as the fact is of fundamental importance, I have thought it worth while to attempt its induction by more direct experiments.

2. In order conveniently to estimate the attractions of such minute quantities of electricity as are susceptible of being retained by a conductor placed in highly rarefied air, it became necessary to construct an electrometer that should be very delicate, and at the same time occupy but little space. Accordingly an elliptical beam 1¾ inches in length, made of a thin lamina of gilded mica, was supported as a balance on two fine needle points; a circular plate 1¾ inches in diameter, of the same material, was suspended by silver threads to one of the arms of the balance, and counterpoised by a weight attached by sliding straws to the opposite arm. The index, 2¾ inches in length, consisted of a needle of glass, carrying at its upper end a plate of mica, by means of which the scale could be read off without error from parallax. Two short straws, one sliding with friction within the other, were so adjusted under

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[*] Communicated by the Author.

the centre of gravity of all the moveable parts, that it might be placed no more below the points of support than was absolutely necessary; by which means the instrument was rendered so sensible that a weight = $\frac{1}{80}$th part of a grain moved the index through $4\frac{1}{4}$°; the $\frac{1}{40}$th part moved the index through $8\frac{1}{4}$°; the $\frac{3}{80}$ths moved it through $12\frac{1}{4}$°; and the $\frac{1}{20}$th through 16°; and as the scale might be read off to quarters of a degree, a force = $\frac{1}{1280}$th part of a grain was of course appreciable by its means. When used this electrometer was suspended with its circular plate over an uninsulated disc of gilded wood, by a wire cemented at about its middle into the axis of a glass tube, the latter sliding through an air-tight collar in the top of an exhausted receiver about $6\frac{3}{4}$ inches in diameter, and it was placed in connection with the inner coating of a Leyden jar exposing about two square feet of coating, and whose electrical charge was estimated by means of Mr. Harris's unit jar. The following results were thus obtained.

Table A.

Experiments showing that in highly rarefied air, the distance being constant, the force of electrical attraction varies as the square of the quantities of plus electricity.

Distance of attracting surfaces = ·5 of an inch.

Comparative Quantities.		Degrees moved through.		Same by Calculation.
1	...	1·	...	1
2	...	4·25	...	4
3	...	9·5	...	9

Table B.

Further experiments confirming the same fact.

Distance of attracting surfaces = 1 inch.

Comparative Quantities.		Degrees moved through.		Same by Calculation.*
13	...	1·25	...	1·35 +
16	...	2·	...	2· +
20	...	3·	...	3·2
25	...	5·	...	5·
30	...	7·5	...	7·2
35	...	10·25	...	9·8
40	...	13·	...	12·8
45	...	16·75	...	16·2
50	...	20·5	...	20·

Considering that the quantity of electricity employed was so small that its attractive force in no case exceeded the $\frac{1}{16}$th part of a grain, the accordance of these results with Coulomb's law is more perfect than could have been expected. The necessity for so minute a charge had previously been ascertained by finding, on the approach of the attracting surfaces being mechanically prevented, that a quantity of electricity the attractive force of which was under a grain, passed to the negative body in a diffused discharge.

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* Founded on the fourth result.

3. We may now notice a corollary to this law of electrical attraction which seems hitherto to have escaped notice; namely, the *definite* nature of the electrical attraction. It will be at once evident, that if a given quantity of electricity exerted an attractive force on common matter without reference to quantity, an electrical action set up between any two given bodies would, by Coulomb's law, continue constant so long as their distance from one another remained unaltered, whatever changes might be taking place in the relative situations of contiguous bodies. That this is not the case, we are assured by the commonest observation; for nothing is better known than the interception of the electrical force between two dissimilarly charged bodies by the intervention or near approach of a third.

4. By this we see that the attraction which is reciprocal between electricity and common matter, must be definite with regard to quantity as well as force; and as we examine the following pages we shall find such abundant and conclusive evidences of the same fact as will make it irresistible. The cause for the opposite opinion having been so long adhered to is the susceptibility of common matter to undergo an augmentation or diminution of its *natural quantity*,—a term, by the by, of itself sufficiently expressive of a certain undefined but commonly prevailing notion of *saturation*: this subject will shortly come more fully under consideration, when we shall take occasion to show that these changes in quantity are not only quite compatible with the alleged definite attractions, but a necessary consequence of a second, and, I believe, hitherto unsuspected electrical force, the entity of which it will then devolve upon me to establish.

5. Since then common matter is naturally associated with a definite quantity of electricity, we are required by strict philosophical reasoning to believe that each atom of that matter contributes its part to the general effect by attracting to itself its own equivalent of electricity; and for this doctrine we have been in a measure prepared by the admirable experimental researches of Dr. Faraday on the definite nature of galvanic analysis.

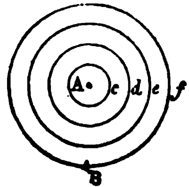
6. The law by which electrical are united to common atoms becomes therefore an important object of investigation; and if we be not immediately led to it we may derive from observation such a ratio as at least corresponds most exactly with all the facts. This ratio may be thus announced: *if any number of equal quantities into which we may suppose the electrical equivalent to be divided be denoted by numbers increasing in arithmetical progression, then the forces of these quantities on the common nucleus* will be expressed by numbers increasing in a certain geometrical progression, as in the scheme below:

Quantities	1st.	2nd.	3rd.	4th.	5th.	6th.	&c.
Forces	1.	3.	5.	7.	9.	11.	&c.

for instance, if the abstraction of one part of electricity from a common nucleus be resisted by an unit of force, the abstraction of a second part will be opposed by three units of force; a third part by five units, and so on. How this ratio is obtained will appear on comparing it, and the doctrine of the definite nature of the electrical attraction, with the known results of experiment.

7. Let the point A, in the annexed figure, be an insulated body charged with a given excess of electricity; then as the attraction of electricity is definite it will act on an equivalent of matter, *wherever it may be found*, with a force determined by the law of Coulomb. Suppose B to be a comparatively large mass of matter in its natural state of electrical saturation, or equilibrium, and uninsulated; then an equivalent of common matter in B will be acted on electrically in two directions; first, by the plus electricity in A, and again by the electricity possessed by itself; now as each of these forces is to the other a retarding force, whenever they are in equilibrio, either may, of course, be expressed in the terms of the other.

Fig. 1.



Let the circular lines *c d e f* represent portions of spherical surfaces increasing in distance in arithmetical progression from the point A; and suppose B to be placed successively on all these lines; then if while on *f* it be acted upon by an unit of force, at *e* it will, according to the law of Coulomb, be under the influence of $1\frac{7}{9}$ units of force, at *d* under four units of force, and at *c* of sixteen units; and as the quantity of common matter in B acted upon is constant at all distances, being the equivalent of the constant quantity of plus electricity in A, these numbers without alteration will express the forces by which the plus charge and the minus matter tend to come together; or in other words, *the quan-*

tity of plus electricity being constant, its attractive force will vary as the square of the distance inversely.

8. All things remaining as before, let the quantity of plus electricity in A be doubled; then its action on B at either of the distances *c d e f* will also be *doubled.* A second quantity of electricity in B held with three times the force of the first quantity (6.) will accordingly be dismissed from connection with its common matter; and the latter in consequence be attracted by A with a force equal to the sum of these two forces, that is $1 + 3 = 4$.

By the same action a third quantity in A will liberate a third quantity from B held with five units of force, and thus attract B with an intensity compounded of the three several forces $1 + 3 + 5 = 9$; a fourth quantity in A will attract B, with a force $= 1 + 3 + 5 + 7 = 16$; and so on; or, in other words, *the distance being constant, the attraction will vary as the square of the quantities directly.*

9. We thus arrive at two theorems, the latter of which has been hitherto an inexplicable anomaly. Philosophy demands that every effect should be, under the same circumstances, exactly proportionate to its cause; but in this case, without any change of circumstances appearing, a two-fold influence was observed to produce a quadrupled effect; a treble amount of causation nine times the amount of effect, and thus onward, the effect continually increasing as the *square* of the quantities. An explanation of this fact has never, so far as I am aware, been attempted; it has excited the surprise of one of the most acute electricians of the present day, who supposes a portion of the electrical force to be "masked by the operation of some peculiar influence;" but the remark furnishes no elucidation.

10. By compounding the preceding theoretical ratios we obtain a third theorem, equally important and induced from facts as the other two; namely, *the attraction being a constant force, the distances vary as the quantities of electricity directly.* If a table be drawn up in the following manner, the total force of any number of quantities for any given distance may be arrived at by the addition of a corresponding number of elementary forces in the appropriate column.

Quantities.	1	2	3	4	Distances.
First.........	1	$\frac{1}{4}$	$\frac{1}{9}$	$\frac{1}{16}$	Elementary forces.
Second......	3	$\frac{3}{4}$	$\frac{3}{9}$	$\frac{3}{16}$	
Third	5	$1\frac{1}{4}$	$\frac{5}{9}$	$\frac{5}{16}$	
Fourth	7	$1\frac{3}{4}$	$\frac{7}{9}$	$\frac{7}{16}$	

11. These principles explain the nature of electrical *induction*, which they show to be a function of the electrical force, whereby electricity separated from its natural connection obtains what may be called *compensation* by acting on its equivalent of other common matter. Thus compensated and their compensating bodies form with each other a temporary or artificial state of equilibrium, perfect in direct proportion as their compensation is perfect.

12. We have supposed B, fig. 1. (7.) to be uninsulated; but it is very obvious that were it either insulated, or itself an insulating substance, the action of A upon it would be precisely the same; only in the latter cases it would be rendered *similarly electrical*, in consequence of the retention of plus electricity virtually dismissed from connection with its common matter.

13. Before we enter more fully on the subject of the distribution of plus electricity on insulated conductors, we are led to prove the existence of a new electrical force, by virtue of which the electrical conditions of bodies become altered. The most simple indication which we have of this force is observed in the phænomenon of the electrical spark, in which atoms of electricity are closely associated together by reason, as is commonly taught, of the atmospheric pressure overcoming their repellency, or, as I conceive, in consequence of their mutual attraction for one another. We may premise that the former of these explanations is not consistent with facts; for, let the atmospheric pressure be given, then, the repellency being constant, the density of equal masses of electricity should always be the same, which is not true; for a given quantity of electricity may be passed in an aggregated form between two conductors through a certain thickness of rarified air, whereas through a greater thickness it will pass only as a diffused stream. Again, I find that electricity passes as a dense and brilliant spark in the most perfect vacuum that can be procured when the conducting wires are separated from each other only by a plate of mica; and in this case at least the aggregation of the atoms must be ascribed to some other cause than external pressure.

14. While pursuing this inquiry, it occurred to me that the nature of the force exerted by the electrical atoms for one another might be best ascertained by observing the effect produced by the mass of electricity in the earth on a certain quantity accumulated in a conductor; and since the force whose nature was sought was probably as great as the very feeble force of gravitation, it seemed not unreasonable to ex-

pect that it might, like gravitation, be appreciable by a common balance; indeed the thought had then occurred, which since I have found abundant reason to be satisfied with, that the second electrical force was no other than the cause of gravitation itself.

15. To determine the nature of this electrical force, I therefore proposed to find in the case of a given body, first, whether its tendency to approach the earth remains constant, while the quantity of electricity in it is made to vary; and secondly, if its tendency be not constant, whether it varies with the quantity of electricity *directly* or *inversely*, and which of course would depend on the nature of the force exerted by the electrical atoms on one another. But any alteration in the quantity of electricity natural to a body causes it to be attracted by surrounding bodies; and this attraction in respect to the force to be investigated may be either an accelerating or a retarding force; this attraction then, as well as the gravitating force or weight of the body, requires to be so accurately counterpoised, that there should be no tendency in it to motion in any direction; and this I accomplished by the following arrangements.

Experiment C.—Two circular plates twelve inches in diameter were made of card-board and covered with tinfoil; one was placed horizontally on an uninsulating support, and the other suspended by fine silver wires above and about twenty inches distant from it: by means of a micrometer screw this distance could be increased or diminished with great nicety. Midway between the plates was suspended, by a silk thread attached to one of the arms of a balance, a sphere sixteen inches in diameter, of caoutchouc covered with Dutch leaf, and weighing about 1900 grains: the counterpoise at the opposite arm of the balance being partially immersed in water, the index could be readily brought to zero, where it remained very steadily. Under these circumstances, whenever the sphere was charged, either positively or negatively, it became attracted by both the plates with forces greater as their respective distances were less.

The sphere being kept constantly in a negative state, the distance of the upper plate was so adjusted that the attraction between these two was barely greater than the force in the direction of the under plate; a fact, which of course was made known by a slight tendency in the sphere to ascend. All other things remaining the same, the charge was then changed to positive, when, in addition to the attraction of the sphere by the two plates, there was also an increased quantity of force between the electricity in the sphere and the electricity in the earth; and the attractive nature of this force was rendered manifest by the immediate *descent* of the sphere.

Experiment D.—The preceding experiment was next modified in the following manner. The sphere being kept in a positive state, its distance from the upper plate was so much diminished that the attraction between them failed only a little of counterpoising the united forces of the sphere's attraction for the lower plate and its increased tendency to descend whilst sustaining the plus charge. Under these circumstances, the sphere evinced, of course, a slight tendency to descend; but on making the charge negative, this tendency was instantly changed for a more powerful one in the opposite direction, and the sphere ascended.

These experiments have been repeated too frequently for any doubt to remain of their accuracy; and it is thought that they place the lesser electrical attraction beyond the reach of rational controversy. Long-cherished attachments to old and opposite opinions may, perhaps, meet this new fact with hesitation; but when in the subsequent pages of this paper its consequences shall be disclosed, its claim will, doubtless, be recognised and admitted.

The major and minor electrical attractions as thus represented, I have applied somewhat extensively as prime physical forces; and with a success that could hardly have attended investigations based on erroneous data. I hope hereafter to make this appear, but at present I shall confine the application of the theory to such facts as are commonly understood to appertain to the science of electricity; commencing by showing its sufficiency to elucidate the phænomena attendant on the accumulation of free or plus electricity in common matter.

16. We know by facts that free electricity brought into contact with an insulated conductor in its natural electrical state may attach itself to it; by the theory this result cannot be due to the major attraction, for the attraction of the conductor for electricity is definite and already saturated (3.); and it may be due to the minor electrical force, which acting between the electricity natural to the conductor and the free electricity, would tend to accumulate the latter about the centre of gravity of the former. But free electricity in one body is always attracted with a great absolute force by the minus matter of some other body compensating it (7.); and by Coulomb's law this force is greater as the distance is less: by extreme distance it might of course be so much reduced as to be even inferior to the minor force retaining it; but under ordinary

circumstances the compensating body is contiguous (generally, the atmosphere); and by which the free electricity is attracted to the bounding surfaces of the insulated conductor.

17. The disparity of the electrical forces seems to be so great, that the minor has very little influence in determining the *quantity* of free electricity accumulated on different parts of a conductor of irregular figure, as all combinations of conductors of dissimilar figures or dimensions may be considered; that being almost wholly regulated by the major attraction.

To understand this, let us suppose an insulated conductor to be charged positively and submitted to the sole influence of a compensator whose distance is given. If we conceive its free electricity to have been received in two successive and equal parts, then the retarding force to the first part will have been an unit of major attraction in the compensating body, and the retarding force to the second part three units of major attraction (6.). Now let a second perfectly similar conductor, with a perfectly similar compensator, acting at the same distance, be placed in contact with it; if the second conductor have been previously charged to the same extent, the major attraction will not tend to disturb the electrical state of either; but if the second conductor be unelectrified, it will equally divide the charge with the first conductor, since the treble amount of retarding force to the second increment of charge thereby becomes reduced from 3 to 1. The same mode of reasoning applies either to any greater number of similar conductors, or to any multiplication of the original charge.

18. Again, let the plus charge of a body whose compensator is given, be divided into three equal parts; and a second precisely similar body, but having *two* such compensators, instead of one, be brought into momentary communication with it: by the same reasoning that we used in the former case we see that this second body will abstract two thirds of the total charge of the first; for the retarding force of two of the units of charge will thereby become reduced from $5+3=8$ to $1+1=2$.

19. If instead of two compensators the second conductor have only one, but that one at half the original distance, the result will still be the same: that is to say, the second conductor will receive two parts and the first conductor retain one part of the free electricity; for although the retarding force in the second compensator to the action of two units of charge be four times greater than in the last case, the force of those two units on the compensator will be four times greater also.

20. According to these principles, all insulated conductors

placed in communication with one that is positively charged, will acquire quantities of free electricity proportionate to the perfection of their compensation respectively; and the same thing is also true of all the several parts of any charged conductor of irregular figure.

21. Whenever charged bodies are freely insulated in the atmosphere, the air by which they are immediately surrounded is the nearest body, and therefore their compensator; hence the quantity of electricity that may be accumulated in such a conductor under any given intensity will vary in the simple ratio of the *quantity of air* within a given distance of its surface (18.).

22. Now, in the first place the quantity of air within a given distance of an electrified surface varies directly as its density; and that the proportionate quantity of electricity susceptible of accumulation in a conductor varies directly as the density of the air, has been completely proved by experiment*. Hence the reason of the very minute quantities of electricity only that can be accumulated on conductors placed in what we are accustomed to speak of as a vacuum.

23. In the second place, the density of the atmosphere being given, the quantity of air within a given distance of the surface of a conductor of irregular figure will be greater as its several parts are more angular or projecting; for example, the cubic contents of the immediate aërial envelops of points are greater than in the case of equal surfaces of any other figure; the cubic contents around convex surfaces are greater than those which are opposite to planes; and the latter again than when the surfaces are concave; the aërial envelops of plates of equal area are more voluminous as their perimeters are proportionately more extended; and the volumes of air inclosing spheres are proportionately greater as the spheres are less. Now the accordance of these several deductions with facts will be recognised on comparing them with the well-known experiments on this subject by Coulomb and Harris.

24. The latter philosopher has proved that a charged parallelogram will equally divide its plus electricity with either a cylinder or a prism into which the parallelogram may be supposed to have been formed†; a result which, making very trifling allowances for unavoidable inaccuracies, is quite in accordance with the principles we are considering, for the cubic contents of the compensating atmosphere of those different figures would not be very different.

25. Mr. Harris has also shown in the same series of ex-

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* Phil. Trans. 1834, p. 228, par. 44 and 45.

† Phil. Trans. 1834, p. 233.

periments that when circular plates of different sizes are charged and brought into contact, each retains a quantity of free electricity directly proportionate to its area; now plates of this figure are always enveloped in compensating atmospheres exactly proportionate in volume to their sizes, for their perimeters and their superficies bear a constant relation to each other.

26. We have hitherto considered the distance of the compensator to be given, and have found that its influence varied in the direct ratio of its quantity; and we now have to remark that if its quantity be constant and its distance variable, the quantity of electricity susceptible of being accumulated in a conductor under its influence will vary in the inverse ratio of the distance; for the quantity of electricity is as the compensation, and compensation varies as the square of the distance inversely, and as the square of the quantities directly.

27. Besides distance and quantity there is yet another cause of fluctuation in the efficiency of compensating bodies, and which we find in their natures. There are many reasons for believing that the atoms of all the different sorts of common matter are not combined with equal quantities of electricity; indeed, if the minor electrical attraction be acknowledged to be the cause of gravitation, the quantities of electricity around atoms of different sorts will be directly as their respective weights; and all that we know about the electrical attractions leads us to the conclusion, that if there be electrical atoms around common nuclei, those at the greatest distances will be held with the least forces.

28. If therefore two compensators whose natures differ as we have thus imagined be alike exposed to the same action of free electricity, the major attraction in each of the atoms will be a retarding force to the major attraction acting on it from a distance; and these will be in equilibrium at a certain point of distance from each of the common nuclei. *But this point of distance will not be the same in the two bodies.* Now as all the electrical atoms more distant than that point from their respective common atoms will accordingly be set free, the numbers liberated will not be the same, and therefore the compensating powers of the two bodies will not be equal. This theoretical inference was thus tested by experiment: a pane of glass eleven inches by fifteen inches, being coated in the middle with different substances in succession, one of its coatings was uninsulated, and the compensating power of the material of which it was formed estimated by the attractive force of a given charge on an uninsulated disc suspended to the arm of a balance in the opposite direc-

tion. The induction of this experiment will be easily arrived at if we bear in mind that compensation being a constant function of the major electrical force is necessarily appreciable by any measure of that force; and that it may be set up by any given accumulation of free electricity in two or more compensators at the same time, in opposite, or any directions.

Table E.

Diameter of attracted disc, 4·75 inches; distance, ·6 of an inch.

	Comparative Quantities.	Comparative Forces.
Glass coated with thick paper	30	6
	43	12
	60	24
Plus coating of tinfoil, the uninsulated coating of thick paper	32	6
	46	12
	65	24
Both coatings of tinfoil	60	6
	86	12
	121	24

The results in the preceding table plainly show that the quantity of electricity susceptible of accumulation in conductors varies with the *nature* of its compensator.

The doctrine of compensation thus explains with great precision the accumulation of different quantities of free electricity on conducting surfaces of dissimilar figure, under an equal force of major attraction, or, as it is with sufficient propriety called, under an equal intensity; and it enables us to understand, however greatly the quantities of electricity in equal surfaces of different figures may vary, that when the atmosphere is the only compensator, the compensating envelope opposite to all the parts will have an uniform thickness.

29. Hence if a conductor of irregular figure be charged with electricity and freely insulated in the atmosphere, and an uninsulated solid be made gradually to approach the conductor at all points of its surface in succession, it will begin to assume the compensation at exactly the same distance in all the cases; and therefore it is true that the distance at which a charged conductor can attract bodies is the same at all parts of its surface.

30. But as the penetrating body can under no circumstances wholly displace the air around a conductor, whatever be its figure, it can never wholly assume the compensation; and consequently the whole force of attraction of any electrified

body can never be concentrated on any one solid body. Amongst other facts of which we are instructed by this theoretical conclusion, we learn why the electrical condition of the uninsulated Leyden coating can never become so highly intense as the opposite coating to which the charge is directly communicated.

What has been said may suffice to explain that the principle of compensation is one of the necessary consequences of the definite nature of the major electrical attraction; but the evidences are far from being exhausted. I have in this paper endeavoured, and shall continue in those which are to follow, to test the new theory when practicable by reference to experiments already known and by facts generally acknowledged; rather than to adopt others that might, perhaps, have been regarded as partial in their applicability or complicated in their conditions.

London, April 17, 1838.

[To be continued.]

LXXVII. *Specimen of a Thermometrical Diary kept abroad in the Years* 1824, 1825, *and* 1826; *and compared with a corresponding one made in London during the like Period. By* JOHN HOGG, *Esq., M.A., F.C.P.S., M.R.G.S., &c., Fellow of St. Peter's College, and late one of the Travelling Bachelors, in the University of Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE following specimen of a diary of observations made with the external thermometer, graduated on the scale of Fahrenheit, during an excursion on the continent in the summer of 1824, and also during more extensive travels abroad in the years 1825 and 1826, I now beg to offer you, even after such a lapse of time as twelve or fourteen years, for the purpose of impressing upon future travellers, if not the necessity of making similar observations, at least that a great degree of utility and interest may be attainable to science, by keeping careful diurnal registers of the temperature of the atmosphere during their absence from England, so that upon their return home they may compare them with observations taken in England during those identical days.

I have only thought it necessary to insert in this specimen a very small portion of my Diary,—indeed, just sufficient to enable your readers to see the manner I adopted of entering the daily observations, and to understand the comparative *abstract* of the temperatures subjoined hereto.

The *hours* which I selected for observation were *ten* o'clock in the *morning* and *ten* o'clock at *night*, not because they were the best adapted for taking the differences of the day's temperature in general, but because I soon found from experience that they were the most convenient, and those on which I could rely with the greatest certainty for being able to continue uninterruptedly similar registries. In making the morning's observation, I, as carefully as the situation would allow, suspended the thermometer to the north, or most shady spot, and at such a height above the ground as to prevent any effect from reflected heat, or from the influence of the rays of the sun being increased by any closely adjacent wall, or roof, or water, &c. If, however, as it sometimes happened, it was either too much past the stated hour, or I could not meet with a proper situation at that exact hour, I preferred entirely omitting such observation, rather than make one which would have been manifestly incorrect; hence, where a vacancy occurs in the 1st or 2nd columns of the annexed portion of my Diary, it shows that I was prevented from correctly observing the thermometer, for either one or both of these reasons.

I have added in the 3rd and 4th columns similar observations made with Fahrenheit's thermometer in England, for the sake of comparing the difference between the atmospherical temperature of the place where I then was, with that of the English metropolis upon the same day. This statement I have taken from the "Meteorological Diary, by W. Cary," London, published monthly in the 'Gentleman's Magazine.' The editor of that able periodical has kindly informed me, "that Mr. Cary made his thermometric observations three times daily, in the Strand, in the years 1824, 1825, 1826; that the thermometer was out of doors, and in nearly a due north aspect." (See Gent.'s Mag. for March, 1836, vol. v. No. III. p. 218.) Although, as it will be seen, Mr. Cary made his *morning's* observation *two* hours *earlier*, but that of the *night one* hour *later**, than I had been wont to do with mine; yet, upon the whole, they come the nearest to my own hours of any of the London registers of the thermometer with which I am acquainted; for those of the Royal Society, as at present published in the Transactions of that body, are taken at 9 o'clock in the morning and 3 o'clock in the afternoon; which hours, perhaps, it would be advisable for any traveller in foreign parts, who intends to keep a daily account of the atmospheric temperature, to adopt for his hours of contemporaneous observation*.

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* But of course I need scarcely say, that there will be some variation as to the *corresponding* hours according to the difference of longitude which, however, can easily be ascertained.

I have likewise given a specimen of an *Abstract*, showing in what manner the highest, lowest, and mean temperatures of several principal places in Europe, taken from my *Diary*, may be compared with those of London during the identical periods.

The recent appearance of the admirable "Instructions for making and registering meteorological observations," principally drawn up by one of the most learned philosophers of this century †, in the fifth volume of the Journal of the Royal Geographical Society, has induced me to lay before your readers this short paper; which, though in itself not very important or valuable, still, I trust, from affording a fair specimen of many accurate observations, may, to the comparative meteorologist, prove of some little interest; and more especially as an example to others, who are either about to travel, or to reside abroad ‡, I hope it may suggest a precedent for keeping a similar diurnal register of the temperature of the weather; because by so doing, we may practically expect to render more certain our present confined knowledge of the laws of climate.

I remain, Gentlemen, yours, &c.

London, April 28, 1838. JOHN HOGG.

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* The "Instructions" subsequently alluded to propose the following hours of observation and registry; namely,

Morning, 8 A.M. Afternoon, 2 P.M. Evening, 8 P.M.;

but I would here recommend these as preferable,

Morning, 9 A.M. Afternoon, 3 P.M. Evening, 9 P.M.

because the morning's and afternoon's observations may then be compared with those taken out of doors at the Royal Society's apartments in Somerset House, in London.

† Sir John F. W. Herschel, at the Cape of Good Hope.

‡ I will remark that this may also prove a simple and useful precedent for registering, abstracting, and comparing the degrees of temperature in the more remote counties and districts of the British islands; a much neglected but important subject for inquiry in natural science.

Specimen of a Thermometrical Diary, 1824.

Day of the Month.	Place of Observation.	Observation. Abroad.		Comparison. Strand, London.	
		10 A.M.	10 P.M.	8 A.M.	11 P.M.
July 10.	Les Haut-Geneveys	65°		60°	
———	La Chaux de Fond		67°		62°
July 11.	Le Locle	67		60	
———	Neuchatel		69		62
July 12.	Anet	75		60	
———	Bienne (Biel)		70		63
July 13.	Bienne	71	72	63	72
July 14.	Soleure	77		72	
July 15.	Olten	71		60	
———	Baden		70		62
July 16.	Kaiserstuhl on the Rhine	73		66	
———	Schaffhausen		67		60
July 17.	Schaffhausen	74	73	64	60
July 18.	Schaffhausen	75	66	60	60
July 19.	Eschert on the Zellersee	63		60	
———	Constance		58		59
July 20	Frauenfeld	62		59	
———	Zurich		59		61
July 21.	Zurich	70	63	61	60
July 22.	Einsiedeln		52	60	61
July 23.	Richterschwyl, Lake of Zurich	66		64	
———	Zurich		61		63
July 24.	Inn on Mount Albis	68		60	
———	Lucerne		69		60
July 25.	Lucerne	72	73	59	64
July 26.	Lucerne	76		66	
———	Rigi-culm		64		52
July 27.	Rigistaffel	65		55	
———	Altorf		70		58
July 28.	Amsteg (Steg)	68		59	
———	Andermatt		53		61
July 29.	Amsteg	70		60	
———	Stanz		64		55
July 30.	Sachslen, Lake of Sarnen	72		55	
———	Brienz		67		55
July 31.	Brienz	68		54	
———	Meyringen		65		60
Aug. 1.	Meyringen	66	64	59	54
Aug. 2.	Guttannen, in Ober-Hasli	67		54	
———	Meyringen		62		60
Aug. 3.	Chalet of the Schwarzhorn-Alp.	66		61	
———	Grindelwald		61		61
Aug. 4.	Grindelwald	65		61	
———	Lauterbrunnen		60		61
Aug. 5.	Interlaken	68		60	
———	Bern		64		60
Aug. 6.	Bern	66	65	60	58
Aug. 7.	Bern	69	66	58	60
Aug. 8.	Bern	65	63	61	61

Specimen of an Abstract of the highest, lowest, and mean Temperatures of some of the principal Places comprised in Mr. Hogg's *Diary as compared with those of London during the same Periods.*

Period of Observation and Comparison.	Place of Observation.	Observation. Abroad.						Comparison. Strand, London.					
		10 o'Clock a.m.			10 o'clock p.m.			8 o'Clock a.m			11 o'Clock p.m.		
		Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.
1824. Aug. 5 to Aug. 8, both inclusive...	Bern	69°	65°	66¼°	66°	63°	64½°	61°	58°	59¼°	61°	58°	59¼°
1824. Aug. 13 to Aug. 16, both incl.......	Chamouni	63	58	60¼	60	50	55¼	60	55	57¼	60	55	56¼
1824. Aug. 26 to Aug. 29, both incl.......	Milan	74	72	73	71	68	69¼	65	60	61¾	66	60	62¼
1825. June 8 to June 17, both incl.	Frankfort on Maine	75	64	71⅗	73	62	68⅖	67	55	60⅕	66	49	59 9/10
1825. June 28 to July 1, both incl.	Heidelberg............	72	68	70¼	65	59	62	56	55	55¼	57	55	56
1825. July 2 to July 5, both incl.	Frankfort on Maine	69	61	65¾	63	57	59¾	64	55	59	66	58	62¼
1825. July 13 to July 16, both incl.	Leipsick	74	72	73	73	67	69¼	81	60	69¼	72	67	69¼
1825. July 30 to Aug. 21, both incl.	Dresden	76	62	68⅝	73	59	64 18/23	70	54	59 17/23	73	54	60 4/23
1825. Aug. 24 to Aug. 31. both incl. ...	Berlin..................	71	68	69⅞	66	60	62⅞	68	55	60⅛	69	56	61⅛
1825. Sept. 3 to Sept. 8, both incl.	Dresden	66	62	64	62	55	57⅙	58	54	55⅚	57	49	54
1825. Sept. 14 to Oct. 25, incl., omitting Sept. 24, 25, and Oct. 10—13, incl.	Vienna	70	48	57 34/36	68	44	54 19/36	66	37	53 9/17	66	35	53⅙
1825. Nov. 29 to Dec. 15, both incl.......	Venice	60	48	53 14/17	59	48	52⅛	48	33	42 8/17	48	40	44 8/17
1825. Dec. 21 to Jan. 11, 1826, both incl.	Florence	60	42	51 1/22	55	43	49⅙	49	24	35	50	24	35⅘
1826. Jan. 19 to Jan. 31, both incl.	Rome	56	43	49 7/13	48	36	41⅛	44	29	36	43	30	36 2/13
1826. During the month of February......	Rome	58	48	52 11/28	51	46	41 11/28	49	30	42⅞	50	31	42⅓
1826. March 1 to April 4, both incl. ...	Rome..................	62	49	54 7/33	56	44	49 11/35	52	33	41 13/35	56	30	40⅙
1826. April 8 to April 30, both incl. but April 25 and 26 excepted...........	Naples	66	58	62 14/21	61	50	55 13/21	58	38	48 7/13	53	34	44⅓
1826. May 1 to May 16, both incl. omitting May 3, 13, and 14........	Naples	66	59	62 3/13	60	52	55 7/13	51	40	46⅙	56	39	45 5/13
1826. May 18 to May 21, both incl.......	Messina	68	63	66	63	59	61	60	52	55¼	58	46	53¼
1826. May 25 to May 28, both incl.	Catania	68	67	67½	66	64	65	56	55	55¼	55	50	52
1826. June 10 to June 21, both incl.	Palermo	74	69	71	72	65	68 5/12	69	40	60⅙	65	54	58⅓
1826. June 23 to July 4, both incl.	Naples	77	70	74 5/12	74	68	70⅔	74	56	68¼	71	56	66¼
1826. July 6 to July 13, both incl..........	Rome	81	78	79⅝	76	71	74	73	60	69⅛	72	64	68
1826. July 20 to July 23, both incl.	Pisa....................	79	76	77¼	74	70	72¼	61	56	59¼	61	52	57¼
1826. July 24 to Aug. 2, both incl.	Bagni Caldi di Lucca	78	72	74⅕	66	63	64⅖	75	54	65⅗	74	56	64⅗
1826. Aug. 7 to Aug. 20, both incl.	Genoa	84	77	80 3/14	81	76	77 11/14	74	60	65¼	69	56	62 5/14
1826. Sept. 9 to Sept. 19, both incl.......	Munich	67	58	63	66	56	59 10/11	60	50	55	56	50	52 4/11
1826. Sept. 21 to Sept. 25, both incl......	Stuttgard	68	55	60⅕	48	47	47⅕	59	49	53⅕	60	45	53⅗
1826. Sept. 27 to Oct. 1, both incl.	Strasburg	67	63	65	65	60	62⅕	60	57	60	60	49	56
1826. Oct. 11 to Oct. 23, both incl.	Brussels	65	58	61 [illegible]	69	54	58 [illegible]	60	45	[illegible]	60	48	59 [illegible]

LXXVIII. *Observations on* Dr. Buckland's *Theory of the Action of the Siphuncle in the Pearly Nautilus.* *By* THOMAS WRIGHT, *M.R.C.S.*[*]

ALTHOUGH the valuable memoir by Professor Owen on the *Nautilus pompilius* has thrown a new and important light upon the history and organization of siphoniferous cephalopods, still, however, much remains to be learned of the singular structure of this interesting group of the *Mollusca.* From the announcement made by Professor Owen in his article "Cephalopoda," in the Cyclopædia of Anatomy and Physiology, I was led to expect that Dr. Buckland's Bridgewater Treatise would contain a satisfactory explanation of the action of the siphuncular apparatus of these mollusks. Whilst I admire the tone, talent, and highly popular style of the Bridgewater essay, still I am of opinion that the learned author's theory of the action of the siphuncle is at variance with the facts revealed by the dissection of the animal. On this subject Dr. Buckland observes: "The last contrivance, which I shall here notice, is that which regulates the ascent and descent of the animal by the mechanism of the *Siphuncle.* The use of this organ has never yet been satisfactorily made out; even Mr. Owen's most important Memoir leaves its manner of operation uncertain; but the appearances which it occasionally presents in a fossil state supply evidence, which taken in conjunction with Mr. Owen's representation of its termination in a large sac surrounding the heart of the animal, appears sufficient to decide this long-disputed question. If we suppose this sac to contain a *pericardial fluid,* the place of which is alternately changed from the pericardium to the siphuncle, we shall find in this shifting fluid an hydraulic balance or adjusting power, causing the shell to sink when the pericardial fluid is forced into the siphuncle, and to become buoyant, whenever this fluid returns to the pericardium. On this hypothesis also the chambers would be continually filled with air alone, the elasticity of which would readily admit of the alternate expansion and contraction of the siphuncle, in the act of admitting or rejecting the pericardial fluid[†]." In order to estimate the value of this hypothesis, it is necessary to inquire whether the nautilus spends the greater portion of its existence at the bed of the sea, or navigates the surface of its waters. The few authenticated instances where this mollusk has been seen at the surface, when compared with the thousands of its shells which are annually imported into Europe, affords *prima facie* evidence that the nautilus is an inhabitant of the silent depths of the sea; but when we inquire whether the organization of the animal sanctions this inference, we discover in its anatomy peculiarities of structure to adapt it to such a mode of life, the function of which it is impossible to mistake. The number and rudimentary condition of the cephalic appendages, the presence of a ligamento-muscular disc analogous to the foot of gasteropods, and adapted as a locomotive instrument for creeping along the bottom, the simple structure and pedunculated character of the eyes, the dense calcareous nature of the jaws, the structure of the digestive organs, but above all the contents of the stomach, which consisted, according to Owen, of the remains of a species of crab*, constitute an assemblage of characters which enable us to pronounce the manor of this mollusk to be the bed of the sea, where it preys upon crustacea and other invertebrata. But the nautilus has been seen occasionally at the surface of the water, and the question naturally arises, what are the conditions necessary to accomplish its ascent and descent?

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[*] Communicated by the Author.

[†] Bridgewater Treatises, VI. Geology and Mineralogy, vol. i. p. 325.

1st. That the animal should be capable of rendering itself specifically lighter and heavier than the ambient element.

2nd. That the mechanism by which this act is accomplished should be under the control of its will.

Now, Professor Buckland's theory allows only of *a change of place* in the adjusting fluid, from the pericardial cavity into the siphuncle, and *vice versâ*; consequently the specific gravity of the *entire animal* remains the same. The accompanying outline †, from Owen's dissections, shows the relative position of the internal organs: *a a* is the enveloping fleshy mantle dissected off to expose *b b*, the branchiæ floating in *c*, the branchial chamber for the reception of the water; *d* is the heart with its large vascular canals surrounded by clusters of glandular follicles, *e e*. The capacious pericardium *ff* is laid open to show its boundary and relation to the central organs of the circulation; it is partially divided internally by thin muscular septa, *g g*.

From the posterior wall of this musculo-membranous bag there proceeds a canal, or siphuncle, *s s*, destined to traverse all the chambers of the shell: the arrow shows the direction of this aquiferous tube. Anteriorly the pericardium communicates with the branchial chamber *c*, by two apertures *h h*, through each of which a bristle is passed to indicate the channels of communication. From this arrangement it is evident that the pericardial bag has three openings, *one* behind which conducts the fluid into the siphon, and *two* before which open into the branchial chamber, into which the sea water is constantly flowing to bathe the respiratory organs. With this mechanism before me, I humbly submit whether it is not a reasonable inference to suppose *that the sea water alone is the ballast by which the nautilus is retained at the bottom, and its ejection the means by which the animal is enabled to rise to the surface at pleasure.*

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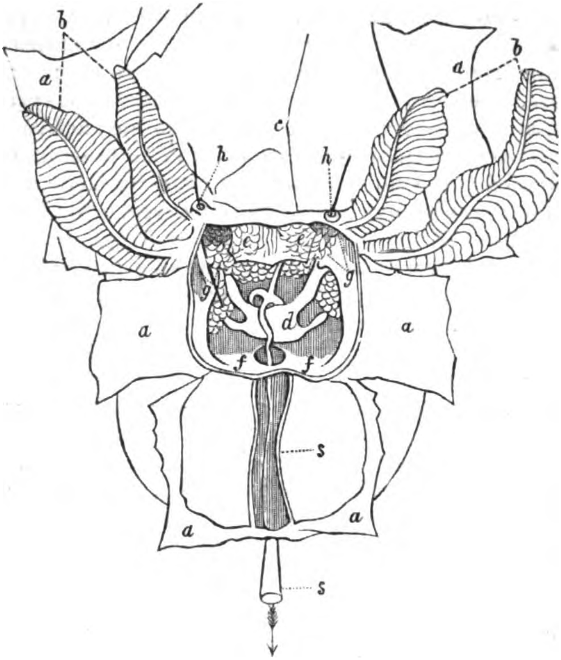
* Art. *Cephalopoda*, Cyclopædia of Anatomy, p. 531.

† For splendid figures of the animal and shell of the *Nautilus pompilius*, consult Dr. Buckland's 31st and 34th plates; also Prof. Owen's invaluable memoir.



Thus by relaxing the anterior orifices, *h h*, that communicate with the common branchial chamber, the water would flow into the pericardial sac, and from thence into the siphon *s*: during this distended condition of the apparatus the animal and shell would be specifically heavier, and the nautilus, in obedience to a prescribed law, would remain at the bottom without any muscular effort on the part of the animal to retain itself in that situation. Let us suppose that it is the will of the animal to rise to the surface; by calling into action the muscular layers of the pericardium, its watery contents would be ejected through the two orifices *h h*, a partial vacuum would be thus produced, the remaining portion of the fluid which filled the siphuncle would flow into that cavity, and from thence be ejected from the body: it is clear, therefore, that the nautilus would be thus rendered specifically lighter, and would consequently ascend to the surface. When it wishes to descend, it has only to admit the water through the orifices *h h*, the siphuncular apparatus would be again distended, its gravity increased, and its descent to the bottom accomplished*.

This explanation of the action of the siphuncle is applicable to the various modifications of structure observed in the mechanism of that tube, and avoids the many serious objections which may be reasonably urged against Dr. Buckland's hypothesis:

1st, We have not sufficient evidence to support the supposition that the air contained in the chambers of the shell undergoes compression; on the contrary, we find that the *Nautilus Sypho* from the tertiary strata of Dax possessed a calcareous siphon, which passed through the entire chamber and entered the aperture in the adjoining plate; and it can be demonstrated that the spirula has a calcareous siphon of an analogous structure extended along the concave side of the shell, so that in these mollusks the siphuncle is a *continuous calcareous tube* incapable of dilatation, and consequently their ascent and descent in the water was accomplished *without those conditions on which Dr. Buckland's hypothesis rests*, i.e. the dilatability of the siphon, and compression of the confined air.

2nd, I have already shown that the nautilus is peculiarly adapted for seeking its prey among the myriads of invertebrata that crowd the bed of the sea. Now according to Dr. Buckland, "When the arms and body are expanded, the fluid remains in the pericardium, and the siphuncle is empty, and collapsed, and surrounded by the portions of air that are permanently confined within each air chamber; in this state, the specific gravity of the body and shell together is such as to cause the animal to rise, and be sustained floating at the surface[*].” If this explanation be correct, the nautilus cannot remain at the bottom unless the siphon is distended by the retreat of the animal into the last chamber of its shell. What a prodigious muscular effort must, therefore, be constantly required to keep the nautilus at the bed of the sea! Again, it may be asked, how is the nautilus to seize its prey at the bottom, seeing the instant its head is protruded to search, and its arms expanded to seize it, “the fluid would be forced back again into the cavity of the pericardium, and thus the shell, diminished as to its specific gravity, would have a tendency to rise[†]”? This theory, therefore, is at variance with the inference obtained from an examination of the organization of the nautilus, that it seeks its prey at the bottom, and is but an occasional visitor at the surface: it is opposed likewise to a well-known law of the animal œconomy, that mechanical contrivances are always substituted, where long-continued action is required, to œconomise the expenditure of muscular power; a familiar example is afforded in the *Conchifera*, where an elastic ligament is employed to keep open the valves of the shell, adductor muscles being furnished for the occasional closing of the same. If we test the theory by this principle, we find that to keep the tube distended and the air compressed, in order that the nautilus may remain at the bottom, a constant muscular effort would require to be sustained, in order to overcome the elasticity of the confined air, the expansion of which is, according to Dr. Buckland, the power by which the siphuncle is emptied of its aqueous contents. The explanation which I have ventured to propose is in perfect harmony with the œconomical law alluded to; for whilst the nautilus is at the bed of the sea, the muscular powers of the pericardium would be in a passive state, just as the adductor muscle of the conchifer is in a state of repose when the valves are kept open by the elastic hinge; *no effort would be required to keep the animal in its natural situation*; as in the *Conchifera*, so it may be in the nautilus; an effort of the will shuts the valves of the former, and the contraction of the pericardium, by ejecting the watery ballast from the siphuncle of the latter, would allow it to change its feeding ground, ascend to a higher stratum of the water, or to its surface if required.

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* This explanation was proposed in a course of lectures on Fossil Zoology, which I delivered at the Philosophical Institution of this town, (Cheltenham) a report of which the Editor of the Naturalist has kindly inserted in the last number of his valuable periodical.

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[*] Bridgewater Treatise, vol. i. p. 329 note.

[†] *Ibid.*, p. 330.

3rd, In reviewing the nature of the peculiar glandular appendages that surround the large vascular canals of the nautilus, I deem it an unfair inference to suppose that they are the organs that secrete the fluid which circulates in the siphuncular apparatus, seeing that the same modifications of glandular structure exist in the dibranchiate cephalopods which are destitute of a siphoniferous shell: the true function of these follicular bodies is a physiological problem that yet remains to be solved.

Nuneham House, Cheltenham, May 8th, 1838.

LXXIX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

Anniversary Address of the Rev. W. Whewell, M.A., F.R.S., *President.*

[Continued from p. 440.]

IN attempting a rapid survey of the contributions to geological knowledge which have come under our notice during the past year, I may perhaps be allowed to advert to a distinction of the subject into Descriptive Geology and Geological Dynamics; the former science having for its object the description of the strata and other features of the earth's surface as they now exist; and the latter science being employed in examining and reducing to law the causes which may have produced such phænomena. We appear to be directed to such a separation of our subject by the present condition of our geological studies, in which we and our predecessors have accumulated a vast store of facts of observation, and have laboured with intense curiosity, but hitherto with very imperfect success, to extract from these facts a clear and connected knowledge of the history of the earth's changes. Nearly the same was the condition of astronomy at the time of Kepler, when the accumulated observations of twenty centuries resisted all the attempts of that ingenious man and his contemporaries to construct a science of physical astronomy. But though checked by such failures, they were not far from success; and when for the next succeeding century philosophers had employed themselves in creating a distinct science of Dynamics, the science of physical astronomy, full and complete, made its appearance, as if it were a matter of course; and thus showed the wisdom of separately cultivating the study of causes, and the classification of facts.

DESCRIPTIVE GEOLOGY.

If we begin with geological facts, our attention is first drawn to that district on the earth's surface within which the facts have been subjected to a satisfactory comparison and classification, which may be considered, in a general way, as including England, France, Italy, Germany, and Scandinavia. The language which the rocks of these various countries speak has been, in a great measure, reduced to the same geological alphabet. The questions of the determination of any member in one country, or the identification of similar members in two countries, are, for the most part, problems admitting of a definite and exact solution. In countries out of this district, on the other hand, we have not only to explore but to classify. We have to divine their geological alphabet;—to decipher as well as to read. We have not only to discover of what British rocks the observed ones are the equivalents, but we have to ascertain whether there be an equivalence; and where this relation vanishes, we have to discover what new resemblances and differences of members are most worthy our notice. The great difference in the nature of the geologist's task in these two cases seems to me to make it desirable to employ the familiar division of *Home* and *Foreign* Geology in a wider sense than has hitherto been common, including in the former all that region of Europe which has had its order of strata well identified with our own; this distinction then I shall employ.

1. *Home* (*North European*) *Geology*.—If we attempt, in this part of our subject, to follow an order of strata, we must begin with the oldest stratified rocks, though they are undoubtedly the most obscure; for the same reason which compels the historian of states to begin with the dim twilight of their savage or heroic times; namely, because at the other extremity of the series there is no boundary; since the events of past ages and their records form an unbroken series, leading us to the unfinished occurrences and works of to-day. Going then as far back as the historian of the earth can discern any light, and, for reasons which may hereafter be spoken of, shaping our course by the stratified rocks alone, we should first have to ask what addition has been made during the past year to our acquaintance with those formations which have generally been called *transition*. And here, gentlemen, many of you well know, that if I had had to address you at a period a little later, I might have hoped to be able to point out, among the labours of our members, some which may be considered as events of primary importance in this part of our knowledge;—steps which may be described as a new foundation rather than a mere extension of this portion of European geology;—a separation and arrangement of transition rocks, which is likely to become the type and classical model of that part of the geological series, as Smith's arrangement of the oolites became the type of that portion of the strata. I speak of Professor Sedgwick's views on the Cambrian rocks, which occupy the north-west of Wales, and Mr. Murchison's on the Silurian formations which cover the remainder of the principality and the adjacent parts of England. Mr. Murchison's work, which cannot but be one of first-rate value and interest, will, I trust, be in our hands in a few weeks; and I should grieve to think that Professor Sedgwick will be not only so unjust to his own reputation, but so regardless of the convenience and expectations of geologists, as to withhold from the world much longer the views which his sagacious and philosophical mind has extracted from the accumulated labour of so many toilsome years, on a subject abandoned to him mainly from its difficulty and complexity.

Turning then to the researches which have been laid before us upon the earlier stratified rocks, I am first led to notice the important memoir of the two gentlemen I have just mentioned, upon the structure of North Devonshire*. According to the views of these gentlemen, founded upon an extended examination of the county, this portion of England forms a great trough, having an east and west position, in which a series of culmiferous beds rest at their northern and southern extremities upon older rocks. The plants found in the culmiferous beds are said to be all identical with species which are abundant in the coal-fields of the central counties of England, and of the South Welsh coal basin: and it was at first conceived that these plants differed essentially from the scanty and imperfect remains of vegetables which are found in the older rocks. More recently, however, the same fossil plants which occur in the culm measures are said to have been detected in the subjacent strata. Before this fact was known, the identity of the fossils and the resemblance of mineralogical character seemed irresistibly to prove the culm-bearing beds of Devon to be the same formation with the culm or coal-bearing measures of Pembrokeshire on the opposite side of the Bristol Channel. How far this apparent anomaly admits of explanation, and in what manner it is to be allowed to modify the conclusion previously drawn, we may perhaps most properly consider as questions hereafter to be decided. The rocks which support the culmiferous formation on the north are conceived by Messrs. Sedgwick and Murchison to be a series, of which the last ascending term is probably of the date of the lowest portion of the Silurian system. On the south the culmiferous strata rest partly upon the granite, and partly upon the oldest slate rocks of Devon and Cornwall.

The same general view of the nature of the transverse section of Devon, and of the age of the culm, has been presented, perhaps I ought to say adopted, by the authors of two other papers upon the same region which have been brought before us,—Mr. Austen and Mr. Weaver †; and also, at least so far as the section is concerned, by the Rev. D. Williams in a communication made to the British Association in September last. Nor am I aware that it has been dissented from by any one who has examined the county in question since this view was made generally known. Resting on the concurrence of so many able observers, I should conceive, therefore, that we may look upon this view as *established*, so far as the time which has elapsed allows us to use the term. No truths should be termed incontestable till a considerable period has been left for the antagonists to show themselves and to try their force.

Although this view has thus so good a claim to acceptance, you are aware, gentlemen, that it is entirely different, both as to the form of the section and the age of the members, from that which was entertained up to the time when these gentlemen turned their attention to the subject. Their opinion respecting Devonshire being

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[* An abstract of this memoir will be found in Lond. and Edinb. Phil. Mag. vol. xi. p. 311.]

[† Abstracts of Mr. Austen's and Mr. Weaver's papers will appear in future Numbers.]

adopted, along with the views of the same eminent geologists respecting Cumberland and North and South Wales, one-third of our geological map of England will require to be touched with a fresh pencil.

Nor is this wonderful. It is rather a matter of extraordinary surprise, that when the rest of the geological map of England is again drawn, there are scarcely any but microscopic alterations which require to be made. No higher evidence can be conceived of the vast knowledge and great sagacity of its author.

Such modifications we must ever expect to have to make of a first approximation; and I should think it a misfortune to our researches if we should attempt to elude this necessity by giving up the key of all our geological knowledge of our country,—the doctrine that there is a fixed order of strata, characterized mainly by their organic fossils. If we have not advanced so far as to prove this, what have we proved? If our terms do not imply this, what is their meaning? Is it not true, in our science as in all others, that a technical phraseology is real wealth, because it puts in our hands a vast treasure of foregone generalizations? And if we evade the difficulties which may occur in the application of this phraseology to new cases, by declaring that our terms are of little importance, is not this to deprive our language of all meaning and all worth? Do we not thus refuse to recognise as valuable the tokens which we ourselves circulate, and plainly declare ourselves bankrupts in knowledge? When certain strata of Devon have thus been identified with the coal measures of other regions, can we still term them grauwacke? Either this term implies members having a definite place in our series of strata, or it does not. If it do, it is certain that these strata have not that place. If it do not, it conveys no geological knowledge at all. But if it be used to imply a rejection of such series, it involves a denial of all geological knowledge hitherto asserted concerning the older rocks of this county.

The transition downwards from the culmiferous beds of Devon to the older strata on which they rest, is, according to almost all who have studied the subject, wrapt in great obscurity. In this obscurity, if it be true that the fossil plants of the culm measures are found also in the subjacent rocks, there is nothing which need make us mistrust the clear and positive part of our knowledge. And even if this be so, it will not be the less necessary to separate the culmiferous from the subjacent Silurian and Cambrian systems, by a different name in our lists, and by a different colour in our geological maps, if they are to represent the present state of our information.

The interest of this question has induced me to dwell upon it longer than I had intended, and I must on that account be very brief in my notice of many other communications. I may observe that the very nature of several of these indicates very remarkably the European character which our geology has assumed, since they have for their object the identification of some members of the recognised series of England, and of France, or Germany. Thus Mr. Murchison and Mr. Strickland have attempted to show, by the evidence of organic fossils, now for the first time adduced on this point, that the red saliferous marls of Gloucester, Worcester, and Warwick shires, with an included bed of sandstone, represent the keuper or *marnes irisées* of Germany; and that the underlying sandstone of Ombersly, Bromsgrove, and Warwick is part of the bunter sandstein or grès bigarré of foreign geologists. They are thus led to conclude that though the muschelkalk, which intervenes between these formations in Germany, is absent in the new red system of England, and of a large part of France, its other members may be identified over the whole of the north of Europe*.

Proceeding from the new red to the oolite system, we have a memoir from Mr. Pratt containing an examination of the geological character of the coast of Normandy, which necessarily implies a comparison of this series of rocks with those of England. The identification is found to be complete, as had already been believed; but Mr. Pratt has made some alteration in the received doctrines on this subject; for instance, the Caen stone, which is usually considered to represent the great oolite, he finds to resemble in its fossils the inferior oolite.

Ascending still, we have to notice Mr. Clarke's elaborate geological survey of Suffolk, which, of course, refers entirely to the chalk and overlying beds. With regard to the crag of this district, I may remark that M. Desnoyers, in a communication made to the Geological Society of France, has endeavoured to identify this formation with the *Faluns* of the Touraine. M. Deshayes had referred the latter to the *Miocene*, and the crag to the *Pliocene* formations of Mr. Lyell. The point is one of great interest, since it involves the question of the value and right mode of application of the test of the relative number of recent species, on which Mr. Lyell's classification, or at least his nomenclature, is founded. I conceive that in a matter of arrangement any arbitrary numerical character must lead to violations of nature's classifications; and can only be considered as an artificial method, to be used provisionally till some more genuine principle of order is discovered†.

Mr. Clarke, in his survey, has noted as one division of the diluvium of his district, a clay of a yellowish or bluish hue, containing rolled pieces of chalk. This deposit is of great extent and thickness in East Anglia and the neighbouring parts‡; and is worth notice, since this deposit is one main cause of the geological confusion and obscurity in which that region is involved. In the neighbourhood of Cambridge this diluvial deposit is called the *brown clay*; and I can state, from my own experience, that the recognition of it as a separate bed at once rendered the stratification clear, where it had long been unintelligible.

[* See Lond. and Edinb. Phil. Mag., vol. ix. p. 318.]

[† An abstract of Mr. Clarke's paper was given in Lond. and Edinb. Phil. Mag. vol. xi. p. 110: see also the papers referred to in note * p. 114 of the same volume, and Mr. Charlesworth's remarks on the subject in his Magazine of Natural History, vol. ii. p. 117.]

[‡ See Mr. Rose's paper in Lond. and Edinb. Phil. Mag., vol. viii. p. 28.

Before quitting our stratified rocks, I may notice the communications respecting some of their fossils which we have received, particularly that of Mr. Williamson on the fossil fishes of the Lancashire coal field, and the establishment of the new genus Tropæum, separated from the Hamites of the green sand by Mr. Sowerby.

In attempting to pursue a stratigraphical order, we are compelled to reserve for a separate head the notice of unstratified rocks, since their age and history are only known by the mode in which they interrupt and disturb the rest of the series. We have not had many communications respecting European rocks of this character; but we cannot but be struck by the subversion of ancient ideas which result from the investigations of Messrs. Murchison and Sedgwick. They have shown that the granite of Dartmoor, and consequently that of Cornwall, formerly considered as one of the earliest monuments of the primeval ages of the earth's history, is posterior to the deposit of the culm measures.

Advancing to newer phænomena, we find the evidences of change still unexhausted. We cannot but reflect how familiar those views of the elevation and depression of portions of the earth's surface are become, which were at first considered so strange and startling. This is remarkably shown by the number of communications concerning raised beaches which we have recently received. When we visit places where these occur, and look at the winding shore, where the sea line is faithfully followed or distantly imitated by terraces, sands and pebbles a little above it, we wonder that we should so long have been blind to this kind of evidence. Such raised beaches have been described during the past year, by Mr. Prestwich, as occurring in the Murray Frith; by Mr. Austen, in the valley of the Axe, the Exe, and the Otter. Dr. Forchammer has given us the evidence of recent elevation in the island of Bornholm; Mr. Trevelyan has given us similar evidence for the coast of Jutland, and the islands of Guernsey and Jersey *.

Mr. Morris's paper, describing a series of dislocations in the chalk cliffs to the south of Ramsgate, marked by shifts in a bed of tabular flint, may perhaps be considered as also affording evidence of violent elevation. But since a small derangement of the conditions of support of any stratum might occasion dislocations of the scale of those here described, it would probably be hazardous to consider them as otherwise than local accidents.

Among descriptions of the most recent geological phænomena, I must notice Mr. Clarke's paper on certain peat marshes and submarine forests, which occur near Poole in Dorsetshire; and in his investigation of the causes which have produced the results now visible, we may see by how easy a gradation descriptive geology passes into the other portion of the subject, the study of the processes by which change is produced.

Finally, in concluding this survey of our descriptive home geology, I notice, with great pleasure, Mr. Burr's communication of his notes on the geology of the line of the proposed Birmingham and Glou-

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[* See our present volume. p. 284.]

cester Railway. In a country like this, in which the order and boundaries of the strata are, for the most part, well ascertained, an additional accuracy of measurement, of great value to us, may be supplied by the operations of civil engineers employed on canals, roads, and the like works. With this persuasion, and acting with the advice of the Council, I wrote letters to a great number of engineers, begging them to communicate to us the levels and sections which they might obtain in the course of their professional employments; and I am happy to see so excellent an example as Mr. Burr's paper supplies, of the advantage which may be derived from materials of this class.

2. *Foreign (South European and Trans-European) Geology.*—In proceeding beyond the Alps, and still more as we advance beyond the shores of Europe, we can no longer, so far at least as geologists have hitherto discovered, trace that remarkable correspondence of the strata of different countries which we can study so successfully in our *home circuit.* With the mountain masses of those more distant regions we are, it would seem, hardly authorised as yet in making any more detailed distinctions than the general one of secondary and tertiary strata; the latter including the strata in which we trace an approach to the existing species of animals, and the former implying a general comparison with our chalk, oolites, and lower strata. Perhaps we may further distinguish in most countries which have been visited, a great mass of transition slates; but the establishment of such divisions must be the business of geological observers.

We have had several valuable additions to this portion of our knowledge, including, as we must do, Greece and its islands in this foreign district. That the Apennine limestone is the predominant mass of the Morea, had been made known by the researches of MM. Boblaye and Virlet. Mr. Strickland and Mr. Hamilton have told us that the same rock forms a large mass of the island of Zante and other islands in that sea, and of the neighbourhood of Smyrna. They find also tertiary beds, as on the south side of the bay of Smyrna; on the east side of the island of Zante; and at Lixouri in Cephalonia, where the tertiary beds are remarkable for the number and beauty of their fossils, some of which have been identified with species existing in the Mediterranean*. Dr. Bell, who travelled from Teheran to the shores of the Caspian, has given us an account of the rocks which he observed in Mazanderan. From the statements made by him, we are led to believe, that a more continued and detailed observation of the country would give the true geological order of the deposits in this region; which might then, perhaps, serve as a connecting link between western Asia and India†.

It is among the favourable omens for the geology of India, of which we now see many, that a temperate spirit of generalization has recently been applied to the examination of her soil; a spirit

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[* See present vol. p. 209.]

[† Notices of Dr. Bell's paper, and of others referred to by the President, will appear in future numbers.]

which contents itself with such a general reference of the foreign to the home strata as we have described, till by its own labours it has earned the right of asserting some closer correspondence. If to deny the value of our geological terms within the home district, where they mark an order which has been repeatedly verified, would be a suicidal scepticism in geologists, there would be a rashness and levity no less fatal in applying them to distant regions where no order has yet been ascertained.

Captain Grant in his account of Cutch, and Mr. Malcolmson in his description of a large portion of the Indian peninsula, have not ventured to call the strata which they have examined by the names which describe European formations. We may trust that, hereafter, the admirable activity and resource which our countrymen display in that wonderful appendage of our empire, will enable them to communicate to us a genuine Indian arrangement of secondary strata. In the mean time, Mr. Malcolmson has most laudably employed himself in determining the age of the wide-spread igneous rocks of the peninsula of India, with reference to the contiguous strata*. And Dr. McCleland, who was associated with Mr. Griffith in the scientific deputation sent under Dr. Wallich into Upper Asam, has, among other geological observations, noted a raised bed, at 1500 feet above the sea level, in which none of the species are identical with those of the Bay of Bengal on the one hand, or the secondary strata on the north of the Himalaya on the other; but in which a resemblance was at once recognised with the species of the Paris basin.

This resemblance between the extinct animal population of regions so remote from each other, is in itself remarkable enough. It is still more curious to observe, that the same coincidence of the ancient animals of France and India has recently been detected in another case; and what makes the circumstance still more remarkable is, that the animal was not only new in both countries as a fossil genus, but involved a transgression of the supposed boundaries of fossil forms. Not only had no human bones been found in genuine strata, but as it had been generally held, no traces of those creatures which most nearly imitate the human form. This rule now no longer holds good; for during the past year the bones of monkeys have been discovered both at Sansan, in France, in the Sewalik Hills in the north of Hindostan†, and more recently under the city of Calcutta.

That this is a highly interesting and important discovery, no one who attends to the signification of geological speculations can doubt. I do not know if there are any persons who lament, or any who exult, that this discovery tends to obliterate the boundary between the present condition of the earth, tenanted by man, and the former stages through which it has passed. For my own part I can see no such tendency. I have no belief that geology will ever be able to point to the commencement of the present order of things, as a problem which she can solve, if she is allowed to make the attempt. The gradation in form between man and other animals, a gradation which we all recognise, and which, therefore, need not startle us because

[* See present vol. p. 286.]

[† See vol. xi. p. 33, and 208.]

it is presented under a new aspect, is but a slight, and, as appears to me, unimportant feature, in looking at the great subject of man's origin. Even if we had no Divine record to guide us, it would be most unphilosophical to attempt to trace back the history of man without taking into account the most remarkable facts in his nature: the facts of civilization, art, government, writing, speech—his traditions—his internal wants—his intellectual, moral, and religious constitution. If we will look backwards, we must look at all these things as evidences of the origin and end of man's being. When we do thus comprehend in our view the whole of the case, it is impossible for us, as I have elsewhere said, to arrive at an origin homogeneous with the present state of things; and on such a subject the geologist may be well content to close his own volume, and open one which has man's moral and religious nature for its subject.

In order to complete the notice of the contributions to foreign geology, I must mention Mr. Roy's account of Upper Canada: in which country he conceives that he has detected terraces which exhibit the beaches of the lakes when the level of their surface was more elevated than they are at present*. I must refer also to Mr. Bollaert's paper on alluvial accumulations containing large masses of silver ore in Peru. And, finally, I have to direct your attention to the very curious information respecting the geology of South America, which we have received from Mr. Darwin. In a communication made to us, he gave a very striking view of the structure of a large portion of that continent; and, as I have already had occasion to observe, he has brought to this country the remains of various fossil animals of entirely new kinds, of exceeding interest to the zoologist as well as the geologist. I need only remind you of the gigantic mammifer which has been reconstructed in idea by Mr. Owen, upon the evidence of a fossil skull, and has been named by him the *Toxodon Platensis*†. This animal, although a *Rodent*, according to its dental characters, in other respects manifests an affinity to the *Pachyderms*; and also to the *Dinotherium*, and to the *cetaceous* order. Many other fossil animals have been discovered in South America; and all, from their magnitude, fitted to excite our wonder, when we compare the diminutive size of the present races of animals which inhabit that country. The animal remains found by Mr. Darwin comprise, besides the Toxodon, which extraordinary animal was as large as a hippopotamus,—(2, 3, 4, 5, 6.) the Megatherium, and four or five other large Edentata;—(7.) an immense Mastodon;—(8.) the Horse;—(9.) an animal larger than a horse, and of very singular character, of which a fragment of the head has been found;—(10, 11, 12.) parts of Rodents, one of considerable size;—(13.) a Llama, or *Guanaco*, fully as large as the Camel.

But I should very ill convey my impression of the great value of the researches of Mr. Darwin, by any enumeration of special points of geology or palæontology on which they have thrown light. Look-

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[* See vol. xi. p. 201.] [† See vol. xi. p. 205.]

ing at the general mass of his results, the account of which he has been kind enough to place in my hands, I cannot help considering his voyage round the world as one of the most important events for geology which has occurred for many years. We may think ourselves fortunate that Capt. Fitz Roy, who conducted the expedition, was led, by his enlightened zeal for science, to take out a naturalist with him. And we have further reason to rejoice that this lot fell to a gentleman like Mr. Darwin, who possessed the genuine spirit and zeal, as well as knowledge of a naturalist; who had pursued the studies which fitted him for this employment, under the friendly guidance of Dr. Grant at Edinburgh, and Professor Henslow and Professor Sedgwick at Cambridge; and whose powers of reason and application had been braced and disciplined by the other studies of the University of which the latter two gentlemen are such distinguished ornaments. But some of the principal of these results may be most conveniently mentioned, when we pass from mere descriptive geology, to that other division of the subject which I have termed Geological Dynamics. And this I now proceed to do.

GEOLOGICAL DYNAMICS.

This term is intended to express generally the science, so far as we can frame a science, of the causes of change by which geological phænomena have been produced. Without here speaking of any classification of such changes, I may observe that the gradual elevation and depression, through long ages, of large portions of the earth's crust, is a proximate cause by which such phænomena have been explained: and this class of events, its evidence, extent, and consequence, is brought before our view by Mr. Darwin's investigations, with a clearness and force which has, I think I may say, filled all of us with admiration. I may refer especially to his views respecting the history of coral isles. Those vast tracts of the Pacific which contain, along with small portions of scattered land, innumerable long reefs and small circles of coral, had hitherto been full of problems, of which no satisfactory solution could be found. For how could we explain the strange forms of these reefs; their long and winding lines; their parallelism to the shores? and by what means did the animals, which can only work near the surface, build up a fabric which has its foundations in the deepest abysses of ocean? To these questions Mr. Darwin replies, that all these circumstances, the linear or annular form, their reference to the boundary of the land, the clusters of little islands occupying so small a portion of the sea, and, above all, the existence of the solid coral at the bottom of deep seas, point out to us that the bottom of the sea has descended slowly and gradually, carrying with it both land and corals; while the animals of the latter are constantly employed in building to the surface, and thus mark the shores of submerged lands, of which the summits may or may not remain extant above the waters. I need not here further state Mr. Darwin's views, or explain how corals, which when the level is permanent fringe the shore to the depth of twenty fathoms, as the land gradually sinks, become successively encircling reefs at a distance from the shore; or barrier reefs at a still greater distance and depth; or when the circuit is small, lagoon islands:—how, again, the same corals, when the land rises, are carried into elevated situations, where they remain as evidences of the elevation. We have had placed before us the map, in which Mr. Darwin has, upon evidence of this kind, divided the surface of the Southern Pacific and Indian oceans into vast bands of alternate elevation and depression; and we have seen the remarkable confirmation of his views in the observation that active volcanos occur only in the areas of elevation. Guided by the principles which he learned from my distinguished predecessor in this chair, Mr. Darwin has presented this subject under an aspect which cannot but have the most powerful influence on the speculations concerning the history of our globe, to which you, gentlemen, may hereafter be led. I might say the same of the large and philosophical views which you will find illustrated in his work, on the laws of change of climate, of diffusion, duration and extinction of species, and other great problems of our science which this voyage has suggested. I know that I only express your feeling when I say, that we look with impatience to the period when this portion of the results of Captain Fitz Roy's voyage shall be published, as the scientific world in general looks eagerly for the whole record of that important expedition.

And I cannot omit this occasion of mentioning with great gratification, the liberal assistance which the Government of this country have lent to the publication of the discoveries in natural history which Mr. Darwin's voyage has produced. The new animals which he has to make known to the world will thus come before the public described by the most eminent naturalists, and represented in a manner worthy of the subject and of the nation. I am sure that I may express the gratitude of the scientific world, as well as my own, for this enlightened and judicious measure.

I may here notice Mr. Darwin's opinion, so ably exposed in a paper read before us, that the change by which a variety of materials thrown on the earth's surface become vegetable mould, is produced by the digestive process of the common earth worm*.

I will here also advert to Mr. Fox's paper on the process by which mineral veins have been filled up. This he conceives might be produced by the circulation or ascension of currents of heated water from the deeper parts of the original fissures. The discovery of the causes of the formation and filling of metallic veins, one of the earliest subjects of geological speculation, will remain probably as a problem for its later stages, when our insight into the laws of slow chemical changes is far clearer than it is at the present day.

If, from these proximate causes of change of which I have spoken, we proceed to those ulterior causes by which such events as these are produced;—to the subterraneous machinery by which islands and continents appear and vanish in the great drama of the world's physical history;—we have before us questions still more obscure, but questions which we must ask and answer in order to entitle ourselves to look with any hope towards geological theory. Of late

[* See present vol. p. 89.]

years an opinion has taken root among us, that the dynamics of geology must invoke the aid of mathematical reasoning and calculation, as the dynamics of astronomy did, at the turning point of its splendid career. Nor can we hesitate to accept this opinion, and to look forwards to the mathematical cultivation of physical geology, as one of the destined stages of our progress towards truth. But we must remember, that in order to pursue this path with advantage, we have, in every instance, two steps to make, each of which demands great sagacity, and may require much time and labour. These two steps are, to *propose* the proper problem, and to *solve* it. Last year an important example of this kind was brought under your notice by my predecessor. The supposition that there are, beneath the crust of the terrestrial globe, liquid or semiliquid masses which exert a pressure upwards, leads to the inquiry what phænomena of fissure, disruption, and dislocation, this subterraneous strain would produce. The answer to this inquiry must be given by mathematical reasoning from mechanical principles; and Mr. Hopkins, who proposed, and to a considerable extent solved this problem, has put forth a set of results, with which, so far as they are definite and decisive, it will be highly important to compare the existing phænomena of disturbed geological districts*. The same assumption, of an incandescent mass existing deep below the earth's surface, has led two other distinguished members of our body to another train of speculations; which, however, though highly interesting, I should be disposed to consider as only the enunciation of a problem, requiring no small amount of mathematical skill for its solution. I speak of the speculations of Professor Babbage and Sir John Herschel, concerning the subterraneous oscillations of the isothermal surfaces of great temperature†. They remark that such oscillations will arise, when thick and extensive deposits take place on any parts of the surface of the earth, (as for instance at the bottoms of seas,) because such deposits increase the thickness of the coating over a given subterraneous point; and thus removing the cooling effect of the surface, bring a high temperature to a place where it did not exist before. The deposited strata might thus be invaded by violent heat advancing from below; and there might result both changes of position arising from extension and contraction, and a metamorphic structure in the rocks themselves. It is highly instructing to have this chain of conceivable effects pointed out to us; but we may venture to observe, that in order to render the suggestion of permanent use, it will be necessary to express, in some probable numbers, the laws of the result as affected by the conductivity of the earth's mass, the rate and thickness of the deposit, and other circumstances. For instance, we know that a deposit of one thousand feet thick would be quite insufficient to occasion a metamorphic operation in its lower strata. Would then a deposit of ten thousand or of twenty thousand feet call into play such a

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[* Mr. Hopkins's view of his researches will be found in vol. viii. p. 227. *et seq.*—Edit.]

[† See vol. xi. p. 212. 214; also the report of the Society's proceedings in our next Number.]

process *? To answer questions like these, of which a vast number must at once occur to our minds, we have many experimental data to collect, many intricate calculations to follow out. And it would be easy to point out problems of a still more abstruse kind, in which we no less require aid from the mathematician, before we can proceed in our generalizations. May we not hope to see some fortunate man of genius unveil to us the mechanics of crystalline forces? And when that is done, can we doubt that we shall have a ray of new light thrown upon those extraordinary phænomena of slaty cleavage in mountain masses which have lately been brought under our notice? Or, recollecting the experiments of Sir James Hall, (a striking step in geological dynamics,) may we not hope then to learn how those crystalline forces are stimulated by heat; and thus follow the metamorphic process into its innermost recesses? These and a thousand such questions lie before us;—tangled and arduous inquiries no doubt, but connected by their common bearing upon one great subject;—'a mighty maze, but not without a plan.' And through this maze we must force our way in order to advance towards any sound geological theory. The task is one of labour and difficulty; but I well know, gentlemen, that you will not shrink from it on that account. Those who aspire to the felicity of knowing the causes of things, must not only trample under foot the fears of a timid unphilosophical spirit, which the poet deems so necessary a preparation, but they must look with a steady eye upon difficulty as well as violence. They must regard the terrors of the volcano and the earthquake, the secret paths by which hot and cold and moist and dry ran into their places, the wildest rush of the fluid mass, the latent powers which give solidity to the rock,—as operations of which they have to trace the laws and measure the quantities with mathematical exactness. And though there can be no doubt that the greater part of us shall be more usefully employed in endeavouring to add to the stores of descriptive geology, than in these abstruse and difficult investigations, yet we must always receive, with great pleasure, any communications containing real advances in the mathematical dynamics of geology, from those whose studies and whose powers enable them to lay an effectual grasp upon these complex and refractory problems.

I have but a single word to add in conclusion. This Society has always been an object of my admiration and respect, not only from the importance and range of its scientific objects, the wide and exact knowledge which it accumulates, the philosophical spirit which it calls into play, the boundless prospect of advance which it offers; but also for the manner in which its meetings and the intercourse of its members have ever been conducted; the manly vigour of discussion, tempered always by mutual respect and by good manners; the deep interest of all in the prosperity of the Society, to which, whenever the hour of need comes, private differences of opinion and resentments have given way. To be placed for a time at the head of a body which I look upon with such sentiments, I must ever consider as one of the greatest distinctions which can reward any one who gives his attention to science. I trust, by your assistance and kind sym-

[* See p. 533. of the present Number.]

pathy, gentlemen, I shall be able to preserve the spirit and temper which I so much admire;—to hand that torch to my successor burning as brightly as it has hitherto done. And there is one consideration which will make me look with an especial satisfaction upon such a result. I have not myself the great honour of being one of the members of the Society who are connected with it by an early interest in its fortunes, and by long participation in its labours. I may consider myself as only belonging to its second generation. Now if there be a critical and a perilous time in the progress of a voluntary association like ours, it is when its administration passes out of the hands of its founders into those of their successors. It is like that important and trying epoch when the youth quits the paternal roof. I will say however, gentlemen, for myself and for my fellow-officers, some of whom are in the same condition, that our best cares shall not be wanting that the Society may suffer as little as possible by this change. And among our grounds for hope and trust, the main one is this: that though the offices of the Society may be in younger hands, the parental cares of its founders are not withdrawn. We have to discharge our office with the aid and counsel of those excellent persons to whom the prosperity of the Society up to the present time has been owing. Surrounded by such men, knowing their generous and ready sympathy for the attempts and exertions of their followers and disciples, I feel a cheerful confidence in the future destinies of the Geological Society; and a persuasion that it will not only preserve but extend its influence as a bond of scientific and social union among its members.

ROYAL ASTRONOMICAL SOCIETY.

March 9, 1838*.—The following communications were read:—

I. Immersion of λ *Cancri* at Moon's dark limb, March 6, 1838. By R. Snow, Esq. Corrected sidereal time, $13^h\ 14^m\ 54^s$.

II. Extract of a letter from Sir John Herschel to the President, giving an account of a remarkable increase of magnitude of the star η in the constellation *Argo*, observed by him at the Cape, December 16–17, 1837.

"I have just observed a very remarkable phænomenon, the development of which I am watching with much interest. It respects the nebulous star η in the constellation *Argo*, No. 1281 of the Catalogue of the Astronomical Society, marked in that catalogue as of the second magnitude. As such, or rather as intermediate between the first and second, as a very large star of the second magnitude, or a very small one of the first, I have always hitherto observed it, having, in some cases, equalized it with *Fomalhaut*; in others placed it intermediate between α and β *Crucis*, nearly equal with the latter, &c.; nor have I at any time had reason to suppose its magnitude variable. Tonight, however, being at work on my classification of the southern stars in order of their magnitudes, I was much astonished to find its magnitude superior, not only to that of *Fomalhaut* and α *Crucis* (with which stars it no longer admits of a moment's comparison), but even to that of *Aldebaran*, *Procyon*, α *Eridani*, α *Orionis*, and little if at all inferior to that of *Rigel*.

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* We shall notice the communications made at previous meetings at another opportunity.

"This was my own judgement, and that of several persons whom I called to my assistance, in the early part of the night, when η was low and *Rigel* high in the heavens. At the time I write, they have about equal altitudes, and the comparison is decidedly in favour of η, which is, in fact (*Sirius* and *Canopus* excepted), the most brilliant star now visible; α *Centauri* being too low for fair comparison, and veiled with some degree of haze.

"This remarkable increase of magnitude has come on very suddenly, as my attention has frequently of late been drawn to this star in the lower part of its diurnal circle, while watching with some impatience its progress towards the meridian, at a reasonable hour of the night, that I might resume and complete, before my departure hence, a very elaborate monograph of the wonderful nebula which surrounds it. A few evenings before the full moon just passed, in particular, I remember to have noticed it with this view; and had it then been what it now is, a star of the first class, it could not have passed unremarked.

"Whether it be now at its maximum, and about to decrease by insensible degrees; whether, like *Algol*, but in a much longer time, it remains as it were dormant through the greater part of its period, and runs through its phases of increase and decrease in a small aliquot portion of the whole; or whether, lastly, it be on the point of blazing forth with extraordinary splendour, so as possibly to outshine its brilliant neighbours, α *Centauri* and *Canopus*, it is useless to conjecture, and observation will soon determine."

III. Value of the Mass of *Uranus*, deduced from Observations of its Satellites, made at the Royal Observatory of Munich during the year 1837. By Dr. F. Lamont, Director of the Royal Observatory.

In the course of the year 1837, a few favourable nights were employed in taking observations of the satellites of *Uranus*, with a view of calculating the value of the planet's mass; and, though the object has not been satisfactorily attained, owing to the difficulty of the observations, and the present unfavourable position of the orbits of the satellites, the result is not without interest, as it leads to the conclusion that the true value of the mass of *Uranus* is considerably smaller than that which is generally adopted.

The instrument used in the observations was a refractor constructed at Munich, having a focal length of 15 feet, and an aperture of 10½ inches, Paris measure. The fact of its having served to measure the distances of the satellites of *Uranus* is sufficient evidence of its superior power. Dr. Lamont acknowledges, however, that, notwithstanding the great optical power of the telescope, he has not, as yet, been able to discover more than three of the satellites, namely, the second, the fourth, and the sixth*. The sixth was observed only once, and the observation is of consequence omitted, as being of no use in the present inquiry.

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* The satellites are named in the order of their distances from the planet, so that those which are here termed the *second* and *fourth*, correspond re-

The measures were obtained by using a parallel wire-micrometer of Fraunhofer's construction, having the wires, not the field, illuminated. Instead of the lamps usually employed, a light placed at a distance was reflected on the wires by a small mirror. Dr. Lamont remarks, that the use of a mirror is greatly to be preferred to lamps, because, in addition to its being more convenient, it affords in the measurement of faint objects a peculiar advantage, in enabling the observer to direct the illumination to any part of the wires, and with any degree of intensity that may be required.

But, however carefully the illumination may be managed, it would be impossible to bisect with a wire any of the satellites of *Uranus*, and, accordingly, Dr. Lamont had recourse to another method, which he has frequently adopted in similar cases. Placing the fixed wire so as to bisect the disc of the planet, he moved the micrometer until the satellite appeared exactly in the middle of the space between both wires. The measure being repeated on the opposite side of the fixed wire, in order to eliminate the zero point, the difference of the two readings gave the quadruple distance of the satellite.

The table of observations given by the author contains the sidereal time of each observation; the mean Paris time, including aberration; the observed angles of position; the observed distances, and the number of measures taken at each observation. The number of observations of the second satellite was 11, and of the fourth, 15.

Although the observations furnish sufficient proof of the elliptic motion of the satellites, any attempt to investigate the elliptic elements from the few data obtained in the present unfavourable situation of the orbits, would be unavailing. Dr. Lamont, therefore, assumes the satellites to move in circular orbits, in a plane having, as computed by Sir W. Herschel, an inclination of 101° 2′ to the ecliptic, the longitude of the ascending node being 165° 30′; and on this hypothesis proceeds to compute from the observations the distances and times of revolution of the two satellites. The results of the computation are as follows:

	Distance.	Periodic Time.
Second satellite	31″·35	8ᵈ,705886
Fourth ditto	40 ·07	13 ,463263

Having found the distances and periods of revolution, it remains to compute the value of the planet's mass. It is found, however, that the values derived from both satellites exhibit a considerable difference, as might indeed be expected, when it is considered that

spectively to the *first* and *second* of Sir W. Herschel. [An abstract of Sir John F. W. Herschel's investigation of the motions of the same two satellites of *Uranus*, was given in Lond. and Edinb. Phil. Mag. vol. iv. p. 381. Sir John observes, that he had, at the period of his communication, no evidence of the existence of any other satellites of this planet.—Edit.]

the mean distances are the result of a small number of observations calculated upon the gratuitous supposition of circular orbits. On diminishing the radius of the second satellite by 0″·79, and augmenting that of the fourth by the same quantity, in order to make the distances accord with the periods of revolution, the value of the mass of *Uranus* is found $= \frac{1}{24605}$, being less by one-fourth part than that obtained by M. Bouvard, from the perturbations produced by the planet.

Dr. Lamont remarks that, in giving this value he is too well aware of the uncertainty of the data on which it rests to attach any particular weight to it; but, though he considers the measures obtained by him in 1837 as being unfit, until combined with further observations, to give a value of the mass of *Uranus* that might, with confidence, be employed in the theory of the planetary motion, there is one purpose they will serve at present, namely, to enable us to judge whether a given value of the mass of *Uranus* can be regarded as probably true or not by its agreement or disagreement with them. Bouvard's value, which is generally adopted, is $\frac{1}{17918}$. Computing from this the mean distances of the two satellites, the distance of the planet from the earth being assumed = 19·223, he finds the mean angular distance of the second satellite = 33″·96, and that of the fourth = 45″·42. The difference of these values from those computed from the observations, is + 2″·61 and + 5″·35. Now, without entering into the theory of probable errors, it will readily be granted, he conceives, on considering the differences of the individual errors from the mean, and even allowing a probable error for eccentricity, that scarcely an error of 2″, much less one exceeding 5″, can be attributed to the distances obtained from observation. The supposition of a constant error in the instrument can scarcely be admitted; for, on measuring distances that had otherwise been determined with precision, no constant error has been found to exist.

In conclusion, the author states that he considers it certain that the value of the mass of *Uranus*, at present used in the theory of the planetary perturbations, ought to be greatly diminished, though the precise proportion in which this should be done, cannot at present be assigned. Considering the difficulty of the observations, and the small number of nights in which measures of so much delicacy can be made, it will not be possible, within the period of several years, to deduce the true value of the planet's mass from the elongations of the satellites.

A table of these stars, giving the dates of observation, the designation of the objects, and the apparent right ascension for each Observatory, will be found in the Monthly Notices of the Society for March.

IV. Stars observed with the Moon at the Royal Observatories of Greenwich and Edinburgh, and the Observatory of Cambridge, in the months of October, November, and December, 1837.

The anonymous star observed (at Greenwich) on October 12, A. S. C. 79. (*Piscium*, apparent right ascension at Greenwich $0^h\ 39^m\ 53^s,52$; at Cambridge $0^h\ 39^m\ 53^s,73$; at Edinburgh $0^h\ 39^m\ 53^s,30$.), has an erroneous right ascension in the Nautical Almanac, derived from an erroneous right ascension in the Astronomical Society's Catalogue. See the Cambridge Observations, 1831.

April 13.—The following communications were read:—

I. On the Correction of the Mean Distance, Eccentricity, Epoch, and Longitude, of the Aphelion of the Orbit of Venus, by Errors of Heliocentric Longitude, derived from the Cambridge Observations of the Years 1833, 1834, and 1835, and the Greenwich Observations of 1836. By the Rev. R. Main.

The author states, that being furnished by the Astronomer Royal with his computed errors of heliocentric longitude of the planet *Venus*, derived from four years' unintermitted observations, he undertook to correct the above-mentioned elements by means of them. An abstract of the paper appears in the *Monthly Notices* for April, giving the ultimate results of the investigation as follows:

The combined equations are given separately for each year, to the end that any suspected error may be more readily detected, and the solutions given for each year separately. They are as follow:

	1833.	1834.	1835.	1836.
$\delta a =$	+0·0000203	−0·0000047	+0·0000368	+0·0000236
$\delta e =$	−0·0000262	−0·0000223	−0·0000280	+0·0000186
$\delta \epsilon =$	− 2″·1	− 4″·9	− 3″·6	− 4″·1
$\delta \pi =$	−587 ·7	−501 ·3	+328 ·4	− 67 ·3

II. Moon culminating Observations made at Rio Janeiro and Valparaiso in 1836. By Captain F. W. Beechey, R.N.

From these observations, compared with the corresponding observations made at Greenwich, Cambridge, Edinburgh, Paris, and the Cape of Good Hope, and with the *Nautical Almanac*, Captain Beechey has deduced the following results.

Longitude of Fort S. Antonio, Valparaiso..	71° 39′ 36″·7 W.
Longitude of Anhatomirim, Brazil........	43 09 13 ·5 W.

III. Times of Immersion of the first and second Satellites of *Jupiter*, observed at Greenwich Hospital Schools, April 9, 1838. By E. Riddle, Esq. See *Monthly Notices*.

IV. Longitude of the Edinburgh Observatory, computed from the corresponding Moon Culminating Observations made at Edinburgh and Greenwich, from August 24, 1836, till the end of 1837. By Mr. Riddle.

The number of observations is 62, and the mean of the whole, allowing each observation weight proportional to the number of stars observed, gives the longitude = $12^m\ 44^s,7$.

In the *Notices* for March 1837, are given the results deduced from all the corresponding observations of the same class, made at the two Observatories under the direction of the present astronomers, before the date of the first in this list. The mean result of these preceding observations was $12^{m}\ 44^{s},5$, differing only $,2^{s}$ from the present determination.

V. Eclipses of *Jupiter's* Satellites, observed at Edinburgh. By Professor Henderson.

VI. Lunar Occultations observed at Edinburgh. By Professor Henderson. A table of these, giving the date, true sidereal time, object, phænomena, and remarks, appears in the *Monthly Notices*.

VII. Immersion of 47 *Geminorum* at Moon's dark limb, April 1, 1838. By R. Snow, Esq. Corrected sidereal time, $14^{h}\ 25^{m}\ 4^{s},73$.

VIII. The President read an extract of a letter from Mr. Henderson relative to the remarkable increase of magnitude, in *η Argus*, recently noticed by Sir John Herschel, as mentioned at the last meeting of the Society (*ante*, p. 521.). Mr. Henderson states that the star is not to be found at all in Ptolemy's catalogue, although the bright stars of the Cross and the Centaur, which culminated as low at Alexandria, are inserted in it. From this circumstance he infers that, at this remote period, the star was not very bright. It is not in Bayer's maps; and in Halley's catalogue it is said to be of the *fourth* magnitude, which is less than some of the neighbouring stars that in modern times cannot compete with it. It would thus appear that the star has for a long period been increasing in brightness; and it will be remarkable if it should surpass the brightest at present known.

ZOOLOGICAL SOCIETY.

[Continued from p. 451.]

March 28, 1837.—Mr. Chambers read a paper upon the habits and geographical distribution of *Humming Birds*, and exhibited the nest and eggs of the only species (*Trochilus colubris*,) which visits the United States, and which is there very commonly bred in confinement. Mr. Chambers adverted to the probability of success if attempts were made to domesticate these birds in this country. A lady residing at Boston informed him that in that city they are readily reared in cages, and she expressed great surprise on hearing that only one instance had occurred of their being domesticated in England, as the climate so nearly corresponds.

The first part of a paper was then read by F. Debell Bennett, Esq., corresponding member, on "The Natural History of the Spermaceti Whale."

Mr. Yarrell then brought before the notice of the meeting "A Synopsis of the *Fishes* of Madeira," by the Rev. R. T. Lowe, Corresponding Member of the Society. This synopsis includes all the *Fishes* hitherto found at Madeira, with observations upon many of the species, and the character of such genera and species as are new. The Author has also drawn up a table, showing the comparative number and distribution of the British, Mediterranean, and Madeiran *Fishes*. It appears from this, that notwithstanding the uniformity of its shores, both in structure and materials, occasioning a corresponding uniformity in food and shelter, that the number of marine species found at Madeira equals two thirds the amount belonging to the British seas.

With the exception of the genus *Anguilla*, the fresh-water species are entirely absent, the physical structure of the island preventing the formation of lakes and pools, and reducing its streams to the character of rapid rivulets or mountain torrents. A result indicated by the table just referred to, and which Mr. Lowe particularly notices, is, that Madeira possesses as many species in common with Britain as it has with the Mediterranean, and also that there is a variation in the ratio between the marine *Acanthopterygians* and *Malacopterygians* proportionate to the latitude. In Britain the marine *Acanthopterygians* are to the marine *Malacopterygians* as one and a quarter to one; in the Mediterranean, as two and three fifths to one; while at Madeira the ratio increases to three and a half to one.

The Author's remaining observations principally relate to the particular periods of the year, and to the comparative abundance in which certain species are met with.

A Notice by Thomas Wharton Jones, Esq., was then read, "On the mode of closure of the gill-apertures in the tadpoles of Batrachia."

Mr. Jones observes, that when the right gill of the tadpole disappears, it is not, as is usually supposed, by the closure of the fissure through which it protrudes, but by the extension of the opercular fold on the right side towards that of the left, forming but a single fissure, common to the two branchial cavities, through which the left gill still protrudes. He also remarks that conditions analogous to those which occur during several stages of this process exist in the branchial fissures of the anguilliform genera, *Sphagebranchus*, *Monopterus*, and *Synbranchus*.

April 11, 1837.—The reading of Mr. F. De Bell Bennett's paper "On the Natural History of the Spermaceti *Whale*," was resumed; an abstract of which is given in No. lii. of the Proceedings.

Mr. Gould then called the attention of the meeting to a new and beautiful species of *Ortyx*, a native of California, from the collection of the late David Douglas, and characterized it under the name of *O. plumifera*.

He remarked that this genus was first brought before the Society eight or nine years ago by Mr. Vigors, at which time only five species were known, but since that period the number had been doubled; and from the remarkable development of the feathers forming the crest in the species then exhibited Mr. Gould anticipates the discovery of others, which shall connect *Ortyx plumifera* with those species in which this character is less prominently shown. In support of this opinion Mr. Gould directed attention to the genera *Larus*, *Trogon* and *Caprimulgus*, which possess certain characters largely developed; but the degree of development increases gradually from the species in which it is least apparent to those in which it attains its greatest extent.

Mr. Gould then exhibited a new species of the genus *Podargus*, from Java, which he proposes to name *P. stellatus*.

Some observations on the *Physalia*, by George Bennett, Esq., F.L.S., Superintendent of the Australian Museum at Sydney, and

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Corresponding Member of the Zoological Society, were then read.

Some specimens of *Physalia pelagica* having been captured by Mr. Bennett while on his voyage to Sydney, he had an opportunity of observing the action of the numerous filamentary bodies attached to the air-bladder of this animal.

The longest of these appendages are used by the *Physalia* for the capture of its prey, and are capable of being coiled up within half an inch of the air bladder, and then darted out with astonishing rapidity to the distance of 12 or 18 feet, twining round and paralyzing by means of an acid secretion any small fish within that distance. The food thus seized by the *tentacula* is rapidly conveyed to the short appendages or tubes, which are furnished with mouths for its reception. These tubes appear to constitute the stomach of the animal, for upon a careful dissection nothing like a common receptacle for food could be observed, nor could Mr. Bennett detect any communications between them and the air-bladder, to the inferior portion of which they are attached by means of a dense muscular band. After an examination of an immense number of specimens, Mr. Bennett was unable to discover the orifice usually stated to exist at the pointed end of the bladder, nor could he ever succeed in expelling any portion of the contained air without a puncture being previously made. This organ consists of two coats, the outer of which is dense and muscular, readily separating from the inner, which resembles a cellular membrane.

The partial escape of air from the bladder did not at all affect the buoyancy, or appear in any way to incommode the *Physalia*; and even when it had completely collapsed, the animal still floated on the surface; upon removing the bladder entirely, the mass of tentacula sank to the bottom of the vessel, and though their vitality remained, all power of action was entirely destroyed.

A letter was then read, addressed to Mr. Gould, from M. Natterer, describing a new species of *Pteroglossus*, from Para in Brazil, which the writer proposes to name *P. Gouldii*, in commemoration of the valuable contributions which ornithology has derived from the labours of Mr. Gould.

April 25, 1837.—A letter was read addressed to N. A. Vigors, Esq., M. P., from Mr. Henry Denny of Leeds, stating that a fine male specimen of the Snowy Owl had been recently captured at Selby in Yorkshire.

Mr. Gray then exhibited the horn of a Deer supposed to come from India, which he considered as characteristic of a new species peculiar for the elongate acute form of the basal branch, which appears to have been depressed, and directed obliquely across the forehead of the animal. This horn, which had not attained its full period of growth, agreed with that of the Rein Deer, in being palmate, and in having the basal frontlet depressed, in which latter character it is allied to an Indian species called by Mr. Gray *Cervus Smithii*, known by a drawing belonging to the collection of General Hardwick in the British Museum.

Mr. Gray then adverted to some observations which he had made on a former occasion during a discussion upon the nature of the relation existing between the Argonaut shell and the Cephalopod which inhabits it. On that occasion, one argument made use of by him in favour of the parasitic nature of this animal, was, that the nucleus of the Argonaut shell is larger than could be contained within the eggs which often accompany the Ocythöe. He is now disposed to attach less importance to this circumstance, having recently observed that the eggs of some mollusca, as the *Buccinum undatum*, prior to the period of hatching, are eight or ten times as large in diameter as when first deposited.

A paper was then read by Thomas Bell, Esq., entitled "Observations on the genus *Galictis*, with a description of a new species." Mr. Bell in 1826 laid before the Zoological Club of the Linnæan Society some remarks upon a living female Grison which had been several years in his possession, and he then proposed to consider the species as constituting a new generic type, to which he gave the name of *Galictis*, but without assigning its distinctive generic characters. Since that period the examination of a specimen in the collection of the Zoological Society, exhibiting a distinct specific difference from the former, but agreeing with it in the more essential particulars, has confirmed the propriety of establishing this genus; and in the present communication the author points out the characters and affinities of *Galictis*, and gives a description of the new species under the name of *G. Allamandi*, M. Allamand having figured a specimen in the fourth edition of Buffon's Natural History, which may perhaps be identical with this second species. In constituting this new genus of *Mustelidæ*, Mr. Bell has been guided solely by the semiplantigrade form of the foot, for in no other important character does it deviate from the typical genus of that family. A knowledge of this character led Thunberg to place it among the *Ursidæ* under the name of *Ursus Brasiliensis*, to which group it slightly approximates, and in which it may probably be represented by the genus *Ratellus*. By Desmarest it is arranged in the genus *Gulo*, and the name *Gulo vittatus* given to it by that author has been adopted by the Cuviers, and all other subsequent writers, with the exception of Dr. Traill, who in the third volume of the Memoirs of the Wernerian Society restores it to its proper family, the *Mustelidæ*, but under the erroneous name of *Lutra vittata*, for it has no nearer affinity to the Otters than any other genus of that family. By Schreber it was placed among the *Viverræ*, under the name of *Viverra vittata*, and the name has been retained by Gmelin and others.

The characters of *Galictis*, and the description of the two species which at present constitute this genus, are as follows.

Fam. MUSTELIDÆ.
Genus *Galictis*, Bell.

CHAR. GEN. *Dentes molares spurii* $\frac{2-2}{3-3}$.

Rostrum breve.

Palmæ atque *plantæ* nudæ subplantigradæ.

Ungues breviusculi, curvi, acuti.

Corpus elongatum, depressum.

Sp. 1. *Galictis vittata.*

G. vertice, collo, dorso, atque caudâ flavescenti-griseis; rostro gulâ et pectore fuscescenti-nigris; fasciâ a fronte usque ad humeros vescenti-albidâ; pilis longis laxis.

Sp. 2. *Galictis Allamandi.*

G. vertice, collo, dorso, atque caudâ nigricanti-griseis; partibus inferioribus nigris; fasciâ a fronte usque ad collum utrinque albâ; corpore pilis brevibus adpressis.

Habitat.

June 27, 1837.—A Letter was read addressed to Mr. Gould, from Mr. Thomas Allis of York, in which the writer remarks that the sclerotic ring of the great *Podargus* does not present the slightest appearance of distinct plates, being simply a bony ring; the first instance in which Mr. Allis had observed this peculiarity.

A Letter was also read from His Excellency Hamilton Hamilton, Esq., Her Majesty's Minister at Rio, announcing the present of a *Chilian Eagle* for the Society's Gardens.

Mr. Gray exhibited a specimen of a *Paradoxurus* from the Malayan Peninsula, which had been presented to the Museum of the Society by the President, the Earl of Derby, and for which he proposed the specific name of *Derbianus*.

Mr. Gray also brought before the notice of the Meeting some Mammalia, which he had lately purchased for the British Museum from a collection made by the late Colonel Cobb in India, among which was an adult specimen of the *Once* of Buffon (Hist. Nat.), on which Schreber formed his *Felis uncia*, which has been regarded by Cuvier, Temminck, and most succeeding authors as a leopard, but which is a distinct species, easily known by the thickness of its fur, the paleness of its colour, the irregular form of the spots, and especially by the great length and thickness of the tail. Mr. Gray observed that a more detailed description of this animal was unnecessary, as it agreed in all particulars with the young specimen described by Buffon.

Two new species of *Sciuroptera*, which agree with the American species in colour, but differed from one another in the size, make, and form of the soles of the feet, were described under the specific names of *fimbriata* and *Turnbulli*.

A new species of *Fox*, nearly allied to *Vulpes Bengalensis*, but evidently larger, Mr. Gray designated as *Vulpes xanthura*. In describing this species, he remarked, that it had a large gland, covered with rigid brown hair, on the upper part of the base of its tail, very distinctly marked; and that on looking at the tail of the several other species of this genus, as *V. Bengalensis*, *V. vulgaris*, *V. fulva*, and some others, a similar gland was easily recognisable, though it appeared to have been hitherto overlooked.

Mr. Ogilby afterwards characterized a new species of Gibbon (*Hylobates*), which had been presented to the Society many years ago, by the late General Hardwicke, and hitherto considered as the female of the Hoolock. A specimen of the latter species had been presented to the Society at the same time, and from the same locality; but their specific identity was sufficiently disproved, not only by the fact of both specimens being of the same sex, and from our being perfectly acquainted with both sexes of the Hoolock, but likewise by the marked difference of colour and external structure exhibited by the two animals. The greater height of the forehead and prominence of the nose in the new species were pointed out as alone sufficient to distinguish it from all the other Gibbons; whilst its ashy-brown colour and large black whiskers rendered it almost impossible to confound it with the Hoolock, which has fur of a shining black, and a pure white band across the forehead. Mr. Ogilby observed, that we have had two distinct instances of real Apes from the continental parts of India; and referred to various passages of Pliny, in which the Roman naturalist professed to describe different races of human beings from the remote provinces of India, whom he relates to have teeth like dogs, to live among trees, and to converse by frightful screams. These distorted accounts Mr. Ogilby conceives to have been founded upon the vague tales brought back by the few Greek and Roman travellers who at that time penetrated beyond the Ganges, and proposed therefore to call the new Gibbon by the name of *Hylobates Choromandus*, the name of one of the supposed tribes of men described by Pliny. The same gentleman afterwards exhibited and described the skin of a new species of Colobus, or four-fingered monkey from Africa; for which he proposed the specific name of *Colobus leucomeros*, on account of the white colour of the thighs, the rest of the animal being a deep shining black.

Dr. Smith exhibited some small Quadrupeds, forming part of the collection obtained during his recent expedition into South Africa. They consisted of some new or rare species belonging to the genera *Macroscelides*, *Chrysochloris*, *Pteromys*, and *Otomys*. Dr. Smith entered into some interesting details respecting their habits, which will be published in his forthcoming work on African Zoology.

LINNÆAN SOCIETY.

Jan. 16, 1838.—Read a Paper on the Structure of *Cuscuta europæa*. By Charles C. Babington, Esq., M.A., F.L.S.

The descriptions and figures of this plant given in the various works on our native plants are very imperfect. Mr. Babington's observations on recent specimens gathered in Sussex, in company with Mr. Bower, confirm the statement of Mr. Brown as to the existence of scales in the tube of the corolla, a fact denied both by Sir J. E. Smith and Sir W. Hooker, who, however, appear to have examined dried specimens. These scales are transparent, closely pressed to the corolla, and very minute, so that they are easily overlooked, even in recent specimens, and in dried ones it is scarcely possible to discern them. They are bicuspidate, erect, and situated at the inner base of the filaments, which they partially inclose. Their form and position appear to have been first accurately described by Raymond, as recorded by Römer and Schultes. Reichenbach describes and figures them as palmate, and as situated at the base of the tube, so that it is probable his plant is different from ours, as Mr. Babington suggests. The nature of these scales is not well understood: by most botanists they are regarded as a verticil of abortive stamens, and by Reichenbach as petals; but their situation always within the stamens, and opposite to them, appears to refute both these opinions. Analogous scales occur in *Hydrophyllea*.

Feb. 20.—Read a Paper, by John Hogg, Esq., M.A., F.L.S., on the classification of the Amphibia.

The author takes a review of the different modes of arrangement that have been proposed for this remarkable class of animals, and he concludes his paper by suggesting a new classification founded upon the organs of respiration, as the result of his investigation.

March 6.—Read a description of the Mosses collected in the journey of the late deputation into Upper Assam, in the years 1835 and 1836. By William Griffith, Esq., Assistant Surgeon on the Madras Establishment. Communicated by R. H. Solly, Esq., F.R.S. & L.S.

The discovery of the China tea plant in Upper Assam attracted the attention of the Indian government, and accordingly a deputation, consisting of Dr. Wallich, Mr. M'Clelland, and Mr. Griffith, was sent from Calcutta to investigate the subject. The present paper comprises descriptions of the Musci collected in the journey; but the greater portion of the species, Mr. Griffith states, was gathered in the Khasya Hills, an elevated tract of country, forming part of the eastern frontier of British India.

The climate is described to be excessively moist, which will account for the large number of mosses collected in the journey by Mr. Griffith, forming about one eighth of the entire family, 1324 being the amount of species enumerated by Bridel in his Bryologia Universa.

The collection contains *Sphagnum obtusifolium*, *Polytrichum urnigerum* and *aloides*, *Weissia Templetoni*, *Dicranum scoparium* and *glaucum*, *Bartramia fontana*, and several others familiar to the European muscologist; but the far greater part of the species have not been previously described.

March 20.—Read a description of the Mora tree. By Mr. Robert H. Schomburgk. Communicated by George Bentham, Esq., F.L.S.

This tree is a native of the forests of British Guiana, where it attains a large size, the trunk often exceeding ninety feet in height, with a circumference of upwards of twenty feet. The trunk produces large buttresses at its base, which from their partial decay afterwards become hollow beneath, and form a chamber capable of sheltering several persons standing erect. The tops of these buttresses, and the trunk itself, are found clothed with innumerable epiphytes, which greatly add to the singularity of the tree. The tree affords timber of excellent quality, being close-grained, strong, tough and durable, and not liable to split. The Mora tree constitutes a new genus of the order *Leguminosæ*, belonging to the suborder *Cæsalpineæ*, and tribe *Cassieæ*. Mr. Bentham adopts the native name for the genus, and proposes that of *excelsa* for the species.

The genus is nearly related to *Tachigalia* of Aublet, and *Leptolobium* of Vogel, but differs from both in the woody texture of the pod, which is moreover naturally dehiscent, in the greater regularity of the parts of the flower, and in the sterility of the alternate stamina.

April 3.—Read a communication on the existence of Stomata in Mosses. In a letter to R. H. Solly, Esq., F.R.S. & L.S. By William Valentine, Esq., F.L.S.

The discovery of stomata in mosses was reserved for Mr. Valentine, an opinion of their absence from that family having universally obtained amongst botanists. It was in *Bryum crudum* that Mr. Valentine first detected stomata, and of one hundred and three British mosses examined by him, seventy-eight were found to possess these organs. Their situation in this family is very remarkable, being confined, with one exception, to the theca, and the thinness of the tissue will readily account for their absence from other parts.

The more common form of the stomata in mosses is similar to that generally found amongst phænogamous plants. Each consists of two oblong reniform cells, with their concave sides opposed to each other. In *Funaria hygrometrica* they consist of a single cell in the form of a hollow ring, and in five British species of *Orthotrichum* (*diaphanum*, *pulchellum*, *rivulare*, *anomalum*, and *cupulatum*) they have a raised border of projecting cells which form a cavity above the stoma, resembling somewhat those of *Marchantia* and *Targionia*.

ROYAL INSTITUTION.

April 27.—Mr. Grainger on the physiology of the spinal cord.

May 4.—Mr. Hickson on vocal music as a branch of education.

May 11.—Mr. Brayley on the Theory of Volcanos. The subject was introduced by allusion to the important results which have ensued from the recent direction of the minds of mathematicians to the philosophy of geology, and from the application to the theory of geological phænomena, both of actual mathematical analysis and of general mathematical reasoning. Mr. Hopkins's researches on the mechanical theory of elevation, recently applied by Prof. J. Phillips to explain the structure of parts of the North of England, and the theory of volcanic action dependent on that of the secular variation of the isothermal surfaces within the globe, in which Mr. Babbage and Sir John F. W. Herschel have independently concurred, having been cited as examples of those results, the object of the present communication was stated to be, first, the familiar exposition of that theory; and, secondly, to offer reasons why the chemical theory of volcanos originally proposed by Sir Humphry Davy, should not be discarded "as a mere chemical dream," even on the high authority of Sir John Herschel.

The foundation of the former theory being the observed augmentation of temperature, as we descend from the surface of the earth towards its interior, a brief statement of the general result of the observations hitherto made on that subject was first given, illustrated by reference to a table exhibiting the temperatures (on Fahrenheit's scale) corresponding to a variety of depths, from 50 feet to 200 miles below the surface of the earth, on the average (assumed for convenience, but corresponding closely with that of observation,) of 1° of increase of heat for every 50 feet of depth. The theory of the secular variation of the isothermal surfaces of the interior of the globe, considered as a cause of geological phænomena, was then explained; as it has been enunciated by Mr. Babbage in his paper on the temple of Serapis at Pozzuoli, read before the Geological Society in 1834, and by Sir J. Herschel in letters addressed to Mr. Murchison and Mr. Lyell, communicated to that Society in 1837, and subsequently published by Mr. Babbage, together with his own paper, in his work entitled "The Ninth Bridgewater Treatise:" the extreme generality of the terms in which Mr. Babbage's application of the theory to volcanic phænomena, properly so called, had been announced, and the main object of his paper having been to explain by its means the pyrometric expansion of rocks as the cause of elevation, being assigned as reasons for his views having remained comparatively unregarded, until the more recent promulgation of views identical with them in their leading features, but more explicitly developed in their application to those phænomena, by Sir J. Herschel. The rise of the isothermal curves on the deposition of fresh solid matter on the earth's surface, their conformation to the sphericity of the earth in its central regions, but to the configuration of the surface as they approach it, and Sir J. Herschel's application of the theory to explain the elevation of continents, the production of the metamorphic rocks, and the origin of submarine volcanos and chains of volcanic vents, were severally noticed, and illustrated by reference to drawings. The attention of the auditory was directed, especially, to the generalization explaining the situation of volcanic chains along lines of coast; Sir H. Davy, it was remarked, had assigned a reason why volcanos should only exist in the vicinity of great bodies of water, but it was reserved for Sir J. Herschel to explain their actual disposition conformably to the coast lines, and also why they should exist at all.

As the temperature attained by the bottom of the newly deposited mass of strata would be proportionate to the thickness of that mass, and would be the same as that existing at a depth within the earth equal to that thickness, (viewing the subject in the most general approximate manner, agreeably to the amount of our present knowledge, and unavoidably disregarding a multitude of considerations which must enter into the discussion of the problem, in order to its exact solution,) the table of temperatures was now again referred to; for the purpose of indicating, on the one hand, how great a thickness of deposited matter would be required for the original surface to rise to even a moderate temperature; but, on the other, at how insignificant a depth (or thickness,) compared to the earth's radius, adequate temperatures would occur; the latter consideration being illustrated by a diagram exhibiting the relative magnitudes of various depths corresponding to some of the temperatures in the table, as referred to a radius of twenty feet, representing that of the earth. Thus, at a depth equal to the height of Snowdon, 3571 feet, upon the average assumed, a temperature of 71° only would be found; at two miles' depth, only the temperature at which water boils at the earth's surface under ordinary pressure, or 212°; and a depth of 30,600 feet, about 5¾ miles, (considerably more than equal to the elevation of the highest peak of the Himalayas) must be attained, before we arrive at a temperature adequate to the fusion of lead, or 612°; whereas the fusion of some of the most refractory earthy substances would be required for the production of volcanic phænomena. At the depth of about 26 miles, however, which is less than $\frac{1}{150}$th of the earth's radius, a temperature sufficient for the fusion of cast iron would occur (which would of itself be sufficient also for that of many earthy and alkaline compounds); at little more than 33 miles deep would be attained the greatest heat of a flint-glass furnace; at 37 miles soft iron would melt*; at 50 miles, a temperature of above 5000° Fahr. would be obtained, and at 100 miles, only $\frac{1}{40}$th of the earth's radius, one of nearly 11,000°, either of which, from all analogy, would be more than adequate to the effects required. If the immense activity generated by such heats may be admitted to take place at these depths, there will be no difficulty in conceiving its extension upwards to depths less great, and finally to the surface of the earth.

The second part of the subject was commenced by a statement of the chemical theory of volcanos, as originally announced by Sir Humphry Davy, in the Phil. Trans. for 1808, and afterwards repeatedly advocated; admitted by him, even when he finally relinquished it in favour of the theory of an ignited nucleus of the earth, to be adequate to the explanation of all the phænomena; and subsequently adopted and expounded in greater detail by Dr. Daubeny. The inference was then drawn, that if the theory of volcanos dependent on that of the secular variation of the isothermal surfaces were true, (and on this point a strong affirmative opinion was expressed) then the chemical theory must also be true, as being necessarily involved in the former. This was argued principally on the following grounds: the new deposits formed at the bottom of the sea by detrital matter must inevitably contain much carbonaceous and other combustible materials derived from organized beings, and these would become distributed, sometimes in a finely-divided state, intimately mingled with earthy bodies,—that is, with the oxides of the earthy, alkaline, and common metals. At the exalted temperatures implied in the theory, many of these oxides, including those of the earthy and alkaline bases, would become reduced to the metallic state; the ignited water with which the whole would of necessity be saturated, would be decomposed; its oxygen re-oxidizing the bases, and its hydrogen being evolved in an uncombined state. Now one of the most abundant elements of all the detrital matter would necessarily be oxide of iron, which would thus be presented, in a state of minute division, to incandescent, but enormously compressed free hydrogen, by which, agreeably to known results of experiment, it would be reduced to the metallic form, water being re-composed. A new affinity would now come into action: finely divided metallic iron being in intimate contact with the earthy and alkaline oxides, they would be reduced, as in the ordinary method of obtaining potassium and the process by which Davy and Berzelius first succeeded in de-oxidizing the combustible bases of silica and alumina, and would eventually re-act upon the water still present. By this constant circulation of affinities, exerted simultaneously in different portions of the heated mass, according to their respective temperatures and to the local distribution within it of the various substances evolved, (dependent on their respective properties, as modified by the enormous pressures to which they would be subject,) chemical equilibrium would alternately be established and subverted; and all the phænomena and effects of plutonic and volcanic action would ensue —as originally suggested by Davy;— but as simple consequences of the secular variation of the isothermal surfaces, as explained by Babbage and Herschel; the latter theory being a wider generalization, and including the former as one of its elements.

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* As the expressions of many of these high temperatures in degrees of Fahrenheit differed from the estimates commonly stated, it may be well to add that they were obtained from the experimental results of Wedgwood, Prinsep, Prof. Daniell, and others, all having been corrected agreeably to the pyrometrical researches of the chemist last named.

Mr. Poulett Scrope's views of the origin and constitution of lava, &c. hitherto regarded as so anomalous, were also briefly alluded to as other probable truths involved in the new theory; and the communication terminated with some general reflections, including the remark, that the names of Babbage and Herschel united would now descend to posterity associated with the history of geological dynamics, as they had already been with the improvement of many branches of mathematical analysis, and with the progress of some of the more recondite departments of physical science.

May 18.—Mr. Faraday on the solid, liquid, and gaseous state of carbonic acid, illustrated by Thilorier's apparatus.

LXXX. *Notices respecting New Books.*

Description d'une Collection de Minéraux, formée par M. Henri Heuland, et appartenant à M. C. H. Turner, de Rooksnest, dans le Comté de Surrey en Angleterre. Par A. Lévy, Membre de l'Université de France, etc. Trois volumes, avec un Atlas de 83 *Planches.*

WE have quite accidentally omitted sooner to notice the above very valuable work on Mineralogy, for such we deem it to be, although described merely as a catalogue. Those who are engaged in the examination of minerals, and particularly of their crystalline forms, cannot fail to derive much useful assistance and instruction from this publication. It appears from a notice prefixed to the work, we presume by M. Heuland, that many years had been occupied in forming the collection, and under peculiarly favourable opportunities, for obtaining the finest and most rare specimens of every variety of mineral. The prefatory notice also states that the descriptions were drawn up several years since by M. Lévy, and might, if he had acted with good faith towards M. Heuland, have been long since published. It is however stated, that after receiving a large sum of money during the progress of the work, that M. Lévy "promettait bien de terminer les planches, mais il promettait toujours, et n'achevait rien." It is afterwards even stated that for the completion of the work M. Heuland lies under obligation to the friendship of Mr. J. H. Brooke and his son, and other gentlemen whom he names.

The delay has, however, very little, if at all, impaired the usefulness of the work to the mineralogist, in reference to their geographical relations, and the great variety of their crystals.

It is difficult to select, but we would direct the reader's attention particularly to the extensive series of forms of carbonate of lime, sulphate of barytes, topaz, epidote, pyroxène, and idocrase; and among the metals red silver, blue carbonate of copper, and other of the less common substances; and indeed, we cannot but express our surprise that so large a collection, containing so great and such a splendid variety of crystals, should be found in any single cabinet; and if it were not for the full, and, we conclude, accurate descriptions of the specimens given in the work, we should greatly regret that they were rendered less acceptable than the cultivator of mineralogy would desire, by the distance of Mr. Turner's residence from London.

To give some idea of the extent of the collection, we may remark, that of carbonate of lime, the substance first mentioned, 513 varieties are described; the crystalline forms of it occupy ten plates; these require 158 figures, which are drawn and engraved with great clearness and precision: besides these, the crystalline forms of arragonite and magnesian limestone occupy two plates. The analyses and chemical formulæ of each substance are included in its description, and we do not think that we can give a better idea of the manner in which the work is conducted than by copying a portion of it which relates to red silver, of which 55 varieties and 106 specimens are described, with engravings of 39 crystals.

The first 14 varieties, all we think it requisite to give, are thus described:

"ARGENT ROUGE.

"*Caractères définis.*

		Thénard.	Formule chimique.
" *Analyse.*	Argent....	58.	2 A *n* $S^2 + 3$ A*g* S^2.
	Antimoine..	23,5.	
	Soufre....	16.	
	Perte	2,5.	
		100,0.	

"*Forme primitive.* Rhomboïde obtus de 108° 30′.

"*Clivage.* Parallèle aux faces du rhomboïde primitif.

"*Pesanteur spécifique.* 5,5886... 5,846. Cristaux de Beschertglück.

"*Caractères indéfinis.*

"*Chimiques.* Au chalumeau, décrépite en répandant une odeur

assez semblable à l'odeur d'ail de l'arsenic, mais sensiblement plus faible; si l'on contiuue le feu on obtient un bouton blanc métallique qui est de l'argent pur.

"*Cassure.* Conchoïdale.

"*Dureté.* Cassant, facile à racler avec le couteau.

"*Couleur.* Rouge vif, ou gris de fer; translucide et transparent lorsqu'il est rouge, opaque lorsqu'il a le brillant métallique.

"1re VARIÉTÉ, SIGNE REPRÉSENTATIF. $p\,d^1$.

"*Le prisme hexaèdre régulier, terminé par les faces du rhomboïde primitif.* (Prismé, Haüy.)

"1. Rouge clair, transparent, cristaux nets, avec chaux fluatée cubique, et un peu de braunspath; mine Morgenstern, Freyberg, Saxe.

"2. Rouge clair, transparent, avec chaux carbonatée dodécaèdre; mine Himmelsfürst, Freyberg.

"2me VARIÉTÉ, SIGNE REPRÉSENTATIF. $d^{\frac{3}{2}}$.

"*Dodécaèdre aigu à triangles scalènes.*

"3. Gris métallique, cristaux dont les faces sont sensiblement striées parallèlement aux bords inférieurs du rhomboïde primitif, et dont quelques-uns sont creux, avec chaux carbonatée corrodée; Himmelsfürst.

"3me VARIÉTÉ, SIGNE REPRÉSENTATIF. $a^1\,d^1$.

"*Prisme hexaèdre régulier.* (Prismatique, Haüy.)

"4. Rouge peu foncé, cristaux groupés, avec un peu de braunspath; Johanngeorgenstadt, Saxe.

"5. Noirâtre, avec argent sulfuré triforme, cobalt arseniaté terreux, et chaux fluatée violette cubique, sur du cobalt arsenical amorphe; mine Neuen Morgenstern, Freyberg.

"4me VARIÉTÉ, SIGNE REPRÉSENTATIF. $b^1\,d^1$.

"*Prisme hexaèdre régulier, terminé par les faces d un rhomboïde plus obtus que le rhomboïde primitif.*

"6. Rouge mat, cristaux nets, avec un peu de chaux carbonatée, sur plomb sulfuré; mine Samson, Andreasberg, Hartz.

"7. Gris métallique, cristaux groupés; mine Morgenstern.

"8. Gris métallique, cristaux nets, sur chaux carbonatée prismatique, avec stilbite nacrée, et plomb sulfuré lamellaire; mine Neufang, Andreasberg, Hartz.

"9. Gris métallique, éclatant, cristaux dont les faces latérales sont striées longitudinalement, sans adhérence, (pèse 2 onces 12 pennyweights); mine Morgenstern, Freyberg, Saxe.

"10. Gris métallique, éclatant, gros prisme, dont les faces latérales sont striées longitudinalement, recouverts de petits cristaux de la même substance, sans gangue, (pèse 7 onces et demie); mine Morgenstern.

"11. Rouge foncé, transparent, petits cristaux nets, avec argent sulfuré fragile de la seconde variété, sur fer sulfuré granulaire; mine Churprinz, Freyberg.

"12. Rouge clair, petits cristaux nets, sur cobalt arsenical cubique, recouvrant du cobalt oxidé terreux; Annaberg, Saxe.

"13. Irisé à la surface, cristaux aplatis, disposés en forme de bouquet, sur quarz amorphe; mine Beschertglück, Freyberg.

"14. Rouge foncé, petits cristaux, avec d'autres d'un rouge clair qui offrent aux sommets les faces du rhomboïde primitif, avec chaux carbonatée dodécaèdre raccourcie, sur fer sulfuré magnétique; mine Churprinz."

We trust we have now exhibited sufficient proofs of the value of this work, and we again repeat our opinion that it will prove highly useful to the cultivators of Mineralogical Science, and it must, we think, accompany every collection of the slightest degree of importance.

LXXXI. *Intelligence and Miscellaneous Articles.*

ON THE ELECTRICAL CURRENTS PRODUCED DURING THE PROCESSES OF FERMENTATION AND VEGETATION. BY MR. JAMES BLAKE, MEDICAL STUDENT, UNIVERSITY COLLEGE, LONDON.

BEFORE describing the experiments I have made on this subject, I shall relate the manner in which I arranged the apparatus in order to detect any electrical currents which I expected would be produced during the decomposition of the sugar. Having observed that during the earlier stages of fermentation the yeast remains in a layer on the bottom of the vessel, I placed a disc of platinum at the bottom of the vessel and another disc in contact with the surface of the liquid; these were connected to platinum wires, which were again soldered to copper wires serving to connect them with the galvanometer. The wire in connection with the lower disc of platinum was passed through a glass tube closed at the bottom so as to prevent its being in contact with the upper portion of the fluid. The vessel was of earthenware and contained about 3½ gallons.

The galvanometer contained 360 coils, the needles forming an almost astatic arrangement, and weighing, together with the wire which connects them, 2 grs. For convenience of reference I shall call the wires which connect the platinum in the fluid with the galvanometer *b* and *c* respectively; the former being attached to the lower, and the latter to the upper disc. The apparatus being arranged, and the jar containing wort in which the process of fermentation had lately commenced, I found on connecting the wires *c* and *b* by means of the galvanometer, that a current of electricity passed through it, which entered by the wire *c*, indicating that the upper part of the fluid was positive in relation to the lower. I allowed the wires to remain, in order to see if any change took place either in the quantity or direction of the current, during the subsequent stages of fermentation. For this purpose I made observations from time to time, and found, for some hours after the process of fermentation had commenced, a gradual increase in the quantity of electricity set in motion; after a certain time this arrived at a maximum, and then gradually decreased, until no indication of a current was afforded. In a short time, however, a deflection of the needle was again produced, but in an opposite direction. This also gradually increased, arrived at a maximum, and then decreased, ceasing apparently with the process of fermentation.

On attempting to investigate the cause of these curious phænomena, I was led to conclude that they were connected with a *catalytic* decomposition undergone by the yeast. The reasons which led me to adopt this explanation were, first, that these phænomena would not admit of being explained by supposing them to result from a mere ordinary chemical decomposition; secondly, I had noticed that the change in the direction of the current took place on the first appearance of a considerable quantity of yeast on the surface; and, thirdly, supposing the galvanic currents to be caused by a *catalytic* decomposition, their direction was the same as results in the analogous appearances produced by the contact of spongy platinum, and some metallic oxides, with the peroxide of hydrogen.

In order to ascertain, if possible, the correctness of this opinion, I placed some tow covered with the solid part of yeast well washed, on the surface of a piece of platinum, the tow being necessary in order to make the yeast remain on the platinum. This was placed in a jar of wort the temperature of which was about 66° Fahrenheit. I then introduced another piece of platinum into the fluid, and upon connecting these through the galvanometer there was a deflection of 10°, indicating that the positive current entered the galvanometer by the wire which was not in contact with the yeast. I have repeated this experiment many times, and always with the same result.

From all that has been observed, I conclude that when yeast comes into contact with saccharine matter in circumstances favourable to fermentation, the yeast assumes a negative electrical state, causing the surrounding fluid to become positive.

Whilst conducting these experiments I also endeavoured to ascertain the effect of a galvanic current on the process of fermentation, when passing through a fermenting fluid.

The current used was that resulting from a single pair of plates so arranged that the fluid to be experimented on formed part of the liquid conductor between them without the metals being in immediate contact with it. I found the effect to be that the process of fermentation was constantly accelerated by the passage of a galvanic current through the fermenting fluid, the sp. gr. always diminishing more quickly than in a portion of the same fluid, through which no current was passed. This takes place equally if the current pass from the surface to the lower part of the fluid, or in the reverse direction.

Electrical currents produced during the process of vegetation.—I have lately ascertained that the decomposition going on at the surface of leaves gives rise to electrical currents. The manner in which I have been able to demonstrate this, is by placing a leaf in water, the stalk remaining out of the fluid. A platinum wire is placed in the stalk of the leaf, another wire being placed in the water on the surface of the leaf. On connecting these wires with the galvanometer, a current passed through it which entered from the wire in connection with the stem. This would tend to show that the changes taking place on the surface of the leaf cause it to assume a positive state of electricity, negative electricity being given off to the surrounding medium. The direction of the current is not affected by the presence or absence of light, but a greater quantity of electricity is set in motion during the day than in the night.—*Oct.* 26, 1837.

ON THE DECOMPOSITION OF WATER BY THERMO-ELECTRICITY. BY MR. F. WATKINS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

As the decomposition of water by thermo-electricity is an experiment which must interest all persons engaged in philosophical pursuits, I venture to trespass on your pages to explain how it may be rendered visible in a marked and distinct manner, which, until now, I believe, has not been accomplished.

We learn from the *Bibl. Univ.*, vol. li. p. 337, that in 1833 M. Botto of Turin, succeeded in decomposing water, and acid and saline solutions by means of thermo-electric currents, making use of metallic elements formed not of a series of bars, but of a great number of platinum and iron wires placed alternately. The electrodes in his decomposing apparatus were *oxidizable* metals.

M. Aug. De la Rive informed me, last autumn, that he had repeated M. Botto's experiments, and noticed a few minute bubbles of hydrogen given off at the proper electrode. A communication commences in the Lond. and Edinb. Phil. Mag., No. 62., May 1837, p. 415, from Professor Wheatstone, informing us that Professor Linari of the University of Siena had, in 1836, succeeded in decomposing water with a Nobili's thermo-electric pile; that wires were employed having *oxidizable* electrodes, and that hydrogen was sensibly evolved at one of them.

The escape of a few minute hydrogen bubbles at the proper electrode is sufficient to stamp the fact, on the mind of the philosopher, that chemical decomposition has been effected; but to illustrate the phænomena enlarged effects are required, which cannot be obtained either by M. Botto's, or M. Linari's apparatus. By adopting the plan I shall now proceed to point out, streams of the two liberated gases, consequent upon the decomposition of water when *platinum* electrodes are used, may be seen and collected in the ordinary way.

I employ a massive thermo-battery with pairs of bismuth and antimony, a small apparatus for the decomposition of water of the ordinary description, and an electro-dynamic helical apparatus. The primary coil of wire is 90 feet long, and when the thermo-battery current simply pervades this coil, I do not notice any disengagement of the gases; but so soon as the contrivance for making and breaking battery contact is put in action, then an evolution of the gases takes place, while at the same time powerful shocks are received from the secondary coil of wire 1500 feet long.

I remain, Gentlemen, yours, &c.

5, Charing Cross, March 3, 1838. FRANCIS WATKINS.

DISCOVERY OF THE NORTH-WEST PASSAGE, UNDER THE INSTRUCTIONS OF THE HUDSON'S BAY COMPANY. BY MESSRS. DEASE AND SIMPSON.

In 1826 Sir J. Franklin and Capt. Back followed Sir A. Mackenzie's course to the mouth of the river which bears his name, and coasted 370 miles of the Polar Sea to the westward, tracing the northern shores of America till within 160 miles of Point Barrow, which was reached by Mr. Elson, the master of the vessel under the command of Captain Beechy, only four days after Franklin had been obliged to return. The intermediate portion has hitherto remained a blank on our maps; but the unexplored country between Franklin's Return Reef, in lat. 70° 26′ N., long. 148° 52′ W., and Point Barrow, in lat. 71° 23′ 33″ N., long. 156° 20′ W., has been, as the public have recently learned, successfully traced by Messers. P. M. Dease and Thomas Simpson, acting under the instructions of the Hudson's Bay Company, issued by their resident Governor Mr. George Simpson, to whom the formation and equipment of the expedition had been intrusted. The party started from Fort Chipeweyan on the 1st of June 1837, reached the ocean by the most westerly mouth of the Mackenzie on the 9th of July, and Franklin's Return Reef on the 23rd, where their survey commenced. They proceeded by sea to explore the coast, until they arrived, on July 31st, at a point which they subsequently named Boat Extreme, in lat. 71° 3′ 24″ N., and long. 154° 26′ 30″ W. There now appearing little prospect of their being able to reach Point Barrow by water, Mr. T. Simpson undertook to complete the journey on foot, and acccordingly started on the 1st of August with five men, Mr. Dease and the other five men remaining in charge of the boats. On August 4th (apparently) Mr. Simpson reached Point Barrow. The party arrived at the western mouth of the Mackenzie, on their return, on the 17th of August, and at Port Norman, on the 4th of September, whence their report is dated on the following day.

The expected further results of the expedition in the ensuing summer, will be undertood by the following extract from Mr. G. Simpson's instructions to the explorers:—"The object is to trace the coast from Franklin's Point Turnagain eastward to the entrance of Back's Great Fish River. To that end you will haul your boat across, from the north-eastern extremity of Great Bear Lake to the Coppermine River, before the winter breaks up, and at the opening of the navigation proceed to the sea, and make as accurate a survey of the coast as possible, touching at Point Turnagain, and proceeding to Back's Great Fish River, if the strait or passage exists which that officer represents as separating the main land from Ross's Boothia Felix; but should it turn out on examination that no such strait exists, and that Captain Ross is correct in his statement, that it is a peninsula, not an island, you will in that case leave your boat and cross the isthmus on foot, taking with you materials for building two small canoes, by which you may follow the coast to Point Richardson, Point Maconochie, or some other given spot, that can be ascertained as having been reached by Capt. Back. And you will be regulated in determining whether you will return to Great Fish River or by the coast by the period of the season at which you may arrive there, the state of the navigation, and other circumstances. In order to guard against privation, in the event of your returning to Great Fish River, it will be advisable to make arrangements at Great Slave Lake, that a supply of provisions, with ammunition and fishing-tackle, babiche for snow-shoe lacing, be deposited at Lake Beechy, or some other point of that route. Should you be unable to complete the voyage to the eastward from Coppermine River in one season, you may take up your quarters with the Esquimaux for the winter, so as to accomplish it the following season."

AFRICAN DISCOVERY.

At the meeting of the Geographical Society on the 28th of May, much interest was excited by the proposition of a plan for exploring the course of the sources of the Western branch of the Nile, by employing for this purpose the son of a native Melech of Dongola, who was present at the meeting, and is said to be well qualified for the undertaking.

The advantages of employing a native, well acquainted with the tribes on the banks of the White River, has led to the commencement of a subscription for this interesting enterprise.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1838.

Chiswick.—April 1. Cold and dry: frosty. 2. Sharp frost: cold and dry. 3—5. Fine. 6. Cloudy. 7. Rain. 8. Cloudy. 9—14. Very fine. 15. Fine: clear and windy. 16. Hail showers in forenoon: snow. 17. Cloudy and cold: showery at night. 18. Cold and dry. 19. Slight snow: overcast and cold. 20. Sleet and hail. 21. Fine. 22. Rain: fine. 23. Very fine: rain at night. 24, 25. Fine. 26, 27. Bleak and cold. 28, 29. Cold and dry. 30. Slight rain. The mean temperature of this month was four degrees below its usual average at this place.

Boston.—April 1. Snow. 2—4. Fine. 5. Fine: rain P.M. 6, 7. Cloudy: rain early A.M.; rain P.M. 8. Stormy: rain early A.M.; rain P.M. 9. Cloudy. 10. Rain. 11. Cloudy: 3¾ P.M. thermometer 65°. 12. Fine. 13. Cloudy. 14. Fine: rain P.M. 15. Cloudy: stormy P.M. 16. Stormy: snow P.M. 17. Stormy: snow early A.M. 18. Stormy: snow P.M. 19, 20. Cloudy: snow early A.M. 21. Cloudy: large quantity of hail A.M.: rain P.M. 22. Cloudy. 23. Fine: rain P.M. 24. Fine. 25. Cloudy: rain P.M. 26. Stormy. 27. Rain. 28, 29. Fine. 30. Rain: snow early A.M.: rain P.M.

Applegarth Manse, Dumfriesshire.—April 1. Clear and frosty. 2. Shower of snow: melted. 3. Moist: showery: cold. 4. Showery but mild. 5. Wet: cleared up: fine day. 6. Wet: blowy: cleared up. 7. Wet all day. 8. Dry: hills covered with snow. 9. Dry: cold: frosty morning. 10. Wet: showery all day. 11. Stormy, and wet P.M. 12. Stormy: dry: cold. 13. Clear and cold. 14. Slight showers. 15. Showers: violent wind. 16. Cold and stormy: frosty. 17. Cold and boisterous. 18. Frosty A.M.: very cold. 19. Cold and withering. 20. Still cold and barren. 21. Hoar frost A.M.: dull evening. 22. Dull and cloudy: no frost. 23. Slight rain A.M.: cleared up. 24. Slight rain A.M.: cleared up. 25. Cold and ungenial. 26. Very withering. 27. Hoar frost A.M. 28. Still withering. 29. Looking like rain. 30. Rain: cleared up P.M.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfriesshire.*

Days of Month. 1838. April.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries.		London: Roy. Soc. 9 a.m.	Chiswick. 1 p.m.	Bost.	Dumfries.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries.	Lond.: Roy. Soc 9 a.m.
		Max.	Min.		9 a.m.	9 p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		9 a.m.	9 p.m									
☾ 1.	30·204	29·142	29·140	29·83	30·12	30·02	34·5	51·6	30·3	45	16	36	30	30	NNE.	N.	calm	W.	...	...	...		32
2.	30·056	30·094	29·950	29·57	29·85	29·80	36·3	40·8	29·8	49	29	35	32½	33	WSW.	W.	calm	NE.	...	...	...		29
3.	29·978	29·991	29·939	29·54	29·72	29·61	39·4	47·3	34·3	58	39	38	38	42	N.	NW.	calm	SW.	...	...	...		33
4.	29·928	30·022	29·916	29·38	29·63	29·70	47·8	49·7	39·3	60	41	48·5	46	44	W.	W.	W.	W.	·016	·01	...		39
5.	30·014	30·033	29·903	29·45	29·60	29·50	51·4	55·2	44·9	59	46	50	44	43	W.	W.	calm	SSW.	...	·02	...		40
6.	29·706	29·753	29·709	29·13	29·45	29·50	52·8	56·9	48·3	60	49	52	45	39½	W.	SW.	calm	SW.	·036	·03	·37		45
7.	29·550	29·587	29·200	29·63	29·24	28·83	51·7	58·8	50·3	52	39	51	42	36	SW.	SW.	calm	E by S	·033	·05	·15		48
8.	29·188	29·606	29·236	28·70	29·10	29·42	41·8	52·8	41·5	51	38	41·5	39	35	W.	NW.	N.	NW.	·200	...	·23		42
9.	29·762	29·997	29·767	29·27	29·64	29·65	43·7	49·6	40·0	52	32	40	37	38	NW.	N.	N.	SW.	...	...	...		37
○ 10.	30·060	30·155	30·071	29·52	29·59	29·75	50·7	51·5	41·2	64	35	44	43	43	SSW.	SW.	W.	SE.	...	...	...		42
11.	30·184	30·188	30·053	29·56	29·78	29·75	54·0	59·2	48·8	69	35	55	46	35	S.	SW.	calm	S.	...	...	·02		46
12.	30·230	30·265	30·212	29·53	29·90	30·09	49·6	64·2	43·0	59	32	48·5	44	36	NW.	NW.	NW.	WNW.	...	...	...		44
13.	30·276	30·287	30·081	29·77	30·09	29·85	47·0	56·4	38·0	52	32	43	42	42	NW.	N.	calm	W.	...	...	...		40
14.	29·986	30·015	29·991	29·53	29·83	29·60	46·2	50·8	43·8	54	42	45·5	43	42	S.	N.	calm	W.	...	...	...		42
15.	29·792	29·803	29·717	29·23	29·40	29·38	47·0	50·3	44·2	61	35	49	46	35	SW.	NW.	W.	NW.	...	·03	·06		42
16.	29·718	29·758	29·644	29·15	29·39	29·40	43·5	56·8	37·8	46	29	42	39	34	NW. var.	NW.	W.	NW.	·050	·01	...		34
☽ 17.	29·628	29·753	29·657	29·16	29·43	29·60	40·8	46·7	33·3	47	32	40	39	33	NW.	NW.	NW.	NW.	·025	·03	·04		32
18.	29·724	29·787	29·762	29·33	29·66	29·74	38·7	46·3	34·4	46	32	38·5	31½	32	NW.	NW.	N.	NNW.	·075	·01	...		33
19.	29·716	29·773	29·751	29·36	29·74	29·74	36·2	44·7	34·4	45	26	38	38	34½	NW.	N.	NE.	NW.	·011	·02	·10		35
20.	29·772	29·894	29·754	29·40	29·74	29·70	41·3	44·0	35·3	46	32	37	42	33	NNW.	N.	calm	NNW.	...	·22	·04		34
21.	29·658	29·691	29·424	29·24	29·47	29·19	41·8	44·0	36·2	52	30	41	40½	39½	NW.	W.	calm	SW.	·127	...	·42		35
22.	29·182	29·353	29·220	28·90	29·07	29·18	41·9	48·3	36·8	52	37	43·5	42	41	E. var.	S.	S.	E.	...	·01	...		35
23.	29·372	29·399	29·320	29·03	29·19	29·30	50·3	51·5	41·0	57	36	51·5	40	40½	S.	S.	S.	E.	...	·04	...		37
● 24.	29·460	29·703	29·465	29·14	29·58	29·74	44·8	54·7	40·8	56	31	47	43	40	NE.	SE.	NE.	E.	·055	...	·08		40
25.	29·740	29·818	29·759	29·40	29·86	29·97	47·0	50·6	38·2	52	42	43	43	43	NNE.	NE.	NE.	E.	·036	...	...		41
26.	29·864	30·015	29·891	29·52	30·01	30·05	46·0	51·0	43·0	49	35	45	44	38	NW.	N.	NE.	NE.	...	·01	·09		42
27.	29·926	29·975	29·921	29·59	29·98	29·98	43·8	49·2	37·8	49	32	43	44½	36	NNE.	N.	E.	NE.	...	...	...		37
28.	29·840	29·879	29·748	29·45	29·85	29·71	43·8	47·7	35·0	52	32	43	45	39	NNE.	N.	NE.	NW.	...	...	...		35
29.	29·708	29·740	29·712	29·31	29·69	29·53	41·2	50·4	36·2	50	34	43	40	36½	NW.	N.	NE.	NW.	...	...	...		32
30.	29·546	29·589	29·535	29·11	29·29	29·35	45·7	47·0	38·8	56	44	36·5	37	33½	SE.	SW.	NE.	NE.	...	·03	·08		35
Mean.	29·792	29·835	29·738	29·33	29·63	29·62	44·7	50·9	39·2	53·33	34·8	43·0	40⅞	38					Sum.	·52	1·68	1⅒	Mean.

LONDON AND EDINBURGH
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE,
SUPPLEMENT TO VOL. XII., THIRD SERIES.

LXXXII. *Researches on Heat. Second Series.* *By* JAMES D. FORBES, *Esq., F.R.SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.*[*]

§ 1. *On the Use of the Thermo-Multiplier.* § 2. *On the Polarization of Heat by Tourmaline.* § 3. *On the Laws of the Polarization of Heat by Refraction.* § 4. *On the Laws of the Polarization of Heat by Reflection.* § 5. *On the Circular Polarization of Heat.*

THE first series of these researches, in the exact form in which they are printed, were laid before the Royal Society on the 19th January 1835[†]. The whole of the experiments there described were made, and the paper written and printed, within a space of time little exceeding two months. This haste, unfavourable to composition, I considered as a less evil than postponing for a period of time, which must have been considerable, the publication of a class of facts which might be said almost to embrace a new science. The professional duties which pressed upon me during the whole continuance of those experiments, then called imperatively on my attention, and during the remainder of the Session my time was devoted to them. The summer I devoted to a foreign excursion, some of the results of which I afterwards digested; and it was not till the commencement of the winter session which has just closed, that I prepared, with a fresh stock of health and spirits, to reinvestigate the whole subject of the Polarization of Heat, and to assign numerical values to the effects, whose *existence* I had before been contented to prove.

§ 1. *On the Use of the Thermo-Multiplier.*

I have succeeded in rendering the application of the thermo-multiplier considerably easier, and more delicate than formerly. In my last paper, art. 5, I described the application of the telescope to determine accurately the amount of the deviation of the needle of the instrument indicating degrees of temperature. I have made the arrangement more permanent, placing the instrument on one shelf of a solid press, and clamping a little arm carrying the telescope, and centred at a point in the prolongation of a vertical line, passing through the centre of the card of the galvanometer, to a shelf above. The little arm bearing the telescope, therefore, traverses the divided part of the galvanometer card, just as a micrometer does the limb of an astronomical circle. The result of this application of optical power is, that equally accurate conclusions may be drawn within small limits of deviation, as if the deviations were greater, and observed in the ordinary way. This is important on several accounts, 1. The instrument is not liable to those derangements which follow from exposure to considerable heat,—derangements difficult to allow for, and which I have not succeeded in obviating. 2. The value of the degrees is more nearly uniform, and less liable to abrupt change; so that (as will presently be seen) within the narrow limits under which I am accustomed to operate, the deviations are almost as the forces. 3. The motions of the needle being much more speedy and certain within small ranges (particularly in its return to zero) much time is saved, and the consecutive observations are more accurately comparative. 4. By the use of the telescope all parallax in reading is avoided, and if a diagonal eyepiece be employed, the posture is much less fatiguing than in any other method of observation.

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[*] Abridged by the Author from the Transactions of the Royal Society, vol. xiii., having been read before the Society May 2nd, 1836.

[†] [Prof. Forbes's First Series of Researches on Heat appeared in Lond. and Edinb. Phil. Mag., vol. vi. p. 134 *et seq.*]

Besides using this optical contrivance, I have increased the delicacy of the instrument, by adding a conical reflector so as to concentrate nearly parallel rays upon the surface of the pile. This contrivance is not my own. I saw one in an instrument made on the model of M. Nobili's, in the possession of M. Quetelet, at Brussels, in 1832, which was the first multiplier I had seen. This reflector increases the sensibility of the pile to heat from a given source seven or eight times, or in a proportion not very different from the area of its aperture to that of the pile*. The length of the conic frustum which I employ is $1\frac{3}{4}$ inches, and its aperture $1\frac{3}{4}$ inches.

It is well known that the deviations of galvanometers are

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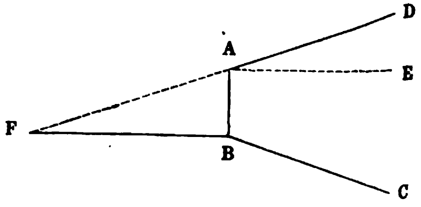
* Suppose that we wish to have a conical reflector ABCD, such that the whole of the parallel rays which fall upon it shall reach *some part* of the surface AB of the pile, which is all that we want, we have this simple construction. Let the length of the trumpet-mouth AE be given. Make FB equal and parallel to it. Join FA, and prolong the line to D, then is DAE the greatest inclination that the sides of the cone can have to answer the purpose intended.



not, generally speaking, proportional to the forces producing them, and that for the most part angular spaces at greater distances from zero correspond to increments of force greater than for equal spaces near zero. Thus to cause the needle to advance from 25° to 30° requires a force greater than to make it deviate from 0° to 5°. Also the force indicated by a deviation of 30° is more than six times the force indicated by a deviation of 5°. M. Melloni has pointed out an ingenious method of comparing the values of the different parts of the scale. This consists in employing two constant sources of heat to affect the opposite extremities of the pile, and after observing their separate effects, noting their joint effect, which will not generally be equal to the arithmetical difference of the others. Thus let one source of heat force the needle in a *positive* direction to 30° on the scale, and a second source of heat acting separately producc a *negative* deviation of 25°, the effect of both acting at once will not be a *positive* deviation of 5° merely, but probably will indicate some greater number, as 6° or 7°. Thus, a *true scale* of degrees equal in value to those near zero may be constructed.

Another mode of estimating the indications of the instrument has been used by M. Melloni, and it is one particularly adapted to our researches. It likewise gives much more uniform results than might have been anticipated. Instead of noting the *final* or stationary deviation due to any heating cause, it is sufficient if we note the arc through which the needle is *first* impelled, and employ a table of reduction, indicating the relation subsisting between the dynamical effect or first arc of impulsion, and the statical effect or that of final deviation*. My experiments have given the following results, and the last column indicates the actual intensities corresponding to the statical deviation in second column.

Dynamical effect, or first arc passed over.	Statical effect, or Permanent deviation.	Intensity.
1.0	1.2	1.2
2.0	2.3	2.35
4.0	4.5	4.65
6.0	6.7	7.1
8.0	8.9	9.6
10.0	11.1	12.05
12.0	13.2	14.4
14.0	15.3	16.9
16.0	17.4	19.35
18.0	19.45	21.75
20.0	21.5	24.3

[* See Scientific Memoirs, vol. i. p. 329.—EDIT.]

The mode of observation by the first impulsive arc I have invariably adopted for obtaining numerical results, and chiefly for these reasons: 1. It saves time, and thus renders consecutive observations comparable. 2. It prevents a long exposure of the pile to heat, which alters the zero point and injures its action. 3. It almost annihilates the effect of conduction where substances, capable of retaining heat, are placed between the source of heat and the pile.

It is a remarkable circumstance, that when *both* the corrections obtained from these tables are applied, we obtain (as far as 20° at least) a measure of intensity increasing, almost uniformly, with the arc first run through. This is found to depend on the circumstance that the curve, expressive of forces, in terms of the *stationary* deviation, is convex towards the axis, or the forces increase *more* rapidly than the arcs; whilst the curve, expressing the *stationary* in terms of the *momentary* deviations, is concave to the axis, or the Statical effect increases in a *less* ratio than the Dynamical effect. The convexity of the one curve, almost compensating the concavity of the other, the relation obtained between the first impulsive arc and the calorific force is nearly linear.

This will be best illustrated by comparing the true ratios of the forces obtained from the above table, with the simple ratio of the first deviations; and to put it in greater evidence we shall suppose a deviation of 20°, which is greater than we have ever employed in these experiments.

First deviations compared.	Ratio.	Ratio of Intensities.
20° and 1°	.050	.049
20 — 2	.100	.097
20 — 4	.200	.191
20 — 8	.400	.395
20 — 12	.600	.593
20 — 16	.800	.796

Since, therefore, even in this case, we should never have an error amounting to a unit in the second decimal place, I have contented myself in this paper with the employment of the simple arithmetical ratio of the first arcs passed over.

It appears, then, that these effects are developed on the whole in a simple and uniform manner; and though such an investigation as we have undertaken of the instrument, was necessary to give us confidence in the numerical accuracy of the results, all *facts* of importance might be determined without it, and even quantitative laws ascertained by a judicious conduct of experiments. Many persons, even though not unaccustomed to physical reasoning, have strangely inaccurate conceptions of the *limits of possible errors*. Nor is there a more important part of the science of experiment than to perceive where *physical proof* becomes satisfactory, though yet far removed from mathematical certainty. To supply the latter is a humbler and more mechanical task, which may be undertaken at leisure, as in this paper we shall partly attempt to do.

§ 2. *Polarization of Heat by Tourmaline.*

On this subject I have little to add. It does not possess the same theoretical importance as in the case of polarization by other methods. Double refraction is better shown by the phænomena of depolarization by mica, described in my last paper (art. 46 *et seq.*), and the phænomena attendant on the absorption of one of the doubly refracted pencils, are so capricious and ill understood, even in the case of light, that it might hold or not for heat without altering our views as to the probable identity of the cause of both those physical agents. With heat from incandescent platinum the effects are extremely well marked. Thus with tourmalines E and F (see First Series, 22) I obtained for the ratio of the quantities of heat transmitted, with axes of crystals parallel, and axes crossed, 100 : 76; or 24 per cent. of the heat was polarized (Jan. 19, 1836). When one of these tourmalines was combined with a mica plate, marked G, polarizing by transmission (see below art. 20), the proportion was 100 : 62, or 32 per cent. polarized of heat from incandescent platinum.

With dark heat incomparably greater difficulty was experienced. Excessively little heat could be obtained through the combined mass of tourmaline and the glass to which it is cemented, and of that little it appeared that but a minute portion was polarized, or at least absorbed by the action of the former. At one time I seriously doubted whether any perfectly dark heat came out of tourmaline polarized in one plane only. I have reason to believe that, in my first experiments, there was a source of error arising from the form of the plates, which was not adverted to formerly. I have, however, satisfied myself that even dark heat is capable of being acted upon by tourmalines in the same manner as light. In my experiments the quantity apparently polarized did not exceed one-seventh or one-eighth of the small quantity transmitted. This was in combination with a polarizing mica plate (marked I).

§ 3. *On the Laws of the Polarization of Heat by Refraction or Transmission.*

In my last paper on this subject, I stated the *fact* of the polarization of all the kinds of heat which I tried by transmission through thin bundles of mica placed obliquely. I stated the difficulties which I experienced, and the quantitative errors to which the results were liable. I showed at the same time that these errors were of a kind calculated to *mask* the effects of polarization, but not to produce them. The numerical results which I gave (I. Series, art. 44), were intended chiefly to show that, even under all these disadvantages, the effects which I observed were of an extremely important and obvious character, such as no slight or capricious anomaly could possibly have produced. No one who candidly reads the paper, or is aware of the labour of so extensive an investigation in so new a field, can suppose that I intended to give these results as to the quantities of heat, of different kinds, polarized by passing through mica bundles, as *definitive* numerical results. I certainly did suppose that the different kinds of heat were polarizable in different degrees under the same circumstances, a conclusion which I am now prepared to establish.

The extent to which the former paper had swelled, likewise prevented me from inserting the description of a multitude of precautionary measures, taken to show that errors, of whose existence I was perfectly aware, had no influence in *producing* the effects which I stated; nor am I now going to enter into details of manipulation, which the experimentalist must learn for himself, and which would be highly tedious to any other reader. I will content myself with referring to the proofs already stated in this Journal*, in justification of my experiments.

The polarizing effect takes place only at the *surfaces* of plates,—the absorptive effect depends upon their thickness. Hence, to polarize heat effectively, a minute subdivision of mica into thin laminæ is essential. This I formerly effected by a pen-knife. I have since, however, discovered a method much more effectual. A piece of mica, thrown into a brisk red fire, is split up, by the expansion of the air between its films, into a multitude of pellicles, which reflect light with almost metallic brilliancy, and polarize it intensely by transmission. Such mica plates I have used, one pair being marked G and H; the other I and K.

By experiments with both these pairs of plates I have been led to the conclusion, THAT SOME KINDS OF HEAT ARE MORE POLARIZABLE AT A GIVEN INCIDENCE THAN OTHERS. Between heat from an Argand lamp, and that from platinum rendered incandescent by the flame of alcohol, there is little difference; but heat, unaccompanied by light, is much less polarizable than luminous heat, and that apparently in proportion to the lowness of its temperature. I may take the opportunity of giving one or two examples of such experiments.

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* Lond. and Edinb. Phil. Mag., for November 1835, and March 1836.

Mica Plates I and K. *Argand Lamp, with Chimney and Reflector:* 16 Inches from Centre of Pile.

	Dynamical Effect.	Mean.	Ratio.
Parallel	15.6		
Crossed	4.2	15.67 . . .	27 : 100
Parallel	15.75		
Crossed	4.4	15.85 . . .	28 . 100
Parallel	15.95		

Dark Heat from Brass, warmed by Alcohol: 16 Inches.

Parallel	10.55		
Crossed	4.05	10.57 . . .	38 : 100
Parallel	10·6		
Crossed	3.9	10.55 . . .	37 : 100
Parallel	10.5		

These are given as examples of the usual mode of proceeding with such experiments, the zero point being ascertained between each observation, and the dynamical effect reckoned from it. On the whole, I obtained for the proportion of heat polarized, or stopped in the transverse position of the plates, the following numbers:

Source of heat.	Rays out of 100, polarized.	
	Plates I and K.	Plates G and H.
Argand lamp	72 to 74	82
Incandescent Platinum, ...	72	79
Brass heated to about 700°, ...	63	68
Heat from the same source transmitted through glass, ...	72	73
Mercury in a crucible at 410°, . . .	48	
Boiling Water, ...	44	49

It thus appears that the Plates G and H are capable of polarizing no less than 82 per cent. of some kinds of incident heat: these plates I began to use in the commencement of December last.

The unequally polarizable nature of different kinds of heat having been controverted, I took several methods of assuring myself that the observed effects were not due to inequalities in the dimensions of the sources of heat employed, or to their variable distances from the pile. A multitude of proofs might be given, but I will content myself with stating one or two of the most decisive. 1. Incandescent platinum and dark hot brass were successively placed at the same distance of twelve inches from the pile; a thin plate of glass being placed between the latter and the pile, and two thick plates of glass between the former and the pile. The quantities of heat reaching the pile were thus almost equalized, and the result was, that the *heat from a dark source, after transmission through glass, became as polarizable as that from incandescent*

platinum, although before NINE *per cent. less of it was stopped.*
2. If heat from boiling water or hot mercury be not really *less* polarizable than that from luminous bodies, it must *appear* to be so in consequence of the surface being larger, or closer to the pile, and therefore seen at the pile under a greater angle. To show that this effect, if it do exist, is at least insignificant in relation to the effect due to the variable nature of the heat, I placed the brass heated to 700° at 12 inches from the pile, and caused its rays to be *sifted* by a plate of glass. I found that 73 per cent. of this heat was polarized by the mica plates I and K. I then removed the glass plate, and withdrew the heated brass from the pile, until the impression on the pile was nearly the same as before. This was at a distance of 26 inches, instead of 12. The source of heat was therefore seen under a much smaller angle than before. But instead of the polarization being augmented by this circumstance, the change in the *quality* of the heat by the removal of the interposed glass reduced it to 64 per cent.—an effect which must have been owing to that cause, and to that alone. Another experiment similarly conducted with the mica plates G and H gave 73 per cent. of heat sifted by glass polarized at a distance of 12 inches, and only 68 per cent. of heat from brass at 700° in its simple state, at a distance of 27 inches. In all these experiments it is clear that the result is true, independently of any reduction for the degrees of the galvanometer, since in each set the deviation is made the same.

The general fact that heat from sources of higher temperature is more polarizable by refraction, agrees with the corresponding case of light. Heat of low temperature is least refrangible, and Sir David Brewster has found that light of less refrangibility is less completely polarized by a bundle of plates placed at a given angle, than the more refrangible rays*.

Mica is preeminently adapted for the purpose of polarizing by its considerable *diathermancy*, and by the extreme thinness of its laminæ. I have, however, succeeded in polarizing heat by transmission through a bundle of rock-salt plates with parallel surfaces†. Two bundles consisting of three plates, or six surfaces each, polarized about one-seventh of the heat which passed in the parallel position, the angle of inclination to the incident heat being about 55°; but when all the six plates were combined into one bundle, and the mica plate I used along with it, not far from a half of the transmitted heat was polarized*.

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* The question of the unequal polarizability of heat from different sources is resumed and very fully discussed in the Third Series of these Researches, an abstract of which will immediately follow the present paper.

† For a supply of this valuable substance I have been greatly indebted both to Sir Philip Grey Egerton, Baronet, of Oulton Park, Cheshire, and to Dr. Traill.

The following is a very convenient mode of mounting the mica plates for polarizing. A cylindrical wooden tube is cut across at an obliquity of 34° to the axis. The plate of split mica is interposed and the parts reunited. The plane of polarization or analysation may thus be made to shift through any angle by turning the tube containing the mica round its axis, and a small support is provided to preserve it in any position; whilst a graduation may easily be applied to the exterior of the tube, so as to mark the angular revolution. The convenience of this construction will afterwards appear.

§ 4. *On the Laws of Polarization of Heat by Reflection.*

The general fact of the polarization of heat by reflection was ascertained by me in December 1834, and I stated the result in my former paper, art. 45. Under any circumstances the experiment is a troublesome one, but I have succeeded in arranging it in perhaps as satisfactory a way as it admits of being done. The great difficulty arises from the minuteness of the quantity of heat reflected, and consequently the large quantity absorbed by the plates, which complicates and obscures the effect. This is more particularly true with dark heat, which, at the same time, furnishes the most important case to be examined. The effect of the absorbed heat is to produce a powerful secondary radiation.

My first inquiry on resuming the subject was to ascertain the relative order of several different substances as to their power of reflecting heat. This was not proposed to be done with a view to a general inquiry into that important subject, which I reserved to another occasion, but simply to ascertain what reflecting surfaces might be best employed in polarizing by reflection. Several series of experiments gave the following arrangement of substances according to their power of reflecting heat, at an incidence of 45°, beginning with the most perfect reflector.

Polished speculum metal.
Mica, split by the hand into thin plates.
Mica, split by heat (see p. 86).
Thick plate of mica.

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* Such plates being equally permeable to every kind of heat, as M. Melloni's admirable experiment shows, would probably enable us to *polarize cold*, or to show the negative effects due to a reduction of temperature. This experiment I have not tried.

Rock-salt, with a thin coating of varnish.
{ Polished rock-salt.
{ Glass.
{ Alum.

The three last substances (so different in their diathermancy) were nearly equal in their reflective power for dark heat (from brass about 700°). The above order did not, however, appear to be changed for heat from incandescent platinum, except that glass seemed to stand *above* alum and even salt. In a general way, we may consider the measure of metallic reflection to be from two to three times as great as that from mica split by heat, which is also very superior to a single surface of mica. Glass, salt, and alum seem to reflect but a third or a fourth part, or even less, of that furnished by the laminated mica.

I did not, as I have said, stop to prosecute these experiments; they clearly showed that of the substances which I tried, mica split into thin films afforded the most copious reflection (next to the metals), and this was the very substance which from the first I had employed. They likewise satisfactorily showed the cause of the failure in former attempts to polarize by reflection, seeing that, for dark heat, glass is almost the worst reflector that could have been used, and as it likewise absorbs almost all the heat, transmitting very little, the effect of secondary radiation is increased. Hence the difficulty experienced by Professor Powell and others in getting any results at all* before the thermo-multiplier was devised, and the failure of the attempts of Signor Nobili of Florence even with its aid†. The last-named eminent philosopher failed also in obtaining traces of polarization by metallic reflection, which was not to be wondered at, as on another occasion we shall be able to make to appear.

The form of apparatus which I have more recently employed for experiments on polarization by reflection was suggested to me by the present astronomer royal, Mr. Airy, after the publication of my first paper. It is represented in section in the figure in the opposite page.

AB and CD are two reflecting surfaces of mica fixed to blocks G and H; the former of which is attached to a board TU, carrying the lamp or source of heat S, and revolving in a horizontal plane round T as a centre;—the latter (H) is permanently fixed relatively to the pile P, provided with its conical reflector. The surfaces AB, CD are parallel, and make angles of about 56° with the horizon; consequently the heat falling on AB at an angle of 34° with the surface, is reflected in the direction EF, which, by the construction, is a vertical line.

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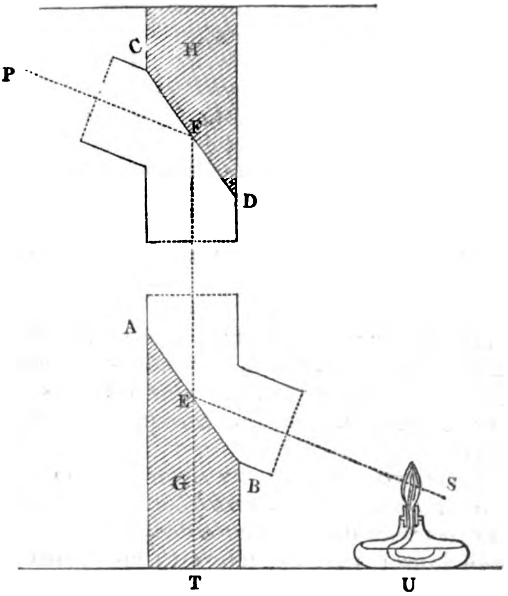
* Edinburgh Journal of Science, N. S., vols. iii. and v.

† Bibliotheque Universelle, Sept. 1834.



From the surface CD, on which, at incidence, it also falls at an angle of 34°, it is reflected to the pile, whose opening inclines downwards at an angle of 22°, so as to receive the rays directly. From this it is clear that the whole apparatus connected with the first plate AB may revolve round the vertical line EF as an axis, until the plane of section be perpendicular to the plane of the paper, and that yet the heat shall be correctly reflected to the pile. In this case it is clear that the planes of reflection becoming perpendicular, a minimum of heat will be reflected if polarization take place*.

Such appears to be the case with all the kinds of heat that I have tried. The disturbing influence of conduction is here more difficultly avoided, and serves to *diminish* the apparent effect. The quantities of heat reaching the pile from any non-luminous source are always small. The results, however, are well marked, and seem decidedly to indicate that under the particular circumstances of the observation, dark heat is *more* completely polarized than the more reflexible heat from an Argand lamp, whilst that from incandescent platinum was more polarizable than either. The following results were obtained on the 12th March, 1836. The source of heat was in all cases at a distance of six and a half inches from the centre of the first reflecting plate, and the whole length of the dotted line PFES was about sixteen inches. The reflecting plates were composed of ten or twelve laminæ of mica, split with a pen-knife, and the plane of reflection was perpendicular to the principal section of the mica.

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* Square tubes of wood serve to inclose the apparatus and facilitate its adjustment. Other means not represented in the figure were also used for preventing direct heat from reaching the pile in any position.

	Rays out of 100 polarized.
Argand lamp without reflector, ...	55
Dark heat, from brass about 700°, . . .	61
Incandescent platinum, ...	65

A mica-plate placed between the two reflecting surfaces in the figure, perpendicularly to the reflected ray, is capable of depolarizing the heat, as in the case of heat polarized by transmission (17th December, 1835). The fact is simply mentioned here, as we do not at present resume the subject of depolarization.

I made some experiments, with a view to the determination of the maximum polarizing angle for heat, with a more convenient apparatus than the one above described.

It is well known that the following law holds for polarized light. *When light, polarized in any plane, is reflected from a refracting surface* AT *the polarizing angle for that surface, it is wholly polarized in the plane of incidence. If it be incident at a* SMALLER *angle than the polarizing angle, the reflected light is polarized in a plane lying on the farther side of the plane of incidence from the plane of primitive polarization. If it be incident at a* GREATER *angle than the polarizing angle, the plane of polarization will be on the same side of the plane of incidence as at first.* Now, this I have fully verified in the case of heat. Having polarized heat by transmission through a mica bundle, in a plane inclined +45° to the plane of reflection, which it subsequently underwent at the first surface of a thick mica plate, I examined its state of polarization by another similar mica bundle interposed between the reflecting mica and the thermal pile. I found that at great incidences the plane of polarization was on the same side of the plane of reflection as at first, whilst at smaller incidences it was thrown to the opposite side. I varied the incidence until the plane of polarization coincided with the plane of reflection, when I concluded that I had reached the polarizing angle. This was found by the quantity of effect when the plane of analysation was inclined + 45° and — 45° to the plane of reflection. With *dark heat*, from brass at

700°, I estimated the polarizing angle to be 57° nearly (16th March 1836). By experiment I found that the polarizing angle for the same mica surface and for homogeneous red light was 59°.

§ 5. *On the Circular Polarization of Heat.*

In my last paper I showed (art. 75) that heat may be circularly polarized, like light, by the doubly refracting action of a plate of proper thickness. This circumstance is indicated by an equal quantity of heat reaching the pile in all positions of the analysing plate.

Last summer it occurred to me that it was probable that rock-salt, refracting heat almost as it does light, would cause it to undergo total reflection at a proper incidence. Supposing this to be the case (and I had afterwards reason to believe that such had been shown to be the fact by M. Melloni), I then foresaw the possibility of trying an experiment of the most conclusive character, as to the nature of heat,—its susceptibility of becoming circularly polarized by means of two total internal reflections, as in the admirable experiment of Fresnel in the case of light.

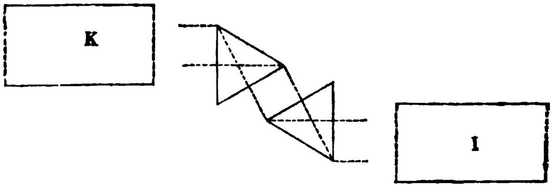
Various circumstances prevented me from trying this experiment until the end of January last [1836], when I procured a rock-salt rhomb, similar to that of glass used by Fresnel, but having its angles calculated by Fresnel's formula, for the refractive index for light of rock-salt. I took the smaller of the two angles which the double solution of the quadratic equation gives, on account of the smaller dimensions required for the rhomb. This angle is nearly 45°. On the 1st of February I performed the experiment with complete success, though with an apparatus less perfect than I afterwards procured.

When the plane of reflection *coincided with*, or was *perpendicular to*, the plane of primitive polarization, the heat (whether wholly dark, or derived from incandescent platinum) came out *unchanged*, that is, on placing the analysing plate in azimuth 0° and 90° relatively to the polarizing plate, the ratio of the effects was the same as if no reflection had taken place.

When the plane of first polarization was inclined +45° or −45° to the plane of reflection, and the analysing plate was placed in the parallel and rectangular positions to the polarizing plate, the ratio of the effects was totally changed, and was, in some instances, reduced nearly to unity. This took place whether the rhomb or the polarizing plate was moveable.

When the plates I and K were used, the ratio was raised by inclining the plane of reflection 45°, from 37 : 100 to 60 : 100; and when heat from incandescent platinum was employed, from 28 : 100 to 64 : 100.

It occurred to me, that, somewhat above the *superior* angle of total reflection indicated for light, the effect of apparent depolarization would be more perfect, and a ready way of doing this presented itself by the use of two prisms of rock-salt, having angles of 60°, with which I provided myself. The *superior* angle of total reflection for rock-salt (whose index of refraction is 1·56) is 57° 28′ nearly, for light, and since it increases rapidly as the refrangibility diminishes, it was reasonable to expect it to be still higher, or not far from 60° for *dark heat* (of low refrangibility). The two prisms, arranged as in the figure below (which is a ground-plan), fulfilled the required conditions, the dotted lines indicating the path of the rays of heat through the prisms; and the result corresponded to my expectation. When the plates I and K were used to polarize and analyse, and the planes of total reflection and polarization were parallel, the ratio in the rectangular positions of the analysing plate was 40 : 100; whilst, when the plane of first polarization was inclined 45°, the ratio was raised as high (in one series of experiments) as 94·5 : 100. With the same apparatus, and with heat from incandescent platinum, the ratio was raised from 29 : 100 to 84 : 100. Thus the astonishing properties of rock-salt enable us most completely to extend the analogies of light even in their most complicated cases to the phænomena of heat.



We are naturally led from the consideration of circular polarization produced by two known methods in the case of light, viz. by transmission through a thin doubly refracting plate, and by total reflection in a refracting medium, to consider the third mode in which it has been effected, that is, by metallic reflection. In this case, also, the analogy holds as to the general fact, which I have succeeded in completely establishing under several circumstances. Whilst the copious reflection of heat which takes place at metallic surfaces, renders it easier to obtain distinct results than in some other cases, the intricacy of the subject, and some deviations from the laws of light, as established in Sir David Brewster's remarkable paper on this subject[*], demand a more prolonged investigation than I have yet been able to give to it. In the hope of being able to resume it in another paper, I content myself at present with a reference to the facts respecting Metallic Reflection, communicated to the Royal Society of Edinburgh on the 21st of March 1836, and printed in their Proceedings[†].

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[*] Philosophical Transactions, 1830.

[†] The following is the Memorandum on the subject, extracted from the Proceedings of the above date.

"I have recently ascertained the following facts respecting heat.

"1st, That heat polarized in any plane, and then reflected from the surface of a refracting medium, changes its plane of polarization in a manner similar to what obtains in the case of light. Thus, with a thick plate of mica, which polarized homogeneous red light most completely at an incidence of about 59°, the plane of polarization of reflected polarized heat remained on the *same* side of the plane of reflection when the incidence was *great*, and was on the *contrary side* when the incidence was *small*. The limiting angle of incidence was about 57°, which therefore should be the polarizing angle of dark heat for mica. This mode of observing the polarizing angle offers some advantages above more direct methods.

"2ndly, Metals polarize heat extremely feebly by reflection. I have carried my experiments up to 85° from silver, yet even there but a small share is polarized. The effect is, however, distinctly recognisable through a considerable range of incidences. The effects are such as would indicate the maximum polarizing angle to be even much higher; perhaps it never attains a maximum. This fact corresponds to that in the case of light, except that the maximum polarizing angle is 73° (Brewster). Did metals act on light like other bodies, we should conclude, from the polarizing angle being greater, that heat is more refrangible than light. An important remark of Sir D. Brewster's, however, shows the statement I have made to be in conformity with the views of the nature of heat which I have published. He finds the maximum polarizing angle to be *greatest* for the *least* refrangible rays.

"3rdly, Heat polarized in a plane inclined 45° to the plane of subsequent reflection at silver, has its nature changed as in the case of light, and presents the conditions of elliptic polarization, though the ellipse is much more elongated, even at great angles of incidence.

"4thly, *Two* reflections from silver increase the polarizing effect of metals. This fact has its counterpart in light. Two reflections likewise produce an increased tendency to circular polarization when the plane of reflection is inclined 45° to that of primitive polarization. This effect increases with the obliquity of incidence up to considerable angles.

"These observations have been verified in the case of heat from various sources, obscure as well as luminous."

LXXXIII. *On the Composition of certain Mineral Substances of Organic Origin.* *By* JAMES F. W. JOHNSTON, *A.M., F.R.SS. Lond. and Ed., F.G.S., Professor of Chemistry and Mineralogy, Durham.*[*]

IV. *Retin Asphalt.*

THE substance described under this name by Mr. Hatchett is well known to mineral collectors as occurring in the wood coal deposit of Bovey[†]. It is met with in lumps of various sizes, generally of an earthy aspect and fracture, rarely compact and glistening, and of a colour more or less brown. Throughout its substance are frequently observed small portions of carbonaceous matter, long, small, apparently pointed, and when broken across exhibiting under the microscope a hollow quadrangular cavity as if they were the remains of slender spines, or of leaves allied to those of the Coniferæ. In the air it melts when heated, burns with a bright white light, much smoke, a slightly aromatic odour, and leaves a pure white ash consisting of alumina with a little silica. Alcohol dissolves a large portion of it, giving a dark brown solution, and leaving a light brown residue. This residue still contains a large quantity of organic matter, which appears, however, to possess in common with asphaltum, which Mr. Hatchett supposed it to contain, no other property but that of being insoluble in alcohol. A portion of the retin asphalt carefully burned left 13·23 per cent. of residuum, after exhaustion by boiling alcohol 32·52 per cent. It consisted, therefore, of

Resin soluble in alcohol........	59·32	100.
Insoluble organic matter	27·45	
White ash..................	13·23	

This proportion of the constituents is probably variable. The insoluble matter heated in a tube, blackens and gives off empyreumatic products. At a red heat in the open air it burns.

Resin of Retin Asphalt. Retinic Acid.

Evaporated and the residue dried at 212°, the dark brown alcoholic solution leaves a light brown resin largely soluble in æther, (from which alcohol throws down the greater part,) and less so in alcohol, from which it is wholly precipitated by water. At 212° it emits a peculiar resinous odour, which becomes more perceptible as the temperature is raised. At 250° Fahr. it begins to melt, and at the same time to lose in

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[*] Communicated by the Author.

[† Mr. Hatchett's account of this substance will be found in Phil. Mag., First Series, vol. xxi. p. 147.—EDIT.]

325

weight; at 320° Fahr. it is perfectly fluid, and at 400° it gives off minute bubbles as if slowly effervescing.

6·885 grs. dried at 212° raised to 250° had lost 0·06, to 320° —0·09, and to 400° —0·24 gr.

Dried at 212° and burned with oxide of copper 6·492 grs. gave water 5·11, carbonic acid = 18·045. → 4.921

Heated to perfect fusion 7·29 grs. gave water = 5·58, carbonic acid = 20·41 grs.

These results are equivalent to

	Dried at 212°.	At 300°.	
Carbon.........	= 76·860	77·414	75.51
Hydrogen ...	= 8·749	8·508	8.75
Oxygen	= 14·395	14·078	15.44
	100·	100·	100

Calculated according to the formula $C_7 H_5 O$, we have

7 Carbon ...	= 535·059	= 76·716 per cent.	12 × 6 = 75
5 Hydrogen	= 62·398	= 8·946	9 × 1
1 Oxygen...	= 100·000	= 14·338	2 × 8
	697·457	100·	

This formula is beautifully simple, but the hydrogen found by experiment is obviously too little to warrant us in adopting it. The true constitution, therefore, I believe to be $C_{21} H_{1}$ O_3, giving

21 Carbon ...	= 1605·177	= 77·171 per cent.
14 Hydrogen	= 174·7144	= 8·400
3 Oxygen...	= 300·000	= 14·429
	2079·8914	100·

which allows for the presence of a little moisture in the oxide of copper.

This constitution is corroborated by two experiments, in which the combustion was imperfect, but which gave the carbon and hydrogen respectively, in the proportions of

Atoms		
3 Carbon to	2·066	Hydrogen.
3 ———	2·143	———

As the nature of the retinic acid and the circumstances under which it occurs lead us to refer its origin to some tree belonging to the family of pines, we should expect to find some relation between its constitution and that of the colophonies, or pine resins of recent production.

According to Heinrich Rose, crystallized gum
Elemi consists of $C_{20} H_{16} O$, and
crystallized Colophony of $4 (C_{10} H_8) + 4O$,

C H O = 75.75, H 8.6, C 15.19

in which we observe an interesting approximation to the number of atoms of carbon in the retinic acid. In fact if gum elemi were so changed that an atom of carbonic acid should replace two of hydrogen, or colophony so that one of carbonic oxide should replace two of hydrogen, retinic acid would be formed, since

$$\left.\begin{array}{c} C_{20}\,H_{16}\,O_1+CO_2-H_2 \\ \text{or} \\ C_{20}\,H_{16}\,O_2+CO-H_2 \end{array}\right\}=C_{21}\,H_{14}\,O_3 \text{ or } 7\,(C_3\,H_2)+3O.$$

That the resinous matter formed upon or exuding from the trees deposited in the Bovey coal field was ever identical in constitution with recent pine resins, or that if so it has during its long burial undergone a change so simple as that indicated by the formula, it is impossible for us to determine; nevertheless researches of this kind are, I think, likely to throw an additional light on the nature and products of the vegetation of remote epochs, correcting or confirming the deductions of the fossil botanist, and it may be suggesting to him new inquiries.

Salts of Retinic Acid.

Retinate of Silver.—Alcoholic solutions of nitrate of silver and of retinic acid give a slight precipitate when mixed, which is determined however more fully by the addition of a small quantity of ammonia. It is of a brown colour, but speedily blackens by the action of the light. It is soluble to a considerable extent in alcohol, giving a dark brown solution. It is washed therefore with difficulty, and is in great part carried through the filter before the purity of the remainder can be depended upon. The filtered solution on standing gradually deposits a black precipitate containing more silver, due in all probability to a decomposition of the acid and reduction of the silver, or to the presence of some foreign reducing substance. The small quantity which falls on the admixture of the two alcoholic solutions, previous to the addition of ammonia, contains also an excess of silver, which may be due to a similar cause.

Heated to 300° Fahr., this and all the other retinates give out the peculiar resinous odour of the acid, and at a higher temperature the metallic salts melt, give off combustible products, and leave a bulky charcoal.

Three different portions, prepared by different processes and more or less perfectly washed, left on burning metallic silver equivalent to

41·780, 42·822, 43·585 per cent.

of oxide of silver respectively. According to theory we should have

$$\begin{array}{rcrcr} C_{21}H_{14}O_3 & = & 2079{\cdot}8914 & = & 58{\cdot}895 \\ AgO & = & 1451{\cdot}607 & = & 41{\cdot}105 \\ \hline & & 3531{\cdot}4984 & & 100{\cdot}000 \end{array}$$

This exhibits a considerable difference from the experimental result. The third portion analysed was precipitated by a solution of the acid in æther; it is quite possible, therefore, that the error may be owing to the presence of reduced silver. At all events the approximation is sufficiently close to show that the *equivalent* of the acid is represented by the same *multiple* of the elements as we have above deduced from direct analysis.

Retinate of Lead.—An alcoholic solution of acetate of lead gives with one of retinic acid a dark brown precipitate, which on drying is of a light umber colour. Heated in the air it behaves like the silver salt, and when burned leaves oxide of lead mixed with a greater or less portion of metallic lead. It is nearly insoluble in alcohol, and therefore may be fully washed; but partly owing to the impossibility of burning it without loss of lead by volatilization, or to some other cause which has escaped me, I have not been able with specimens of this salt prepared by different methods, to obtain nearer approximations to the theoretical per centage than those furnished by the analysis of the silver salt. It is unnecessary, therefore, to insert the numerical results.

Retinate of Lime is precipitated very sparingly and of a brown colour, when ammonia is added to the mixed alcoholic solutions of retinic acid and chloride of calcium. It is sparingly soluble in water, giving a pale yellow solution: when heated in the air it blackens, but does not melt, and at a red heat leaves carbonate of lime. Dried at 300° Fahr. 0·524 grs. left 0·096 carbonate = 18·32 per cent., or 10·312 of lime. This would indicate a sesqui-salt, composed of

			Per cent. calculated.		By experiment.
$1\frac{1}{2}(C_{21}H_{14}O_3)$	=	3119·837	= 89·758	=	89·788
1 CaO	=	356·019	= 10·242	=	10·312
		3475·856	100·		

On a single result however, and obtained from so small a quantity, no great reliance ought to be placed.

The retinates of baryta and strontia may be obtained by digesting the caustic earths in alcoholic or æthereal solutions of the acid. It is difficult, however, by this method fully to saturate the base, a coating of the resinous salt being apt to exclude a portion of it from the action of the acid.

The alkaline salts may also be formed by digesting the resin in a concentrated solution of the caustic alkali in which the salt formed is but sparingly soluble.

With the results of this examination of the salts of the retinic acid I am by no means satisfied, though they appear to leave little doubt that the true equivalent is $C_{21} H_{14} O_3$. The difficulty I have found in obtaining them of a constant composition, seems to demand so much more time for perfecting the investigation than the interest of the subject promises to compensate for, that I have been induced to leave it for a more inviting object of research.

Durham, April, 1838.

LXXXIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 291.*]

December 13, 1837.—A paper "On the Geology of the South-east of Devonshire;" by Robert Alfred Cloyne Austen, Esq., F.G.S., was read.

The district described in this memoir, is included within the rivers Exe and Dart, and extends from the coast to the granitic region of Dartmoor.

The formations of which it consists, are first noticed, then the faults, and, lastly, the probable amount of effects produced at each period of disturbance.

1. *Formations.*—These are considered under two heads:—1st. accumulations produced by actual causes; 2ndly, those produced by causes in operation before the most recent disturbances, including tertiary, secondary, transition, and igneous deposits.

The first of these subdivisions contains a description of the shingle, sand-hills, estuary deposits, and peat-bogs; but the south-east of Devonshire presents no phenomena connected with them, deserving of particular notice.

TERTIARY DEPOSITS.—To this class Mr. Austen assigns the (*a.*) raised marine deposits in estuaries, and (*b.*) raised beaches; (*c.*) the accumulations of water-worn rocks in valleys; (*d.*) the Bovey deposit; (*e.*) ossiferous caves; and (*f.*) the bed of angular chalk flints, and chert on Haldon and Blackdown.

(*a.*) *Raised Marine Estuary Deposits* are considered to exist in the valleys of the Exe and the Otter, because those rivers, in their present state, could not have accumulated the sediment which forms the surface of the valleys, or have worn the vertical cliffs by which they are partly bounded. In the valley of the Exe, above Topsham, is a bed abounding with marine shells of existing species, but high above the reach of any tide.

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* We now resume our notice of papers read before the Society, which has been interrupted by the Anniversary Proceedings, as detailed p. 433 *et seq.*

(*b.*) *The raised beaches* of Hope's Nose and the Thatcher were described by the author on a former occasion*; but in this paper he shows, that similar deposits occur to the west of Bovey-head, and at intervals along the whole southern coast of Devonshire. The upper limit of these beaches seldom exceeds 60 or 70 feet above the present sea-level. The raised beach to the west of Bovey-head, consists of shingle and indurated sand, associated, in the upper part, with red hæmatite, and it is overlaid by a thick mass of the same ore. This hæmatite is connected with the Upton iron lode.

(*c.*) *Accumulations of water-worn rocks.*—In every valley, with the exception of that of the estuary of the Teign, are thick heaps of debris, derived principally from the adjacent formations, and occasionally containing bones of the elephant and rhinoceros. Similar detritus caps all the ridges which lead up to the Haldons; also the summit of those hills, Blackdown, &c.; but the fragments are less water-worn on the tops of the ridges than in the valleys.

(*d.*) *The Bovey deposit* is not described in this paper, the author intending to prepare a separate account of it.

(*e.*) *Ossiferous caves.*—No information is given respecting the contents of the bone-caverns, Mr. Austen referring to the accounts already published respecting those at Chudleigh and Kent's Hole.

(*f.*) *The bed of angular flints*, containing in its lower part large tabular and angular blocks of chert and sandstone, and resting on the green sand of Haldon and Blackdown, is referred by the author to the tertiary series; and the angular form of the fragments strongly distinguishes the bed from the overlying superficial debris. The blocks of breccia, composed of angular flints, cemented by a very hard sandstone, and scattered over the surface of the hills and along the valleys, particularly near Sidmouth, are likewise considered as the remains of a tertiary deposit, probably of the same age as the grey wethers of Wiltshire. In the blocks near Sidmouth, Mr. Austen has observed remains of shells, which he is of opinion belong to the freshwater genus *Planorbis*, and in the Haldon beds numerous individuals of the genus *Cypræa*.

SECONDARY FORMATIONS.—These consist of, (*a.*) chalk; (*b.*) green sand; (*c.*) new red sandstone; and (*d.*) coal measures.

(*a.*) *Chalk.*—The prevailing divisions of this formation in the S.E. of England are stated to extend to Maynorst Cliff; but detached masses of lower chalk are found among the debris as far west as Peak Hill; and a white calcareous bed rests on the green sand of Style Hill.

(*b.*) *Green sand.*—This formation the author has traced along the slopes of the hills flanking the valley of Bovey, where it had not been noticed by previous observers. The beds dip towards and form the lining of the Bovey basin. They rest on new red sandstone, coal measures, limestone, slate, and perhaps granite, and, to a certain extent, are composed of the debris of these formations. A list of the fossils is given, and Mr. Austen states, that mollusca are almost wanting on Little Haldon, and he therefore infers, that the Haldon beds are of a more littoral nature than those of Blackdown. The conchifera also occur, in single valves, and broken, and appear to have been drifted as well as water-worn.

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* Proceedings, vol. ii. p. 102, [and Lond. and Edinb. Phil. Mag., vol. vi. p. 63.]

(*c.*) *New Red Sandstone.*—The subdivisions of this formation are stated to present the following geographical distribution, proceeding from east to west: 1. marls, with gypsum, as far as Sidmouth; 2. sandstone, from that town to a little beyond Dawlish; 3. shingle and conglomerate, to the western boundary of the formation, the pebbles being derived from the adjacent older rocks, and increasing in size towards the edge of the deposit. In some places, however, the shingle is associated with finely-grained fissile sandstone. From this distribution, the author infers, that the conglomerate marks the original shore of the sea in which the new red system was deposited; the sandstone, the finer detritus carried to a certain distance from it; and the marl, the mud diffused through the water, and conveyed to a still greater distance*. The jointed structure is not very distinct, but it may be traced even in the conglomerates; and from the best exhibited cases, the author concludes that the strata are divided into octohedral masses. Vegetable remains are found near Sidmouth.

(*d.*) *Culmiferous or Carboniferous Series.*—After alluding to the rectification, in 1836, by Prof. Sedgwick and Mr. Murchison, of the error in the geological position of this series†, Mr. Austen states that it occupies, in South Devon, the whole of the valley between Great Haldon and the extremity of Dartmoor.

He subdivides the series as follows:—

1st. Shales, which near the granite or trap, sometimes resemble the older slates.

2nd. Sandstones with beds of thick flagstone, as above Greyleigh and Biddlecomb, and below Lewell House.

3rd. Conglomerates, as at Ugbrook, the Orchard Well valley, and above Ryder Farm.

The limestone of Chudleigh is stated to rise through the culmiferous strata N. of Ugbrook-park, and to the S.W. of the Bovey deposit, to form a continuous band.

The mineral contents of the series are various. Tin and copper occur beneath Ashburton Down and near Christow; lead has been found in the same parishes; and iron ore is contained in large quantities in the shale.‡ Where the rock approaches the granite, it is much altered, and encloses numerous small garnets. Remains of plants are scarce, but impressions of Calamites have been found; and minute portions of vegetable matter occur in some of the beds of sandstone.

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* [On this subject compare Mr. Brayley's paper on the consolidation of the new red-sandstone formation, Phil. Mag. and Annals, N. S. vol. vi, p. 71: see also Lond. and Edinb. Phil. Mag. vol. vii. p. 515, note.—Edit.]

† [See Lond. and Edinb. Phil. Mag. vol. x. p. 388, and pres. vol. p. 510.]

‡ [See Mr. Kingston's paper, Phil. Mag. and Annals, vol. iii. p. 359.]

TRANSITION SYSTEM.—The culm measures rest unconformably on a series of deposits belonging to this system, and divided by the author into the following formations, in descending order:—

1. *Rag Limestone.*—A calcareous rock, coarsely laminated, of a dirty red colour, and abounding with stems of encrinites. Locality, Forest of Denbury.

2. *Shale.*

3. *Great Limestone.*—This is the limestone of Newton Bushel and Torquay. It is distinctly jointed, the prevailing strike of the joints being, for one set, N.N.W. to S.S.E., for the other, W.S.W. to E.N.E.; but considerable variations are stated to occur in different quarries.

Organic remains are very numerous, both corals and shells. At its base the deposit presents several alternations of shale and black limestone, and contains some peculiar fossil shells. It passes gradually into the next formation.

4. *Argillaceous Slates and Sandstones, generally Red.*—This deposit is of great thickness, forming the principal part of the slate-hills, and is sometimes worked for roofing-slate. It contains bands of limestone of a peculiar character. Organic remains occur only in the upper part, and agree apparently with those of the "great limestone."

5. *Lowest Band of Limestone.*—The limestone between Staper-hill and Bickington, and on the highway road by Goodstone to Ashburton and Buckfastleigh, is assigned to this portion; also that of Chudleigh, and the limestone at the base of Great Haldon is perhaps of the same age. The organic remains consist of corals and shells. Thin seams of carbonaceous matter also occur.

Igneous Rocks.—These formations consist of granite, porphyry, and trap. The granite of Dartmoor was shown in 1836, by Prof. Sedgwick and Mr. Murchison, to be more recent than the carboniferous strata; and Mr. Austen adopts the same view, as veins of granite penetrate the culm beds at Higher Alway and Lower Alway, near Bovey. The principal mass of Devonshire granite has in some places a height of 1800 feet, but over the whole of its area there is not the slightest appearance of any stratified deposit. The granite of Dartmoor is considered by Mr. Austen to be of different ages, as veins of coarsely grained are intruded among the common variety. Blocks of hornblendic granite are said also to occur, imbedded in the true granite; and in some places the granite is so felspathic as to resemble trachyte.

Trap Rocks—The author describes, with some detail, the hornblendic trap dyke of Wear, and shows that it must have been irrupted subsequently to the deposition of the chalk, because fissures in the limestone, traversed by the dyke, are filled with fragments of various formations, including chalk, and are charged with manganese, an effect produced by the intrusion of hornblendic trap throughout this part of Devonshire.

In the parish of Kington, veins of trap, on approaching the granite, are said to become more compact, and in the proximity of it, to be distinctly crystalline. Associated with the culm strata are bedded traps, apparently of contemporaneous origin; but the close of the culmiferous period is stated to have been marked by irruptions of the porphyry found at Pocombe-hill, and other places near Exeter. At Western, in the parish of Ide, it rests upon the culmiferous shale; but Mr. Austen says, it might be considered to rest on the new red sandstone, as that formation flanks the base of the hill. In the quarry, however, where the porphyry is worked, it has been cut through, and found to rest upon shale*. This rock has contributed largely to the formation of the conglomerates of the new red sandstone. Trap dykes are very common among the older slates, and have produced decided effects on them, and the general features of the country. Their age the author does not attempt to define; but from their being more abundant in the older than in the newer rocks, he conceives that they may have been, in part, irrupted before the latter were deposited. In the coast section, beds of hornblendic trap are included in the transition shale, to which they adhere by the lower surface, but not by the upper. Similar, imbedded, trappean rocks occur at Black Head, west of Babbacomb; the sixth mile-stone between Teignmouth and Torquay; near the village of North Whilborough, and at East Ogwell.

The author then describes the phænomena, which appear to have accompanied the disturbance of the strata at different periods, beginning with those considered to be most recent.

The undulations and deep combs in the new red sandstone, he says, are not due solely to denudations, but to elevations and depressions of the beds while the formation was beneath the sea. On the surface there are no indications of disturbance, the angular irregularities having been rounded before the district became dry land. The Watcomb Fault, however, he conceives, was produced by a subsequent operation, as it preserves its angular outline; and other instances are mentioned of unobliterated faults.

Mr. Austen next describes, with reference to this part of the subject, the raised, marine beds in the estuary of the Exe, the raised beaches at Hope's Nose, the Thatcher, and to the west of Berry Head; he mentions also those which occur at intervals along the southern coast of Devon.

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* [I observed, in 1825, in one of the Radden quarries at Thorverton near Exeter, that the porphyritic amygdaloid was overlaid by the red marl, the sandstone of that formation appearing to graduate into the subjacent amygdaloid, which was also intersected by irregular nearly horizontal veins of the former rock. The phænomena here are altogether such as might naturally be supposed to result from the intrusion of the igneous rock into the new-red-sandstone formation, at least prior to the consolidation of the latter, if not during its deposition. See the papers referred to in p. 566 note *.—E. W. B.]

Another system of disturbances, the author assigns to the tertiary era, because it appears to have been in operation, during the time, when the Haldon and Blackdown tertiary beds were formed. The Haldon strata exhibit the following proofs of disturbance:—

1. A partial destruction of the chalk, followed by the formation of a breccia of angular flints and sand.

2. The breaking up of this breccia and the production of a stratum, consisting of chalk flints, the breccia, quartz, granite and other rocks, all rounded.

Mr. Austen then offers some observations on the probable changes in the extent of dry land during the deposition of the secondary systems, indications of which, he conceives, are traceable in the characters, and the thinning out of the formations between the chalk and the new red series. In alluding to the faults which affect the new red sandstone, he says, that the greater part of them may belong to the tertiary epoch.

In the older formations, the evidences of disturbance during periods anterior to the new red sandstone, are referred chiefly to the unconformable position of the culmiferous strata with relation to the transition; and consequently the disturbances, which gave the slates their present position, must have taken place anterior to the deposition of the culmiferous strata.

With respect to the connexion between the age and the direction of the faults, the author says the district is too limited for any observations to be of much value. The older disturbances, however, appear to have a north and south direction. The most remarkable east and west fault is that of the valley of the Teign; and if the Weymouth Fault be prolonged westward, it would strike the coast of Devonshire at the mouth of that river.

Examples of depression as well as of elevation are mentioned in the paper, and it is said that the former are parallel to the latter, ranging S.S.W. and N.N.E.

Jan. 3, 1838.—A paper was read on the "Geological Relations of North Devon," by Thomas Weaver, Esq., F.G.S., F.R.S., &c.

The observations, which gave rise to this paper, were made during the autumn of 1837, in consequence of the discussions which had taken place relative to the position of the coal strata of the North of Devonshire*. The author states that he derived great assistance, during his investigations, from the Rev. David Williams, who kindly offered to be his guide. The survey, however, convinced Mr. Weaver, that Prof. Sedgwick and Mr. Murchison were perfectly correct in placing the coal with the associated strata at the top of the series, and in removing it from the transition systems to which other observers had assigned it.

The district, more particularly examined by the author, lies between the parallel of Bideford and Chilhampton on the south, and that of the Foreland (E. of Linton) on the north, and is bounded on the east by the meridian of High Down (four miles west of South Molton), and on the west by the Bristol Channel.

* [See p. 566, note *.]

Before he proceeds to detail his own observations, Mr. Weaver gives a comparative table of the subdivisions of the strata, exhibited by Prof. Sedgwick and Mr. Murchison at the meeting of the British Association at Bristol, in August 1836; and those employed by the Rev. David Williams, in a section shown at the meeting of the same body at Liverpool, in September 1837. These subdivisions, he states, are essentially the same, though Mr. Williams considers the coal strata as belonging to the transition systems.

The subdivisions, first established at the Bristol meeting, are adopted by Mr. Weaver, but he employs a nomenclature derived, for the greater part, from the localities where the strata are well exhibited. The following list contains the subdivisions in ascending order.

1. Foreland sandstone. 2. Linton calcareous slates. 3. Trentishoe quartzy slates and sandstone, including the Combe Martine limestone. 4. Morte slates. 5. Wollacomb sandstones, flagstones, and slates. 6. Trilobite slates. 7. Wavellite schistus and limestone. 8. Culmiferous shales (coal strata).

The mineral composition, lithological structure, local variations, and relative order of superposition of each formation are fully detailed; and the following inferences are given, as deducible from the whole of the evidence, collected during the survey.

That there is a general sequence of emergence from south to north, or from the culmiferous shales (8) to the Foreland sandstones (1), the dip being generally to the south.

That from the Foreland sandstones (1) on the north to the Trilobite slates (6) on the south inclusive, the angle of dip increases from 20° to 80°, being 20° to 30° in the Foreland sandstones (1) and Linton calcareous slates (2), 45° to 70° in the Trentishoe quartzy slates and sandstone (3), 70° to 80° in the Morte slates (4) and Wollacomb sandstones (5), and generally in the Trilobite slates (6), though in the last a lower angle is sometimes observable on approaching an undulation. The general strike of the beds is from 10° to 15° N. of W. and S. of E. from a true meridian.

That on the other hand the Wavellite schistus, limestone, and shale (7) and culmiferous beds (8) undulate on a very large scale, and are occasionally subject to contortions upon a smaller.

From the Foreland sandstones (1) to the Trilobite slates (6) inclusive, the series is connected throughout, passing from one to the other in such a manner as to form a consistent whole, the parts of which cannot be separated one from another without arbitrary divisions.

Though the beds, from 1 to 6 inclusive, form one consistent, consecutive series, yet the subordinate parts are subject to change in different portions of the field, both mineralogically and in extent, and occasionally thin out, as in the case of the beds of limestone.

On the other hand, the Wavellite schistus and limestone (7) and culmiferous shales (8), though apparently in some places in a parallel (conformable) position with the Trilobite slates (6), do, when thoroughly examined upon the line of outcrop in the district, form a break with number 6, and are unconformable thereto.

That this unconformity denotes two different æras of deposition, an inference supported by the difference in the organic remains; and Mr. Weaver further states, that he does not consider the occurrence of a few coal plants in the Wollacomb sandstones (5) as at all interfering with this inference.

That the preceding data justify the conclusion, that the strata from 1 to 6 belong to a system distinct from that which includes the beds 7 and 8, the former constituting a *peculiar transition group*; and the latter belonging to the true coal measures of England, the old red sandstone being alone wanting.

In conclusion the author dwells upon the importance of attending to mineral composition in surveying extensive systems of rocks; but he adds, that "the only safe guide in researches into the crust of the earth, is to keep constantly united in view, relative position, organic remains, and mineralogical characters; and not to restrict our attention to one of these distinctions when judging of geological formations."

January 17,—A paper by Dr. Bell, entitled "Geological Notes to accompany Major Todd's Sketch of part of Mazunderān," was first read.

These notes were made during a journey from Teheran (lat. 35° 40′ N., long. 50° 52′ E.), eastward to Feeroozkooh, then northward across the Elboorz mountains, and afterwards along the course of the river Talâr to the Caspian, and back to Teheran by the banks of the river Herâz. The author's observations are given in the order in which they were made during his journey, but the geological details may be classed as follows:—

1. *Alluvium.*—Teheran stands on a plain, consisting chiefly of the debris of limestone and trap rocks. In the bed of a river at the Caravanserai of Dalee Chaee, about 62 miles direct E. from Teheran, is a loose conglomerate composed of fragments of limestone and trap, imbedded in dried mud. A similar deposit forms low hills and valleys in several other places along the line of route, followed by the author, as at the river Gazan Chaee, and on the summits of the hills bordering the plain of Feeroozkooh. Below Pul-i-Seffeed, on the Talâr, is a conglomerate, formed of debris from the neighbouring mountains, united by a calcareous cement. Further down the river, it is finer, and stated to contain minute fragments of shells.

Below Sheergâh, the country, as far as the Caspian, is an alluvial, muddy flat. This sea is stated to be fast filling up, and the discoloured water of the streams, which flow into it, may be traced for five or six miles. Near the shore the water is so fresh that horses drink it; and Dr. Bell says, that the shells are chiefly freshwater. Half imbedded in the banks of mud and sand are innumerable trunks of large trees which have been drifted down by the rivers. A conglomerate similar to that at Dalee Chaee, was noticed at Karoo in ascending the Herzâ; also below the small stream Abi Noor, at the foot of Demavend Peak.

Lithographic Limestone.—A fine-grained limestone, used for lithography in Teheran, forms a high ridge north of the city, the beds dipping to the north, and resting on serpentine, porphyritic claystone, and porcelain stone. In connexion with a blue limestone, it extends over an immense tract to the N. and N.W. of Teheran, on the southern flank of the Elboorz Chain, where it generally rests upon shale and red sandstone, which is underlaid by a calcareous formation, called by the author mountain limestone. To the east of Teheran, the lithographic stone extends nearly to the village of Demavend.

Sandstone and Coal.—Between Teheran and Demavend, a sandstone of a coal formation is occasionally exposed; and in the bed of the river, Dalee Chaee, are upraised beds of "altered shale, like coked coal." On the north side of the Elboorz Mountains, shale and sandstone, assigned to the same coal deposit, are exposed resting upon the limestone. At Abbassabad is a sandstone, but it is not stated whether it belongs to this formation. A sandstone resting on limestone occurs in the ravine through which the Herâz flows above Amol; also on the summit of the limestone pass between Karoo and Bulkulum. About a mile below the latter village, a precipice 900 or 1000 feet high consists of perpendicular beds of coal and sandstone, but on the opposite bank of the river the sandstone strata are nearly horizontal.

Limestone.—Strata, considered by Dr. Bell as the representatives of the mountain limestone, constitute the hills to the S.E. of Teheran, and overhang the ruined city of Rai. To the westward of Demavend, the range of mountains, both north and south of the road as far as the Caravanserai of Dalee Chaee, consist of the same limestone resting on trap. Above the Caravanserai of Ameenabâd, it occurs resting also on trap; and the hills in the neighbourhood of the plain of Feroozkooh are similarly constituted. On the north side of the Elboorz chain—a coal-shale and sandstone rest on this limestone. Below Pul-i-Seffeed, the Talâr runs for a considerable distance through a gorge presenting perpendicular cliffs of beds of limestone and conglomerate. Limestone, resting on trap, also occurs in the ravine south of Amol. In ascending the river beyond this ravine towards Karoo, the following section is exposed. Trap—limestone—sandstone—shale—indurated slate clay—buhrstone—sandstone—limestone—trap. Between Karoo and Bulkulum is a narrow and deep fissure through a mountain of limestone, capped by the coal strata; and near the tomb of Em Zadeh Hashim, limestone again occurs resting on trap.

Organic Remains.—In the superficial conglomerate near the Dalee Chaee Caravanserai, Dr. Bell observed portions of two small Ammonites imbedded in fragments of compact limestone, but he did not notice the same rock in situ*.

Trap.—Greenstone, basalt, amygdaloid, porphyry, claystone, pitchstone, and serpentine, underlie the limestone at many places.

Travertine.—Springs charged with calcareous matter were often noticed, and it is stated, that the preservation of the remains of Shah Abas's causeway is owing to the calcareous drippings from the mountain side. Siliceous globules are formed by a hot spring in the little capital of Usk.

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* Near the summit of the ridge of the Elboorz just below the snow line, south of the district Toonikabān, he found a limestone containing bivalve shells; and near Bayazeed, in the neighbourhood of Mount Ararat, limestone inclosing corals and oysters, and resting on sandstone.

Thermal and Mineral springs occur near Usk.

In conclusion the author says, that the ravines through which the rivers Talâr and Herâz flow, are not due to denudation, but to rents; and that though the ravines are narrow, it would be difficult to point out a spot, where the strata on the opposite sides correspond. He noticed along the course of the Herâz, numerous effects of violent modern earthquakes*.

A paper was next read entitled "Notes on the Geology of the line of the proposed Birmingham and Gloucester Railway," by Mr. Frederick Burr†.

The author of this communication was employed on the survey for the railway, and the following is a general summary of his observations:—

For the first 26 miles, or from Gloucester to within three miles of Worcester, the road passes over the lower lias shale, and for the remainder of the distance over red marl and red sandstone. The lias tract is generally flat, seldom exceeding 100 feet above the level of the sea; but the red marl and sandstone rise considerably higher, and in that portion of the Lickey range intersected by the line of railway, the sandstone attains a height of about 600 feet above the same level. From the Lickey to Birmingham, the country forms an undulating table land, having a mean elevation of from 200 to 300 feet. The author gives numerous bore hole or shaft sections, made during the survey, and is thus enabled to show the nature of the formations in a district, otherwise concealed by its physical features or cultivated surface.

Lias.—The lias strata belong solely to the lower shales, and consist of bluish or blackish slaty clay, containing thin beds of argillaceous limestone. Near the junction with the red marl, there is generally a thick deposit of whitish or yellowish clay, with numerous beds of rubbly limestone, usually blackish. Beds of a light colour, resembling lithographic stone, are exposed in quarries near the Plough, half way between Gloucester and Cheltenham, and white lias is stated to occur at the junction with the red marl near Crawl, four miles N.E. of Worcester. The junction of the red marl is also exhibited in numerous small quarries in the same neighbourhood, but the strata consist of the above-mentioned whitish or yellowish clay, and dark limestone. These junction beds are also exposed at Dunhampstead, three miles S.E. of Droitwich.

Red Marl and Sandstone.—No difference was noticed in this formation from the characters already published. The marl is generally red and brown, but it is occasionally variegated or streaked white, and sometimes it contains a thin bed of red sandstone. The following section is given of the Droitwich brine pits. Red marl, with much water, 40 feet; marl with gypsum, but no water, from 100 to 130 feet in different pits: rock salt, not penetrated through at the depth of 170 feet. A pit at the chemical and salt works at Stoke Prior, four miles north of Droitwich, was sunk to the depth of 460 feet, first through red marl with much water, 111 feet; then red marl with gypsum, but no water, 195 feet; and afterwards marl interspersed with salt and interstratified with four beds of rock salt, 10 feet, 6½ feet, 39 feet, and 30 feet thick, respectively.

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* [We have here an additional instance of the connexion between earthquakes and lines of disturbance.—Edit.]

† [See the President's Address, in our last number, p. 513.]

About two miles beyond Stoke Prior, the red marl is replaced by red sandstone. At the junction, the former becomes slaty and contains thin beds of sandstone, and the latter consists of a pale brownish or yellowish argillaceous sandstone. At Finstat, black coaly impressions were noticed. About a mile north of Bromsgrove, the argillaceous sandstone is succeeded by a bright red micaceous sandstone. The rise of this rock to the surface was probably produced by the elevation of the neighbouring Lickey range, as at Stoke Prior, distant only two miles, the sandstone was not reached at the depth of 460 feet. In the ascent of the Lickey, the surface consists of coarse quartzose gravel, derived from the upper part of the range. The summit level of the railway is near Barnes Green Farm, and it is 384 feet above the sea. At this point a shaft was sunk through the following strata:—

	Feet	Inch.
Gravelly sand	6	0
Hard coarse gravel	1	6
Fine gravel	1	0
Hard coarse gravel	8	0
Indurated red marl	2	0
Hard coarse gravel	11	0
Red sandstone	1	0
Hard conglomerate	20	6
	51	0*

Of these beds of gravel, the author considers only the uppermost as superficial gravel, and the remainder as belonging to the new red. To the north of the ridge, the bright red sandstone re-appears, dipping considerably to the east, and alternating with marl and impure siliceous limestone. About a mile from the Lickey, the red marl again constitutes the surface, and extends to Birmingham.

Superficial Detritus.—The lias in the vale of Gloucester is covered by 8 or 10 feet of brownish, greyish, or mottled clay and loam, overlaid by thick deposits of sand and gravel, derived, in the neighbourhood of Gloucester, Cheltenham, and some other places, from the adjacent oolitic hills; but near Bredon and to the north of that village, the detritus consists of siliceous sand and pebbles.

* In deep cuttings of the Worcester and Birmingham Canal, 1½ mile south-east of this point, Mr. Burr noticed an anticlinal axis, denudated in the centre. The strata dipped south and north at considerable angles, and consisted of red marl overlying red and white sandstone.

Mineral Springs.—From information obtained during the survey, the author states, that at Walton, one mile east of Tewkesbury, a spring similar in properties to the Cheltenham waters, was found at the depth of 90 feet: that in the neighbourhood of Northway, weak brine springs have been discovered in the lias clay at the depth of 40 feet; and likewise on Defford Common. All these springs, Mr. Burr remarks, range N. and S. and in a line with the brine springs of Droitwich and Stoke Prior. Near Stoulton, five miles from Worcester, is a small, brackish marsh.

In conclusion, the author expresses his hopes, that surveyors, employed on similar investigations, will be induced to lay the results of their field work before the Society; and he acknowledges his great obligation to Capt. Morsoom, the superintending engineer of the line, for being permitted to make free use of all the geological information, which he obtained during the discharge of his duties.

A paper on "the Coast Section from White Cliff Lodge, one mile south of Ramsgate, to the Cliff's End, near the Station Brig in Pegwell Bay, Kent," by Mr. John Morris, was afterwards read.*

The cliffs consist of the upper chalk for about ¾ of a mile, and of "the lower or sandy beds of the London clay" for the remainder of the distance. A capping of superficial detritus, of rubbly chalk, chalk flints, and loam, extends the whole way.

The principal object of the communication is to describe a series of dislocations in the chalk, marked by shifts in a layer of tabular flints.

1. At 52 paces from the commencement of the section is a slight indication of a fault.
2. 30 paces further the layer of flints is depressed at a fissure 4 feet.
3. 31 ditto ditto ditto 4
4. 53 ditto ditto ditto 2
5. 33 ditto ditto ditto 3
6. 43 ditto ditto ditto 2
7. 30 ditto ditto ditto 4
8. 11 ditto ditto ditto slightly.
9. 12 ditto ditto ditto 1
10. About 45 paces from the last depression, the vein of flint is brought within 8 feet of the beach, and it is in one place affected by a fault of 1½ feet.

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Close to fissures 1 and 2, are indications of parts of the bed of flint not having been equally disturbed, as they preserve the same horizontal range, while the portions on each side are depressed: a similar example of irregular movement occurs at number 5. Beyond the fault 10, the cliff recedes, forming a small cove, produced, the author believes, by the action of the sea on a considerable disturbance in the strata, the minor faults being always accompanied at the foot of the cliff by a natural excavation or cave.

Where the layer of flint re-appears, it is curved, and is afterwards traversed by three faults producing unequal depression, one of which is coincident with a vein of tabular flint; beyond the Preventive Station is another depression of 5 feet, the upper part of the fissure being filled with chalk rubble, sand and flint, and the bottom hollowed into a cave 10 feet high, and from 15 to 20 feet deep. At 10 yards from this point the flint layer dips beneath the beach. The chalk cliff continues for about 800 paces further, gradually decreasing in altitude, but capped by sandy loam and chalk rubble. Beyond this point, the tertiary strata commence, consisting, in the upper part, of 7 to 18 feet of sand and sandy clay, with occasional masses of sandstone and layers of shells; and in the lower part of 7 feet of bluish clay, which also incloses shells. These strata are visible for about 570 yards, and then dip beneath the marshes. Mr. Morris considers them the equivalents of the beds between Reculver and Herne Bay, agreeing in position, mineral characters, and organic remains. They are overlaid by the same superficial detritus as the chalk.

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* [See the President's Address, in our last number, p. 513.]

The destruction of the cliffs is calculated to have been, until means were taken to defend them, at the rate of 3 feet annually. At the cove before-mentioned, the sea removed in 25 years, about 20 yards, including two cottages and a garden.

The wells at the Preventive Station and Pegwell, are sunk about 30 feet, through loam and chalk. The water is 10 feet deep, but it is sometimes brackish, being affected by the rise and fall of the tide. It is generally lowest after the tide has flowed one hour, and remains in that state about two hours, after which it rises. Whether this effect is connected with the faults in the cliffs, Mr. Morris doubts; but he states, that a freshwater spring issues from the beach at low water, opposite the well at the Preventive Station.

Jan. 31.—An extract was first read from a letter addressed by Sir John Herschel to Mr. Lyell, and dated Feldhausen, June 12, 1837.

In former letters addressed to Mr. Lyell and Mr. Murchison, dated Feldhausen, Feb. 20, and Nov. 15, 1836, and read before the Society May 17, 1837*, Sir John Herschel first proposed his theory relative to the increment of temperature from below, which would be produced in certain portions of the earth's crust by the partial distribution of additional sedimentary matter over the bottom of the ocean; and of the effects which would naturally result from this operation, producing the phænomena of earthquakes, and elevation and depression of strata. In this letter he states, he was not then aware that Mr. Babbage "had speculated on that peculiar mutual re-action of the surface and the interior of the globe, and which must be called the secular variation of the isothermal surfaces of the latter;" nor was he aware of the notice in the Proceedings† of Mr. Babbage's paper on the Temple of Serapis, until his attention was recently called to it by Mr. Lyell and Mr. Murchison, but at the end of which notice a theory identical in the leading point is given.

With respect to the first development of the theory, Sir John Herschel says, "the employment of the pyrometric expansion of rocks as a motive power was, I feel confident, suggested by some one (the name of Mitscherlich or Laplace is somehow connected in my memory with it) many years ago, certainly before 1833. As regards the course of my own ideas, it was simply this. When I first read your book I was struck with your views of the metamorphic rocks, and I began to speculate how and why the mere fact of deep burial might tend to raise the temperature to the required point. Three modes occurred: 1st. development of heat by condensation; but this cause seemed somewhat feeble, and not very clear in its mode of action, since at every moment an equilibrium of pressure and resistance is established: 2nd. plunging down into an ignited pasty mass; here, however, considering the excessive slowness of the process, it occurred to me that there would be plenty of time for the ignited matter below, not merely to divide its caloric with the newly superposed mass, but to take up fresh from below, and thus to establish a regular gradation of temperature from below upwards; and this led to the 3rd and more general view of the matter, which is that of the variation of the isothermal surfaces, as stated in my former letters."

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* Proceedings of Geol. Soc. vol. ii. p. 548. [Or Lond. and Edinb. Phil. Mag. vol. xi. p. 212, 214.]

† Proceedings, vol. ii. p. 72. [Or Lond. and Edinb. Phil Mag. vol. v. p. 213.]

It was, however, the perusal of Mr. Lyell's 4th Edition which led to the final development of the theory.

Sir John Herschel then observes: "When people think independently at different times, and excited by different original subjects of consideration, bearing on one more general object, if their ideas converge towards one view of the matter, it is a proof, that there is something worthy of further inquiry; and if they think to any purpose, it is hardly possible but that many points will occur to the one which do not occur to the other; and that so a theory may branch out and acquire a body much sooner than it would do by the speculation of one alone; and indeed such is, in some degree, the case in the present instance. Babbage, for example, has speculated not only on the heaping on of matter in some parts, but on its abstraction in others as a cause of variation in the isothermal surface, and justly. It is the case of the algebraic passage from + to − passing through 0. In *envisaging* (as the French call it) the question algebraically, the cases could not be separated. Again, he has confined himself to the pyrometric changes in the solid strata, while I have left these out of view, and relied on what I think to be a far more energetic and widely acting cause—the variation of pressure, and the infinity of supports broken by weight, or softened by heat, to produce tilts. Both causes, however, doubtless act, and both must be considered in further detail. The former alone may account for the phænomena of the Bay of Naples; the latter must I think be called in to account for those of Scandinavia and Greenland, and of the Andes.

"I would observe that a central heat may or may not exist for our purposes. It seems to be a demonstrated fact that temperature does, in all parts of the earth's surface yet examined, increase in going down towards the centre, in what I almost feel disposed to call a frightfully rapid progression; and though that rapidity may cease, and the progression even take a contrary direction long before we reach the centre (as it might do, had the earth, originally cold, been as Poisson supposes, kept for a few billions or trillions of years in a firmament full of burning suns, besetting every outlet of heat, and then launched into our cooler milky-way); still as all we want is no more than a heat sufficient to melt silex, &c., I do not think we need trouble ourselves with any inquiries of the sort, but take it for granted that a very moderate plunge downwards in proportion to the earth's radius, will do all we want*."

A paper was next read, entitled "Description of the Insulated masses of Silver found in the mines of Huantaxaya, in the province of Tarapaca, Peru;" by Mr. Bollaert, and communicated by Mr. Darwin, F.G.S.

The mines of Huantaxaya are three leagues from the Port of Iquiqui (lat. 21° 13′ S. long. 70° W.), and in a mountain-hollow 2800 feet above the level of the sea. This depression is bounded towards the west by a hill called Huantaxaya, 3000 feet above the sea level, or 200 feet above the hollow, and on the opposite side by a hill of similar height. The great mass of the mountain consists of a reddish, argillaceous limestone, but the escarpment, towards Iquiqui is covered with loose sand, and near the base, porphyry and granite are visible. The limestone is traversed by numerous argentiferous and other veins, which range from N.E. by E., to S.W. by W., but the mines of Huantaxaya are in a superficial detritus called Panizo.

This deposit is from eighty to one hundred yards thick, and is composed of fragments of limestone not water-worn, and dried mud apparently derived from the same rock. It is divided into beds, some of which, called Sinta, are metalliferous, and others, called Bruto, are barren. The nodules of ore, to which the name of papa has been applied, from their resembling a potatoe in form, consist of pure silver, chloride, and other chemical compounds of silver, sulphurets of copper and lead, and carbonates of copper. The papas are of all sizes, and some have produced 160 ounces of pure silver in a hundred pounds. One celebrated papa weighed about 900 pounds, and resembled in shape the top of a table. The miners believe, that each layer of Sinta has been derived from a particular vein in the limestone, and that they can determine to which vein a papa originally belonged.

The only instruments used in working the Panizo, are an iron bar six inches long and a small iron mallet. With these tools, the Panizero rapidly advances in the soft materials, but rarely makes a larger excavation than is sufficient for his body to pass on hands and knees. In clearing out the contents of these honey-combed galleries, a hide-bag is strapped over the shoulders and under the arms; but in crawling through the narrower parts, the miner transfers the bag to one of his feet and drags it after him.

The danger of working these unconsolidated beds is greatly enhanced by frequent shocks of earthquakes.

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* [See the President's Address, and also the Proceedings of the Royal Institution, p. 519 and 533 of our last number.]

The following section of the principal shaft will illustrate the nature of the Panizo deposit.

1. Caliche. This bed contains near the surface a large quantity of common salt, and occasionally a few small papas are found in it .... 28 yards.
2. *Sinta Cenisada*, ash-coloured, with a few papas .... ¼
3. Caliche, or Bruto........ 12
4. *Sinta*, Tisa chiquita, a bed consisting of 96·4 white sand, 3·6 sulphuric salts and water; also a trace of muriatic salts. A few papas........ ¼

	yards.		yards.
5. Bruto	4	22. *Sinta cascajosa*, gravelly layer	¼
6. *Sinta cascajosa*	¼	23. *Tisa grande*, similar to 4.	6
7. *Sinta Tiquillosa*	½	24. Bruto	½
8. *Sinta challosa*	¼	25. *Sinta cascajosa*, gravelly layer	¼
9. Bruto manto, many fossil shells	¼	26. Bruto	½
10. Bruto conchado, shelly layer*	½	27. *Sinta chadosa*	¼
11. Tisi chiquita, resembling number 4	¼	28. Bruto	¼
12. *Sinta Tiquillosa*	¼	29. *Sinta barrosa*, clayey layer	½
13. Bruto	4	30. *Tisa*, similar to 4	¼
14. *Sinta Tiquillosa*	¼	31. Bruto	6
15. Bruto	4	32. *Sinta cascajosa*, gravelly layer	¼
16. *Sinta challosa*	¼	33. Bruto	½
17. *Sinta cascajosa*, gravelly layer	¼	34. *Sinta chadosa*	½
18. Bruto conchado, shelly*	¼	35. Bruto	3
19. *Sinta conchado*, shelly*	2	36. *Sinta chadosa*	¼
20. *Sinta challosa*	¼	37. Bruto	1
21. *Sinta conchado*, shelly,* few papas	¼	38. *Sinta barrosa*, clayey layer	½

The layer 38 rests upon the limestone rocks.

A paper was afterwards read "On the peat bogs and submarine forests of Bourne Mouth, Hampshire, and in the neighbourhood of Poole, Dorsetshire;" by the Rev. W. B. Clarke, F.G.S.†

The entrance of Bourne Mouth Valley is one of the many chines which intersect the tertiary strata between Poole Harbour and Christ Church Head, and the valley extends from the sea three and a half miles in a N.W. direction. About half way, a fork diverges to the west, and this branch with the lower portion of the main valley is called Bourne Bottom, and the eastern branch of the fork, Knighton Bottom. In each valley is a small current, and their united waters form the brook at Bourne Mouth. At the head of Knighton Bottom is a peat bog, which contains trunks of oak, alder, birch, and beech trees, also hazel sticks and nuts, and fragments of bark. The trunks of the trees lie in the direction of the valley, but the stools are firmly fixed upright in the peat. The wood when extracted is soft, but it becomes firm on exposure to the weather, and it is used for purposes of husbandry. The bark, especially that of the beech, retains its character unaltered. The surrounding district is now sterile, and no oaks of equal size exist within many miles of Knighton Bottom, the neighbouring plantations being of very recent origin. Traces of fire and of the axe are said to have been noticed in the bog-wood. Ten feet of peat have been excavated, but the depth of the deposit is not known. The peasantry have a tradition, that the forest was burned down during the reign of Stephen, though Mr. Clarke conceives that its destruction was effected during the occupation of England by the Romans. At the head of Bourne Bottom there is also a peat bog, but it incloses only fir trees. The submarine peat and forest off the entrance of Bourne Mouth* contains fir, birch, and alder trees; Mr. Clarke however considers that the two latter have been transported from the bog at the top of Knighton Bottom. Some of the trees, as noticed by Mr. Lyell, are pyritous, but the author of the paper is of opinion, that they have been derived from the neighbouring cliffs of plastic sand, having observed in them, during the summer of 1837, a pyritous trunk. The present position of the forest, Mr. Clarke thinks is due to a subsidence and undermining of the strata, which supported it at a higher level.

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* In these layers, fossil shells, derived from the limestone, are found.

† [See the President's Address, in our last number, p. 513.]

Other peat bogs are described on the north of Poole harbour, as between Sterte and Stanley green, at Hatch Pond, Creekmoor and Lytchett. At the first of these localities, in making an excavation to erect a dyke, the workmen found beneath the alluvial soil, gravel, then peat, and afterwards oaks and alders which rested upon mottled clay. The sea, at all states of the tides, overflowed this inlet previously to the erection of the dyke; and the position of the forest Mr. Clarke assigns to an undermining of the strata on which it rested.

At *Hatch Pond*, about two miles north of Poole, in the direction of Winbourn, is an extensive depression through which a brook of some volume flows, and has produced an immense accumulation of peat. This bog communicates with Poole Harbour by a succession of marshy grounds; the whole of which Mr. Clarke conceives were once covered by the sea, as they present phenomena similar to those exhibited at Tottenham, with the exception that no trees have been observed. A branch of a Roman road meets the present highway just upon the edge of the depressed area, and the author infers that that point was, in the time of the Romans, the water head of the bay, though it is now three or four furlongs nearer Poole.

Creekmoor. Another tract of low marshy ground, with a peat-bog containing fir trees, occurs at Creekmoor bridge on the north side of Holes Bay. In draining it, the workmen, about four feet from the surface, tapped a spring which flows with great violence and throws up white sand.

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* An account of this submarine fir-wood was first given by Mr. Lyell in the 4th edition of the Principles of Geology (1835), from information communicated by Mr. Charles Harris. The present submerged position was explained on the belief, that as the sea is encroaching on the shore, the Bourne Valley may once have extended further; and that its extremity consisted as at present of boggy ground, partly clothed with fir-trees; that the sea laid bare at low tide the sandy foundations, which being undermined by streams of freshwater, several of which burst forth in different parts of the existing beach, the matted superstratum of vegetable matter sank down below the level of the sea.—Vol. iii. p. 276.

Lytchett. At various places in this parish, peat bogs and buried trees occur, particularly at Bulbury Bay. They are, however, considerably above the level of the sea; but on the north east side of Lytchett Bay, at the extremity of the canal from the clay works, is a subsided peat bog thirty feet thick, containing trees. It rests upon mottled clay, and is overlaid by nine or ten feet of clay and sand which are constantly covered by two feet of water.

In the pits where the subjacent mottled clay is excavated, springs of great volume burst forth whenever the main body of water is tapped, and the author is of opinion that this subterranean stream may have caused the subsidence of some of the peat bogs, in consequence of its undermining action.

In alluding to the accumulations of mud in Poole Harbour, the author states, that in digging a well in West Street in the town of Poole, a mass of sea-weed was found, with remains of an ancient embankment at the depth of six feet, and a furlong from the present high water mark.

Feb. 21.*—A paper "On part of Asia Minor," by William John Hamilton, Esq., Sec. G.S., was read.

In this paper, the author gives an account of the geological structure of the country from the foot of Hassán Dagh, a few miles S.S.E. of Akserai (lat. 38° 20′ N., long. about 34° E.), to the great salt lake of Toozla or Kodj-hissar, and thence eastwards to Cæsarea and Mount Argæus.

The formations, noticed by Mr. Hamilton, are trachytic conglomerates, considered by him one of the oldest formations of the country; a system of highly inclined beds of red sandstone, conglomerates and marls, which rest upon the trachytic conglomerate, and are apparently connected with the saliferous deposits of the country, though the author did not observe any beds of salt in the sandstone†; a limestone belonging to the vast, calcareous, lacustrine formation of the central part of Asia Minor; a great system of volcanic tuffs, trachytes and basalts, apparently of comparative modern origin; and a grey granite which is newer than the sandstone, as it penetrates and disturbs that formation near Kodj-hissar; but pebbles of a grey granite identical in composition also occur in the conglomerate.

Hassán Dagh, upwards of 8000 feet above the sea, consists entirely of trachyte, and trachytic and porphyritic conglomerates, and rises from the eastern termination of a great calcareous plain. Several volcanic cones, composed of trachytic conglomerates and scoriæ, occur near the base on the S.S.W. and N.W. sides. All the latter, with the exception of one, are in the present valley, and below the tufaceous beds which cap the hills on its north side, and were, therefore, produced subsequently to the excavation of the valley. From one of them a considerable stream of black, vesicular lava proceeds, and encircles some of the smaller cones.

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* [The Anniversary Proceedings of Feb. 16, will be found in our two preceding numbers, p. 433, 508, *et seq.*]

† The extensive beds of rock salt on the borders of Pontus and Galatia, occur in troughs or small basins resting upon the perpendicular edges of a red and brown sandstone conglomerate.

From the foot of Hassán Dagh to the great salt lake of Kodj-hissar, the road traverses a plain, bounded on the south by low hills of the lacustrine limestone; and on the north by hills having narrow peaks and steep escarpments, of red and yellow sandstone, sometimes associated with calcareous conglomerates, sand and marl, and capped towards the east and north-east by beds of tuff and a white pumiceous rock, which passes into trachyte. Still further east, is a hill in which the sandstone rests upon a trachytic conglomerate.

The phenomena presented in this district, the author conceives, indicate the following operations:—

1. The irruption of the trachyte, from which the trachytic conglomerate was formed.
2. The deposition of the sandstones, conglomerates and marls.
3. The ejection of the igneous matter constituting the overlying beds of volcanic tuff and pumiceous rock.
4. The excavation of the valley.
5. The formation of the volcanic cones at the foot of Hassán Dagh.

The water of the salt lake of Kodj-hissar is so highly charged with saline matter, that no fish can live in it; and if the wings of a bird touch it, they become instantly stiff and useless with incrustation. Mr. Hamilton could not ascertain the exact dimensions of the lake, but he was informed, that it is about thirty hours or leagues in circumference. The bottom is a soft mud, incapable of supporting the slightest weight; but at the part examined by the author, a thick, solid crust of salt, which bore the weight of a horse, rested upon the soft mud, and was covered by about six inches of water, which he was informed would be dried up in another month.

The sandstone formation extends beyond the village of Kodj-hissar, towards the N.N.W., dipping in the same direction. It is penetrated near the town by a mass of finely-grained, grey granite, which also sends veins into the sandstone, and produces an anticlinal inclination, the dip towards the south being 80°. In the sandstone conglomerate of the neighbourhood, Mr. Hamilton, however, noticed pebbles of a grey granite similar in composition to that of the protruded mass. About a mile N.W. of Kodj-hissar are detached portions of the horizontal white limestone, either resting unconformably against the sandstone, or filling up irregularities in its surface. In some places it caps the hills, which flank the valley a little to the north of the village.

The only fossils noticed in the sandstone, were impressions resembling fucoids, and similar to those found in the Alpine limestone near Trieste.

The author then describes the structure of the country between Kodj-hissar and Cæsarea, a distance of about 108 miles. It consists of the same sandstone system containing gypsum, and occasionally overlaid by horizontal beds of the lacustrine limestone and volcanic tuff; but the latter constitutes likewise large districts, the fundamental rock of which is not visible. Granite forms a range of hills thirty miles in extent, about midway between Kodj-hissar and Sari-karaman, and is traversed in one place by a N.N.E. and S.S.W. dyke of claystone porphyry: granite occurs also between the latter town and Tatlar. Trap and trachyte were noticed at several places, likewise serpentine and greenstone near Sari-karaman; and basaltic rocks form table lands overlying the volcanic tuff near Tatlar and Baktash; and close to Nembscheher beds of basalt alternate with the volcanic tuff. To the east and north-east of Tatlar the author remarked several volcanic hills, from which streams of basalt or lava appear to have flowed. To the south-east of the village he also saw a stream of a more recent date than that which caps the neighbouring hills; for it not only flows at a lower level, but below the steep escarpments of the older basalt. In the ravine near Tatlar, and in the vallies of Utch-hissar and Urjub, the tuff has been worn into cones from 150 to 300 feet high. They are principally detached from the sides of the vallies, but are connected at the base; and are in some places so numerous and close together, that they resemble at a distance a grove of lofty cypresses. Where the cones occur on the sides of the vallies, they exhibit every stage of development, from the first indication of a mound near the summit of the slope, to the full-formed cone at the bottom. In the valley of Urjub some of them are capped by a mass of hard rock, which projects like the head of a mushroom. The production of these cones the author ascribes to the action of running or atmospheric waters.

One of the principal objects of Mr. Hamilton's visit to this part of Asia Minor, was to ascend to the summit of Mount Argæus, which had not previously been reached by any traveller.

This mountain rises abruptly from the alluvial plain of Cæsarea, sending out prolongations and spurs into the plain which stretches to the north, between Injesu and Cæsarea; but it is connected at its eastern base with other ranges of mountains. It rises, like Hassán Dagh, to a single peak, and it resembles in outline, the summit of Ararat. The highest part consists of a reddish brecciated and scoriaceous conglomerate, full of fragments of trap and porphyritic trachyte, and may be said to be the point of junction of two enormous, broken craters, one of which opens to the N.E., the other to the N.W., the steep sides of which are covered to the north with eternal snow for 2000 or 3000 feet below the summit. The height of the mountain was ascertained by Mr. Hamilton to be about 13,000 feet, the following being the results of his observations.

By barometer	13,293
By angle of elevation from the Greek Convent	13,242
By angle of elevation from Kara-hissar	12,809

A little below the summit, on the S.E. side, rugged, serrated ridges rise through the snow, some of them consisting of a compact trachytic rock, with a highly conchoidal fracture, resembling that of hornstone; but others are composed of porphyritic trachytes of various colours and textures. Near the foot of the great cone, on the S.E., W., and N. sides, rise numerous smaller ones of pumice and lapilli, from some of which on the N.W. side, streams of basalt or lava may be traced.

In conclusion, the author expresses his regrets, that the want of organic remains prevents him from determining the comparative antiquity of the formations, with respect to those in Europe. In only one instance, the fucoid impressions near Kodj-hissar, did he observe a trace of an organic body in the sandstone; and the only occurrence of fossils in the limestone series which he noticed, was in the neighbourhood of Sevri-hissar W.S.W. of Angora, where he discovered, in the upper beds of the formation, Limnea and Planorbis.

March 7.—A notice was first read, on some remarkable dikes of calcareous grit at Ethie, in Ross-shire, by Hugh Edwin Strickland, Esq., F.G.S.

These dikes, which traverse the lias schist, are displayed only at low water. Two of them are parallel to the strata of schist; but another, which sends off branches in various directions, is in no part of its course parallel to those strata. Their thickness varies from one to three feet; but that of some of the lateral branches does not exceed three inches. They exhibit no variation in texture or composition, and show no signs of lamination, but are frequently fractured transversely to their direction. The transition from the dike to the lias shale is immediate; no change being apparent in the latter at the point of junction. The shale, from its greater softness, has been removed between the dikes, leaving them like walls from one to three feet in height.

These dikes, and similar ones in other places, were noticed by Mr. Murchison, in his examination of the coast of Scotland, in 1826.

By what means the dikes were produced, the author does not venture to inquire; his only object being to draw the attention of geologists still further to them.

A paper, on the connexion of certain volcanic phænomena, and on the formation of mountain-chains and volcanos, as the effects of continental elevations, by Charles Darwin, Esq., Sec. G. S., was then read.

The author first gave a detailed account of the volcanic phænomena, which accompanied the earthquake that destroyed Concepcion on the morning of the 20th of February, 1835; and then deduced from volcanic phænomena, certain inferences with respect to the formation of mountain-chains, and continental elevations.

In describing the phænomena of the earthquake of 1835, Mr. Darwin quotes the published accounts by Captain Fitzroy* and Mr.

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* Journal of the Royal Geographical Society, vol. vi., p. 319, 1836.

Caldcleugh*; likewise communications received by him from Mr. Douglas, a resident on the island of Chiloe.

A few days after the earthquake, several volcanos within the Cordilleras, to the north of Concepcion, though previously quiescent, were in great activity. It is doubtful, however, if the volcano of Antujo, in nearly the latitude of Concepcion, was affected, while the island of Juan Fernandez, 360 miles to the north-east of the city, was apparently more violently shaken than the opposite shore of the main land. Near Bacalao Head, a submarine volcano burst forth in sixty-nine fathoms water, and continued in action during the day as well as part of the following night. That island was also affected in a remarkable manner, by the earthquake which overthrew Concepcion in 1751.

In Concepcion, the undulations of the surface appeared, to the inhabitants, to proceed from the south-west; and this direction was likewise inferred, from the effects observed in the buildings; for those walls, which had their extremities towards the point of disturbance, remained erect, though much fractured; whilst those (and the streets cross each other at right angles) which extended parallel to the line of the vibration, were hurled to the ground. This was strikingly exemplified in the cathedral, where the great buttresses of solid brick-work were cut off, as if by a chisel, and thrown down; while the wall, for the support of which they had been built, though much shattered, remained standing.

In Chiloe, south of Concepcion, the shocks were very severe, but they entirely ceased in about eight minutes. The motion, as described by Mr. Douglas, was horizontal, and similar to that of a ship going before a high, regular swell; from three to five shocks being felt in a minute; and the direction being from N.E. to S.W. Forest-trees nearly touched the soil in these directions; and a pocket compass placed level on the ground vibrated, during the violent shocks, two points to westward, but only half a point to eastward; and during the minor shocks the needle pointed north. At Calbuco, a village on the mainland opposite the northern extremity of Chiloe, as well as at Valdivia, between Chiloe and Concepcion, the earthquake was much less severely felt; and near Mellipulli, in the Cordilleras (not far from Calbuco), not at all. The volcano of Villareca, near Valdivia, which is said to be more frequently in irruption than almost any other in the chain, was not the least affected; though the volcanos of central Chili are stated by Mr. Caldcleugh to have been seen, some days afterwards, in great activity. Several of the culminating points of the Cordillera in front of the island of Chiloe, exhibited increased energy during the earthquake, and immediately after it. During the shocks, Osorno, which had been in activity for at least forty-eight hours previously, threw up a thick column of dark blue smoke; and directly it had passed away, a large crater was seen forming in the S.S.E. side of the mountain; Minchinmadiva also, which had been in its usual state of moderate activity, commenced a fresh period of violence. At the time of the principal shock, the Corcovado was quiet; but when the summit of the mountain was visible a week afterwards, the snow had disappeared from the north-west crater. On Yntales, to the south of the Corcovado, three black patches, resembling craters, were observed above the snow-line after the earthquake, though they had not been noticed previously to it. During the remainder of the year, the whole of the volcanic chain, from Osorno to Yntales, a range of 150 miles, exhibited, at times, unusual activity. On the night of the 11th of November, Osorno and Corcovado threw up stones to a great height; and on the same day, Talcahuano, the port of Concepcion, 400 miles distant, was shaken by a very severe earthquake; and on the 5th of December the whole summit of Osorno fell in.

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* Phil. Trans., 1836; Part I. p. 21. [An abstract of Mr. Caldcleugh's paper appeared in Lond. and Edinb. Phil. Mag. vol. viii. p. 148.]

After these details of more particular phænomena, Mr. Darwin alluded to the great areas over which earthquakes have been simultaneously felt; but he added, it is impossible even to guess through how wide an extent, in the subterranean regions, actual changes may have taken place. In order to enable the reader, who may be more familiar with European than South American geography, to comprehend the vast surface which was affected by the earthquake of February 1835, he stated, that it had a north and south range, equal in extent to the distance between the North Sea and the Mediterranean: that we must imagine the eastern coast of England to be permanently raised; and a train of volcanos to become active in the southern extremity of Norway; also that a submarine volcano burst forth near the northern extremity of Ireland; and that the long dormant volcanos of the Cantal and Auvergne, each sent up a column of smoke.

The contemplation of volcanic phænomena in South America, has induced the author to infer, that the crust of the globe in Chili rests on a lake of molten stone, undergoing some slow but great change; for if this inference be denied, he says, the only alternative is, that channels from the various points of eruption must unite in some very deeply-seated focus. This conclusion, however, he doubts, on account of the union of the different trains of volcanos on the one line of the Cordillera, and more especially as many hundred square miles of surface in Chili have been elevated during the same earthquake. Moreover, these elevations have acted within a period geologically recent, throughout the whole, or at least the greater part, of Chili and Peru, and have upraised the land several hundred feet. He is further of opinion, that the shocks coming from a given point of the compass, and the overthrow of the walls, according to their position with respect to this point, prove that the vibrations do not travel from a profound depth, but are due to the rending of the strata not far below the surface of the earth.

In a geological point of view, the author conceives, the three classes of phænomena exhibited during this earthquake of February 1835, viz. a submarine outburst—renewed volcanic activity, simultaneously at distant localities—and a permanent elevation of the land, to be of the greatest importance, as forming parts of one great action, and being the effects of one great cause, modified only by local circumstances. Mr. Darwin further observed, that, as the volcanos near Chiloe commenced, at the moment of the shock, a period of renewed activity, which lasted throughout the following year, the motive power of these volcanos (as well as of the submarine outburst near Juan Fernandez) must be of a similar nature with that, which, at the same instant, permanently raised another part of the coast; and he therefore concluded, that no theory of the cause of volcanos, which is not applicable to continental elevations, can be considered as well-grounded.

Mr. Darwin then offered some remarks on the two tables published by Humboldt, of the great earthquakes which affected, in 1797 and 1811, so large portions of America; and he is of opinion, that a repetition of the coincidences can alone determine how far the increased activity of the subterranean powers, at such remote points, was the effect of some general law, or of accident. He likewise disbelieves, that periodical eruptions, as those of Coseguina, in 1709 and 1809, or of earthquakes, as the shocks felt at Lima on the 17th of June 1578, and the 17th of June 1678, are more than accidental agreements. He also gave a table of the volcanic phænomena in South America in 1835; and concluded, that it is probable that the subterranean forces manifest, for a period, their action, beneath a large portion of the South American continent, in the same intermittent manner as they do beneath isolated volcanos. In the latter table, Mr. Darwin pointed out the case of Osorno, Aconcagua, and Coseguina, (the first and last being 2700 miles apart,) which burst into sudden activity early on the morning of June 20th, 1835; but he hesitated to assent to there being any necessary connexion between them. He further remarked, that if such simultaneous outbursts had been observed in Hecla and Ætna, points unconnected by any uniformity of physical structure, it would be doubtful how far they would have been worthy of consideration; but in South America, where the volcanic orifices fall on one line of uniform physical structure, and where the whole country presents proofs of the action of subterranean forces, he conceives it ceases to be improbable, to any excessive degree, that the action of the volcanos should sometimes be absolutely simultaneous.

The author then briefly described the groups into which the volcanic vents of the Cordilleras have been divided. The most southern extends from Yntales to the volcanos of central Chili, a distance of nearly 800 geographical miles; the second, from Arequipa to Patas, rather more than 600 miles; the third, from Riobamba to Popayan, a distance of about 300 miles; and to the northward, there are in Guatimala, Mexico, and California, three groups of volcanos separated from each other a few hundred miles. That the vents in each of these groups are connected, the author has little doubt; but that the groups are united in one system, there are less satisfactory means of proving.

Mr. Darwin next considered the nature of the earthquakes which occur at irregular intervals on the South American coast. He is perfectly convinced, from the numerous points of analogy which exist between these phænomena and simple eruptions, that they belong to the same class of events; but he makes this distinction, that earthquakes, unaccompanied by eruptions at the chief point of disturbance, are followed by a vast number of minor shocks. These, he believes, indicate a repeated rending of the strata beneath the surface; whereas, in an ordinary eruption, a channel is formed during the first outburst.

Among other phænomena belonging to earthquakes, Mr. Darwin alluded to their affecting elongated areas. Thus the shock in Syria, in 1837, was felt on a line 500 miles in length by 90 in breadth; and those in South America are felt along 800 and 1000 miles of coast, but are on no occasion transmitted across the Cordillera to a nearly equal distance; and, as a consequence, the inland towns are much less affected than those near the coast. He does not conceive, however, that the disturbances proceed from one point, but many ranged in a band, otherwise the linear extension of earthquakes would be unintelligible. For instance, in 1835, the island of Chiloe, the neighbourhood of Concepcion and Juan Fernandez were all violently affected at the same time.

The last consideration which Mr. Darwin entered upon indicating the cause of earthquakes, is, that in South America they have been generally accompanied by elevation of the land; though it is not a necessary concomitant, at least to a perceptible amount. But he especially observed, that, as at Concepcion, during the few days succeeding the great shock, several hundred earthquakes, of no inconsiderable violence, were experienced, whilst the level of the ground in that part of the coast certainly was not raised by them (but after the interval of a few weeks, it stood lower,), there is a clear indication of some cause of disturbance, independent of the uplifting of the land in mass.

In summing up the evidence of phænomena accompanying earthquakes, the author is of opinion that the following conclusions may be drawn:—

1st. That the primary shock of an earthquake is caused by a violent rending of the strata, which, on the coast of Chili and Peru, seems generally to occur at the bottom of the neighbouring sea.

2ndly. That this is followed by many minor fractures, which, though extending upwards, do not, except in submarine volcanos, actually reach the surface.

3dly. That the area thus fissured extends parallel, or approximately so, to the neighbouring coast mountains.

Lastly. That the earthquake relieves the subterranean force, precisely in the same manner as an eruption through an ordinary volcano*.

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* [Those who have perused Sir John F. W. Herschel's views on the theory of volcanic action (Babbage's Ninth Bridgewater Treatise, sec. edit. pp. 230–240,) will not fail to recognise the close accordance with them, of

The author afterwards discussed the nature and phænomena of mountain chains; and stated his belief, that the injection, when in a fluid state, of the great mass of crystalline matter, of which the axis is generally composed, would relieve the subterranean pressure in the same manner as an ejection of lava or scoria; and that the dislocation of the strata would produce horizontal vibrations through the surrounding country. In drawing this parallel, he also stated his belief, that the earthquake of Concepcion marked one step in the elevation of a mountain chain; and he adduced, in support of this opinion, the fact observed by Capt. Fitzroy, that the island of Santa Maria, situated 35 miles to the south-west of that city, was elevated to three times the height of the upraised coast near Concepcion; or at the southern extremity of the island, eight feet; in the middle, nine feet; and at the northern extremity, upwards of ten feet; and that at Tubal, to the south-east of Santa Maria, the land was raised six feet*; this unequal change of level indicating, in his opinion, an axis of elevation in the bottom of the sea, off the northern end of Santa Maria.

Mr. Darwin then alluded to Mr. Hopkins's Researches in Physical Geology,† where it is demonstrated, that if an elongated area were elevated uniformly, it would crack or yield parallel to its longer axis; and that if the force acted unequally, transverse cracks or fissures would be produced, and that the masses, thus unequally disturbed, would represent the irregular outline of a mountain-chain. He further added, that if the force should act unequally beneath the area simultaneously affected, various fissures would be formed in different parts, having different directions, and thus give rise, at the same moment, to as many local earthquakes. The author believes, that this view will more readily explain intermediate districts being little disturbed (as Valdivia in 1835, and in cases alluded to by Humboldt,) than the supposed inertness of intermediary rock in conveying the vibrations from a deeply-seated focus.

If the preceding theory of the cause of earthquakes be true, Mr. Darwin said, we might expect to find, that the many parallel ridges of which the Cordillera is composed, were of successive ages. In Central Chili, the only portion examined by him, this is the case, even with regard to the two main ridges; and some of the exterior lines of mountains appear, likewise, to be of subsequent dates to the central ones. The contemplation of these phænomena led him, while in South America, to infer, that mountain-chains are only subsidiary, and attendant operations on continental elevations.

The conclusion, that mountain-chains are formed by a long succession of small movements, the author conceived may be arrived at by theoretical reasoning. The first effect of disturbing agents, Mr. Hopkins has shown, is to arch the crust of the earth, and to traverse

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the phænomena above described and the conclusions above drawn by Mr. Darwin.—Edit.]

* Journal of the Royal Geographical Society, vol. vi. p. 327.

† [See Lond. and Edinb. Phil. Mag. vol. viii. p. 227, *et seq.*]

it by a system of parallel but vertical fissures; and that subsequent elevations and subsidences of the disjointed masses would produce anticlinal and synclinal lines. In the Cordillera, the strata in the central parts, are inclined at an angle commonly exceeding 45°, and are very often absolutely vertical, the axis being composed of granitic masses, which, from the number of dikes branching from them, must have been fluid when propelled against the lower beds. How then, he asked, could the strata have been placed in a highly inclined and often vertical position, by the action of the fluid rock beneath, without the very bowels of the earth gushing out? If, on the other hand, it be supposed that mountain-chains were formed by a succession of shocks similar to those which elevated Concepcion, and after long intervals, time would be allowed for the injected rock to become solid, as well as the upper part of the great central mass. Thus, by a succession of movements, the strata might be placed in any position; and the crystalline nucleus gradually thickening, would prevent the surface of the surrounding country being inundated with molten matter.

In crossing the Andes, Mr. Darwin was surprised at finding, not one great anticlinal line, but eight, or more; and that the rocks composing the axes were seldom visible, except in denuded patches in the vallies. This circumstance, he conceives, must be due to the thickness of the upheaved strata being equal, or nearly so, to the average distance of the anticlinal from the synclinal lines. For in that case. the masses of strata, when placed vertically, would occupy, or rest on, as great an horizontal extent, as they did before they were disturbed.

In the central ridges of the Cordillera, there are masses of compact, unstratified rocks, half again as lofty as Ætna; and these, he believes, for the reasons before stated, were formed by the gradual cooling of the subjacent fluid mass; afterwards slowly elevated to the present position, by the injection of molten matter at nearly as slow a rate, as we must suppose the innumerable layers of volcanic products, of which the Sicilian mountain is formed, have been ejected.

In conclusion, Mr. Darwin repeated the argument, that mountain-chains and volcanos are due to the same cause, and may be considered as mere subsidiary phænomena, attendant on continental elevations;—that continental elevations, and the action of volcanos, are phænomena now in progress, caused by some slow but great change in the interior of the earth; and, therefore, that it might be anticipated, that the formation of mountain-chains is likewise in progress; and at a rate which may be judged of, by either actions, but most clearly by the growth of volcanos.

March 21st.—A paper was first read, on the Dislocation of the Tail, at a certain point, observable in the skeletons of many Ichthyosauri, by Richard Owen, Esq., F.G.S., Hunterian Professor to the Royal College of Surgeons, London.

Mr. Owen commences his observations by referring to the skeleton of the existing cetacea, and pointing out how slight is the indication afforded by the caudal vertebræ of the large terminal fin, which forms, in that class, so important an organ of locomotion; and the improbability that its presence would have been suspected, had the cetacea been known only by their fossil remains, in consequence of the fin having consisted entirely of decomposable and unossified material.

He states, that the flattened shape of the terminal vertebræ, which gives the ouly indication of the horizontal fin—and which character is not present in all the cetacea—is not recognisable in the skeletons of the *Ichthyosauri* and *Plesiosauri*; but he proceeds to describe a condition of the tail in the skeletons of the Ichthyosauri, which, he conceives, affords an indication of a structure in the extinct animal, analogous to the tegumentary fin of the cetacea, and which has not been suspected by the authors of the conjecturally-restored figures of the *Ichthyosauri*, already published. The condition alluded to, is described as an abrupt bend of the tail about one-third of its whole length distant from the end; and at the thirtieth caudal vertebra in the *Ichthyosaurus communis*; the broken portion continuing, beyond the dislocation, as straight as in the part which precedes it. As there is no appearance of a modification of structure in the dislocated vertebræ, indicative of the tail having possessed more mobility at that point than at any other; and as the dislocation has taken place at the same point in seven specimens examined by the author, he conceives that it must be due to some cause operating in a peculiar manner on the dead carcase of the *Ichthyosaurus*, in consequence of some peculiarity of external form, while it floated on the surface of the sea.

A broad tegumentary fin, composed of dense but decomposable material, might have been attached to the terminal portion of the tail; and such a fin, either by its weight, or by presenting an extended surface to the beating of the waves, or by attracting predatory animals of strength sufficient to tug at, without tearing it off, would occasion, when decomposition of the connecting ligaments had sufficiently far advanced, a dislocation of the vertebræ immediately proximate of its point of attachment. The two portions of the tail, with the rest of the skeleton, would continue to be held together by the dense exterior integument, until the rupture of the parietes of the abdomen, at some yielding point, had set free the gases generated by putrefaction; and the skeleton, having undergone certain partial dislocations, from the decomposition of the more yielding ligaments, would subside to the bottom, and become imbedded in the sedimentary deposits, exhibiting the fracture of the tail alluded to.

With respect to the relative position of this conjectured, caudal, tegumentary fin of the *Ichthyosaurus*, Mr. Owen cannot perceive any indication of its horizontality in the forms of the vertebræ, which he supposes to have supported it; and he regards the superaddition of posterior paddles in these air-breathing marine animals, as a compensation for the absence of that form of fin, which is so essential in the cetacea, for the purpose of bringing the head to the surface of the sea to inhale the air. On the other hand, a vertical caudal fin seems especially required by the short-necked and stiff-necked *Ichthyosauri*, in order to produce, with sufficient rapidity, the lateral movements of the head, which were needed by those predatory inhabitants of the ancient deep; while, in the *Plesiosaurus*, such a fin would be unnecessary, in consequence of the length and mobility of the neck; and Mr. Owen concludes, by stating, that in those skeletons of *Plesiosauri* in which the tail is perfect, it is straight, and presents no indication of the partial fracture or bend, which is so common in the tails of *Ichthyosauri*.

Figures of the tails of five specimens of *Ichthyosauri*, now in London, accompanied the Note; the subject of which was also illustrated by a sixth skeleton of an *Ichthyosaurus* on the Table, the property of Sir John Mordaunt, Bart.

A paper was commenced, on the Primary Formations of England, by the Rev. Adam Sedgwick, V.P.G.S.; Woodwardian Professor in the University of Cambridge, &c.

ZOOLOGICAL SOCIETY.

[Continued from p. 531.]

July 11, 1837.—A letter was read from Mr. Hugh Cuming, Corresponding Member, dated Manilla, December 24th, 1836, addressed to the late Secretary, E. T. Bennett, Esq.

Mr. Cuming states in this letter that he is actively engaged in his favourite pursuit, that of collecting objects in various departments of natural history, and he speaks very highly of the assistance afforded him by the public authorities at Manilla in prosecuting his researches. This letter was accompanied by a large box of skins of birds and quadrupeds, part of which were a donation to the Society.

A letter was read from Keith Edward Abbott, Esq., Corresponding Member, dated Erzeroum, May 12, 1837, stating that he had dispatched a box of bird-skins for the Society.

Mr. Martin then laid before the meeting the following observations on the Proboscis Monkey, or '*Guenon à long nez*.' (*Simia Nasalis*.)

The genus *Nasalis*, of which the "Guenon à long nez" of Buffon, (suppl. vii.,) or Proboscis Monkey of Shaw, is the type, was founded by Geoffroy St. Hilaire in his '*Tableau des Quadrumanes*,' published in the 'Annales du Muséum d'Histoire Naturelle' for 1812. In this outline of the *Simiadæ*, the genera *Semnopithecus* and *Cercopithecus* are blended together under the latter title; but from this group are excluded two monkeys, the Douc, constituting the type of the genus *Pygathrix* (*Lasiopyga*, Ill.) and the "Guenon à long nez". With respect to the genus *Pygathrix* or *Lasiopyga*, founded upon the alleged want of callosities, most naturalists I believe, (aware of the error committed both by Geoffroy and Illiger, in describing from an imperfect skin,) have regarded it as merging into the genus *Semnopithecus*, at least provisionally, until the internal anatomy of its assumed representative be known.

The characters of the genus *Nasalis*, formed for the reception of the "*Guenon à long nez,*" (*Simia Nasica*, Schreb. *Cercopithecus larvatus*, Wurmb,) are laid down as follows:

"*Muzzle* short, forehead projecting, but little elevated; *facial angle* 50°; *nose* prominent, and extremely elongated; *ears* small and round; *body* stout; *cheek-pouches*, *anterior hands*, with four long fingers, and a short thumb, ending where the index finger begins; *posterior hands* very large, with fingers stout, especially the thumb; *callosities* large; *tail* longer than the body."

At a subsequent period, however, in his 'Cours de l'Histoire Naturelle,' published 1828, Geoffroy, adopting the genus *Semnopithecus*, established by Fred. Cuvier, places the "*Guenon à long nez,*" within its limits, doubtfully it is true, and with the acknowledgment that his genus *Nasalis* has not been generally adopted, but at the same time with a bias in its favour; for observing that the manners of these monkeys are those of the Semnopitheci, he adds,—"Cependant, il ne nous parait encore démontré que le singe nasique soit une véritable semnopitheque, et il est fort possible que lorsque l'espèce sera moins imparfaitement connue, on soit obligé de rétablir le genre *Nasalis*, dans lequel on l'isolait autrefois, mais que n'est pas été admis par la plupart des auteurs modernes."

Setting aside the singular conformation of the nose, so remarkable in the *Simia Nasalis*, its external characters are not different from those of the *Semnopitheci* in general, and it is to be observed that in a second species, lately added by Mr. Vigors and Dr. Horsfield, under the title of *Nasalis recurvus*, the proportions of this part of the face are much diminished, and its form also modified. This species (which though doubted by some as being distinct, is, we believe, truly so) takes an intermediate station between the *Simia Nasalis*, and the ordinary *Semnopitheci* with flat noses, thereby showing that the transition in this particular character is not abrupt; even were it so, an isolated point of this nature does not form a philosophical basis upon which to ground a generic distinction.

So far I have alluded to external characters only; it remains for me to give some account of the anatomical characters of this singular monkey, of which, as far as I can learn, modern naturalists do not appear to be aware.

It would seem that M. Otto*, who described the sacculated form of the stomach in one of the monkeys of the genus *Semnopithecus*, is not the first observer of this peculiarity, for I find that Wurmb, in the Memoirs of the Society of Batavia, notices this point in the anatomy of an individual of the *Simia Nasalis*. After giving some interesting details respecting the habits and manners of the species, he proceeds as follows:—"The brain resembles that of man; the lungs are of a snow-white colour; the heart is covered with fat, and this is the only part in which fat is found. The *stomach is extraordinarily large, and of an irregular form*; and *there is beneath the skin a sac which extends from the lower jaw to the clavicles.*" Audebert (with whose work 'Histoire des Singes,' Geoffroy St. Hilaire was well acquainted,) refers to this account of Wurmb; yet Geoffroy does not, as far as I can find, advert to these points, unless indeed his statement of the presence of *cheek-pouches* be founded on the observation of a sac extending from the lower jaw to the clavicles; and if so, he has made a singular mistake, for the sac in question is *laryngeal*, and the words as they stand cannot be supposed to mean any thing else; I know of no monkey whose cheek-pouches extend beneath the skin to the clavicles; but the laryngeal sacs in the *Orang* and *Gibbons*, and also in the *Semnopitheci* themselves are remarkable for development. It is evident, however, from the silence of M. Geoffroy St. Hilaire respecting the laryngeal *sacculus* in the Proboscis Monkey that he was not aware of the real character of the structure to which Wurmb had alluded. With respect to the structure of the stomach, neither Wurmb nor M. Otto drew any general inferences from it; they described it as it presented itself in single species, and regarded it in an isolated point of view; it is, if I mistake not, to Mr. Owen that we owe its reception as an anatomical character, extant throughout the *Semnopitheci*. (See his paper on the subject, in the Proceedings for 1833* and in the Transactions of the Zoological Society, vol. i.)

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* See his paper in the "Nova Acta Academiæ Cæsareæ," vol. xii.

This is perhaps scarcely the place in which to introduce any speculations, but I cannot help observing that the same structure may be expected in the genus *Colobus*, which in form is a mere repetition of the genus *Semnopithecus*, except that the thumb of the forehands, which in the latter begins to assume a rudimentary character, is in the former reduced to its lowest stage of development. In both genera the teeth precisely agree, and present early that worn surface which is the consequence of a continued grinding *rodent-like* action, upon the leaves and herbaceous matter which constitute the chief diet of the animals.

The statement of Wurmb respecting the stomach and laryngeal apparatus of the Proboscis Monkey I have lately been enabled to confirm.

Among the specimens in store brought within the last few months from the Gardens to the Museum occurred an example of the Proboscis Monkey, in brine, but in a state of decomposition which induced me to lose no time in making such an examination as its condition would admit, being indeed extremely anxious to ascertain the relationship of this curious monkey to the other groups of Indian *Simiadæ*, groups to which I have been lately directing my attention.

The specimen in question was a female, measuring from the *vertex* to the *ischiatic callosities* one foot nine inches.

The body was meagre and slender, and the limbs long and slim; the contour of the animal being very unlike that displayed in the mounted specimen in the Museum of the Society, which gives the idea of great robustness.

The abdominal cavity had at some former period been opened and the liver removed, in doing which the stomach had been cut, but not so much as to spoil it entirely. In every essential point this *viscus* is the same as in all the *Semnopitheci* hitherto examined. It consists of a large cardiac pouch with a strong muscular band, running as it were around it so as to divide it into two compartments, an upper and lower, slightly corrugated into *sacculi*; the *cardiac apex* of the upper pouch projects as a distinct *sacculus* of an oval form, and is not bifid. From this *upper pouch* runs a long and gradually narrowing *pyloric portion*, corrugated into *sacculi* by means of three muscular bands, of which one is continued from the band dividing the cardiac pouch into two compartments. The elongated *pyloric portion* sweeps around the *lower cardiac pouch*.

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* [Noticed in Lond. and Edinb. Phil. Mag. vol. iii. p. 295.]

The *œsophagus* enters the first compartment about four inches from its terminal apex, giving off a radiation of longitudinal muscular fibres over the central portion of the first compartment. The second or lower compartment is the largest and deepest, and is embraced by longitudinal muscular fibres from the œsophagus to the division-band, but unlike the same compartment in the stomach of the *Semnopithecus Entellus*, it is very slightly sacculated; indeed it can scarcely be said to be so at all. The admeasurements are as follow:

	feet.	inches.
1st compartment, round the greater curve......	1	6
2nd compartment, measured in the same manner	1	8½
From the entrance of the œsophagus, round the 2nd compartment to the division-band	1	1
The same measurement, round the 1st compartment....................................	0	8½
Length of *pyloric portion*	2	1
Circumference at base	0	9½
Circumference just above pyloric orifice........	0	5½
Length of small intestines	18	0
Length of large intestines	6	2

The average diameter of the small intestines, lying flat, was ¾ of an inch; the ileum, however, was rather more, but not quite an inch.

The *cæcum* is of a pyramidal figure, 5 inches in length, pointed, and somewhat sacculated by three slight muscular bands. Circumference at the base, 5¼ inches.

The large intestines are puckered into *sacculi* by two longitudinal bands; they commence large, becoming gradually smaller, the bands in the meantime gradually disappearing. Advancing towards the *rectum* the intestine again enlarges, and here, to the extent of 2½ feet from the anus, all trace of bands is lost.

The circumference of the large intestines at their commencement is 3½ inches.

The lungs consisted of two lobes on each side, the fissure dividing the lobes on the right side being the most complete.

The laryngeal sac was of enormous size, and single. It extended over the whole of the throat, and advanced below the clavicles, communicating by means of a single but large opening with the larynx. This opening is on the left side, between the *larynx* and the *os hyoides*, and is capable of being closed by means of a muscle arising from the anterior apex of the *os hyoides*, and running down the central aspect of the *trachea* to the *sternum*. The contraction of this muscle draws the *os hyoides* down, so as to press upon the edge of the thyroid cartilage.

There were no cheek-pouches nor any traces of them.

The teeth were much worn, but the fifth tubercle of the last molar tooth of the lower jaw was very distinct.

Mr. Gould afterwards called the attention of the Meeting to the common British Wagtail, and stated his firm conviction of its being distinct from the *Motacilla alba* of Linnæus. He proposed for it the name of *M. Yarrellii*, and observed, that it might be easily distinguished from the continental one, with which it had hitherto been confounded, by an attention to the following characters.

The pied wagtail of England (*M. Yarrellii*) is somewhat more robust in form, and in its full summer dress has the whole of the head, chest, and back of a full, deep, jet black; while in *M. alba*, at the same period, the throat and head alone are of this colour, the back and the rest of the upper surface being of a light ash-grey. In winter the two species more nearly assimilate in their colouring; and this circumstance has doubtless been the cause of their being hitherto considered identical; the black back of *M. Yarrellii* being grey at this season, although never so light as in *M. alba*. An additional evidence of their being distinct (but which has doubtless contributed to the confusion), is, that the female of *M. Yarrellii* never has the back black, as in the male; this part, even in summer, being dark grey; in which respect it closely resembles the other species.

July 25th, 1837.—Mr. Waterhouse directed the attention of the Meeting to several small Quadrupeds which he considered undescribed.

Phascogale flavipes, from North of Hunter's River, New South Wales.

The fur of this animal is moderately long, not very soft, and consists of hairs of two lengths. On the back the shorter hairs are of a palish ochre colour at the apex, and the longer hairs are black: on the sides of the body and limbs the ochreous hue prevails, the black hairs being less numerous: the under parts of the body are of a yellow colour, inclining to white on the throat and mesial line of the belly; all the hairs are of a deep gray at the base both on the under and upper parts of the body. The general hue of the head is gray, a tint produced by the mixture of black and white hairs; the eyelids are black: the hairs immediately above and below the eye are of a yellow-white colour, as are also those of the upper lip and lower part of the cheeks. The moustaches are moderately long; the hairs are black at the base and grayish at the apex. The ears are of moderate size, and have the hinder portion emarginated; they are furnished externally with minute hairs, those on the inner side being chiefly of a yellow colour. The feet are of an uniform deep ochre colour. The tail is about equal in length to the body and half the head, and is furnished with small and closely adpressed hairs, between which rings of scales are visible; on the apical portion of the tail the hairs are longer, slightly exceeding one eighth of an inch in length; the hairs on the under side of the tail are of a deep buff colour, and those of the upper side are black and yellow, excepting at the apex, where all the hairs are black.

The teeth in this species agree in number with those of *Phascogale penicillata*, and in fact scarcely differ in any respect, making allowance for the difference in the size of the animals. The two front incisors of both upper and lower jaws are perhaps smaller in proportion, and the third false molar in the lower jaw is decidedly smaller in proportion, being scarcely visible unless the gum be removed. The last molar of the upper jaw is of the same narrow form, and placed obliquely as in *P. penicillata*.

Not having a skull of *P. penicillata*, I am guided in my observations by M. Temminck's figure in the 'Monographies de Mammalogie.'* Upon comparing the skulls of *P. flavipes* with the same figure, the resemblance is great; in the smaller animal, however, the skull is somewhat narrower in proportion (especially the fore part); the nasal bones are not so broad at their base.

Phascogale murina, from North of Hunter's River, New South Wales.

This species may be readily distinguished from the former by its much smaller size, being in fact rather less than the common mouse (*Mus musculus*), or less than half the bulk of *P. flavipes*. The fur is rather short and soft; its general hue is gray with a faint yellowish tint, the longer hairs on the upper parts of the body being gray at the apex, and the shorter hairs tipped with pale yellow or cream colour; the feet and under parts are white, as are likewise the sides of the face beneath the eye. All the hairs of the body are of a deep slate colour at the base. The tail is covered with very minute closely adpressed silvery white hairs. The dentition is evidently that of an adult animal: the canines and anterior incisors of both upper and lower jaws appear to be smaller in proportion than in *P. flavipes*.

Mus Hayi, from Morocco.

This species, which is rather larger than *Mus musculus*, was presented to the Zoological Society by E. W. A. Drummond Hay, Esq., Corr. Mem., after whom I have taken the liberty of naming it.

Mus Alleni, from Fernando Po.

This species is less than the harvest mouse (*Mus messorius*), and of a deeper colour than the common mouse (*Mus musculus*), being in fact almost black. The ears are smaller in proportion, and more distinctly clothed with hairs. The tail is very sparingly furnished with minute hairs. The tarsi are covered with blackish hairs above; the toes are dirty white.

I have named the species after Lieut. W. Allen, R.N., Corr. Mem. by whom it was discovered and presented to the Zoological Society.

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* In M. Temminck's figure the three lateral incisors of the upper jaw are represented as being close to the anterior pair. There is, however, a space between the anterior incisors and the lateral, both in *P. penicillata* and in the two species here described.

Mus Abbottii, from Trebizond.

This species is less than the harvest mouse (*Mus messorius*), and of a deeper colour than the *Mus musculus*, in which respects it agrees with *Mus Alleni*; from this, however, it may be distinguished by the tail being longer in proportion, the ears larger, and the tarsi more slender. It was presented to the Zoological Society by Keith E. Abbott, Esq., Corr. Mem., after whom it has been named.

Mr. Gould then continued the exhibition of Mr. Darwin's Birds, a series of which were upon the table. One only among them was considered new, a species belonging to the genus Pyrgita from the island of St. Iago. Mr. Gould characterized it under the name of *Pyrgita Iagoensis*, from St. Iago. This is in every respect a typical *Pyrgita*, and rather smaller than the common species, *P. domestica*.

Mr. Gould then called the attention of the Members to some specimens of *M. alba* and *M. Yarrellii*, which presented in a very decided manner the distinctions referred to by him at the last Meeting. He afterwards characterized a new species of that genus under the name of *Motacilla leucopsis*, from India.

August 8th, 1837.—Mr. Gould then characterised the following birds from the Society's collection as new species:

Corvus nobilis, from Mexico. This beautiful species is a true raven, and may be distinguished from the European, and from that inhabiting the United States of America, by the more metallic lustre of its plumage, by its more lengthened and slender bill, the greater length of its primaries, and the more cuneate form of its tail.

Ortyx guttata, from the Bay of Honduras; *Thamnophilus fuliginosus*, from Demerara; *Dendrocitta rufigaster* from India.

Mr. Ogilby exhibited skins of two species of his new genus *Kemas**, and directed the attention of the Society to their generic and specific characters. Mr. Ogilby observed, that the genus in question occupied an intermediate station between the goats and the *Oryges*, agreeing with the former in its mountain habitat and general conformation, and with the latter in the presence of a small naked muzzle and four teats in the females. Of the two species exhibited, one was a fine male specimen of the *Iharal*, presented by James Farrall, Esq., and the other a new species from the Neilgherry Hills, known to Madras and Bombay sportsmen by the name of the Jungle Sheep, and which Mr. Ogilby had long looked for. In form and habit of body, as well as in the character of the horns, this animal is intermediate to the *Iharal* and *Ghoral*; the specific name of *Kemas Hylocrius* was proposed for it in allusion to its local appellation. The body is covered with uniform short hair, obscurely annulated like that of most species of deer, and more nearly resembling the coat of the Ghoral than that of either the *Iharal* or *Chamois*, the other species of which the genus is at present composed. The horns are uniformly bent back, surrounded by numerous small rings, rather flattened on the sides, with a small longitudinal ridge on the inner anterior edge: the ears are of moderate length, and the tail very short. Mr. Ogilby entered at some length into the characters and relations of the genus *Kemas*; he observed that naturalists and commentators had greatly puzzled themselves to discover the derivation of the word *Kemas*, and the animal to which the ancient Greeks applied that name. Among others, Col. H. Smith applies it to the *Chiru*, with which the ancients certainly were not acquainted: but Mr. Ogilby observed, that the root, both of the Greek *Kemas* and the modern *Chamois*, was manifestly traceable to the German word *Gems*, which is still the name of the Chamois eastward of the Rhine, and which the Dutch colonists have transferred to the Cape *Oryx* (*Oryx capensis*).

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* [See Lond. and Edinb. Phil. Mag. vol. xi. p. 473.]

August 22nd, 1837.—Mr. Owen brought before the notice of the Society, through the kindness of Mr. Edward Verreaux, the cranium of an Orang Outang (*Simia Wurmbii*, Fisch.), exhibiting an intermediate or transitional state of dentition, there being in the upper jaw the first or middle incisors, and first and second molares on each side belonging to the permanent series, and the lateral incisors, the canines, and the first and second molares (which are replaced by the bicuspides) belonging to the deciduous series; and in the lower jaw, both the middle and lateral incisors, and first and second molares on each side belonging to the permanent series, and the second left lateral deciduous incisor (not yet shed), the deciduous canines, and the first and second deciduous molares.

The permanent teeth, which were in place, corresponded in size with those of the great *Pongo* of Wurmb, and prove that the Orang differs from man in the order of succession of the permanent teeth, having the second true molar, (or fourth if the bicuspides are reckoned as molars), in place before the appearance of the permanent canines.

Mr. Owen remarked, that the intermaxillary suture still remained unobliterated in the immature cranium exhibited, and he conceived that the ultimate obliteration might be caused by the increased vascularity of the parts during the protrusion of the great laniary teeth. In the Chimpanzee this obliteration takes place at a much earlier period.

Although the marks of immaturity, and consequently those which impress an anthropoid character upon the skull of the Orang, were generally present in the head exhibited, yet, on a comparison of it with the skull of a younger Orang in which all the deciduous teeth were retained, an approach to the condition of the mature cranium might be observed in the greater protrusion of the intermaxillaries, the lengthening of the maxillary bones, a thickening and greater prominence of the external and superior boundary of the orbit, an enlargement and thickening of the malar bone and zygoma, in the commencement of the development of the cranial ridges, and in the widening and deepening of the lower jaw*.

Mr. Owen then directed the attention of the Meeting to an exceedingly interesting preparation of a fœtal Kangaroo, with its accompanying uterine membranes, upon which he proceeded to offer some observations. He remarked, that in a paper read before the Royal Society in 1834*, he described the fœtus and membranes of a Kangaroo (*Macropus major*), at about the middle period of uterine gestation, which in that animal lasts thirty-eight days. In this instance the condition of the membranes, and the relation of the fœtus to the mother, were essentially such as are found to exist throughout the ovo-viviparous reptiles, with the exception of there being no trace of the existence of an allantois. Mr. Owen, in order to determine whether an allantois was developed at a subsequent period of the growth of the embryo, dissected very young mammary fœtuses of different marsupial animals, as the *Kangaroo*, *Phalangista*, and *Petaurus*; and finding in them the remains of a *urachus* and umbilical vessels, he stated that "it would appear that an allantois and umbilical vessels are developed at a later period of gestation, but probably not to a greater extent than to serve as a receptacle of urine." (Phil. Trans., 1834, p. 342.)

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* [Abstracts of Prof. Owen's former memoirs on different species of Orangs have appeared in Phil. Mag. and Annals, N.S. vol. ix. p. 60; and Lond. and Edinb. Phil. Mag. vol. vi. p. 457, and vol. x. p. 295.—Edit.]

The examination of a uterine fœtus of a Kangaroo kindly placed at Mr. Owen's disposal by Dr. Shearman, and exhibited on this occasion to the Society, has proved the accuracy of this prevision. The chorion, which enveloped and concealed the fœtus, was a sac of considerable capacity, exceeding probably by ten times the bulk of the fœtus and its immediate appendages, and adapted to the smaller cavity of the uterus by being disposed in innumerable folds and wrinkles. It did not adhere at any part of its circumference to the uterus, but presented a most interesting modification not observed in the previous dissection of the Kangaroo's impregnated uterus, viz., that it was in part organized by the extension of the omphalo-mesenteric vessels upon it from the adherent umbilical sac. The fœtus was further advanced than the one previously described in the Philosophical Transactions. The digits on the hinder extremities were distinctly formed. The umbilical chord extended nearly three lines from the abdominal surface of the fœtus; the amnios was reflected from this point, to form the usual immediately investing tunic of the fœtus; and, beyond the point of reflection, the chord divided into a very large superior vascular sac, organized by the omphalo-mesenteric vessels, corresponding in all respects with the vitelline sac described and figured in Mr. Owen's first paper; but below the neck of this sac there extended a second pyriform sac, about one-sixth the size of the vitelline sac, having numerous ramifications of the umbilical vessels, and constituting a true allantois. This sac was suspended freely from the end of the umbilical chord: it had no connexion, at any part of its circumference, with the chorion, and consequently was equally free from attachment to the parietes of the uterus in which the fœtus was developed†.

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* [See Lond. and Edinb. Phil. Mag., vol. iv. p. 438.]

† The following note has been communicated by Mr. Owen to be appended as a postscript to the above remarks. "Having been anticipated

Mr. Charlesworth then exhibited a series of specimens of the paper nautilus, in several of which injuries to a very considerable extent had been repaired with new substance agreeing in every respect with the original shell; affording the most decisive evidence that the animal by which they were constructed possessed the same reparative powers as other testaceous molluscs. It would appear from the observations of Captain Rang, who had recently repeated at Algiers the experiments originally undertaken by Madame Jeanette Power at Messina, that the Poulp does not fill up the breaches artificially produced in its habitation by a deposit of shelly matter, but with a transparent diaphragm, which has neither the texture, whiteness, or solidity of the original shell. This fact, in connection with the specimens exhibited to the Meeting, appeared to Mr. Charlesworth strongly to confirm the opinion entertained by Mr. Gray, De Blainville, and others, of the parasitic character of the genus *Ocythöe*.

Mr. Owen remarked, that he could not admit the validity of the line of argument adopted by Mr. Charlesworth, because the differences in the nature of the reproduced portions might depend upon the particular part of the shell in which the perforation or fracture had been effected, and a consequent difference in the reproductive powers of the corresponding part of the mantle.

METEOROLOGICAL SOCIETY.

April 10, 1838.—An interesting paper on the cold of January last, as experienced at Brussels, communicated by Prof. Quetelet, Secretary to the Royal Academy of Brussels, was read by the Secretary. The extreme cold which occurred on the 20th of the

in the description of my preparation, so far as relates to the allantois, by M. Coste, I here subjoin, by permission of the Committee of Publication, a statement of the circumstances which enabled that embryologist to announce the discovery of the allantois to the Academy of Sciences. In a recent work on Embryogeny, M. Coste* has stated that the Marsupiata differ from other Mammalia in the absence of an allantois,—a statement which appears to have arisen from a misconception of my memoir in the Philosophical Transactions for 1834, in which, although the allantois was not developed in the embryo, whose dissection is there figured, (Pl. VII. fig. 1.), yet the evidences of the ulterior development of an allantois in different marsupial genera, are described in the text, (p. 338, 342.) I therefore took the opportunity of showing to Dr. Coste during his visit to England the fœtal Kangaroo with the allantois now before the Society; and Mr. Coste having expressed some doubts respecting my determination of the two appended sacs, we together dissected the fœtus, and found that the vessels ramifying on the larger sac, which I had before described as the umbilical vesicle, had the usual disposition and connections within the abdomen of omphalo-mesenteric trunks, corresponding with the figure above-cited in the Philosophical Transactions, and that the allantois was continued from an urachus, such as is represented in figs. 6, 7 and 8, pl. VII., Philos. Trans., 1834."

* *Embryogenie comparée*, p. 118.

month was 5° Fahr. This was the coldest January since 1823, the lowest point of which was stated by M. Quetelet to be $1\frac{1}{2}$° Fahr. M. Crahay noticed the thermometer at Maestricht on the same day to stand at $9\frac{1}{4}$ degrees below zero.

A short paper was next read from Prof. Wartmann, Geneva, giving an account of an atmospheric bow, which was seen on the 12th February 1837, in perfectly serene weather; it exhibited all the colours of the rainbow in a very distinct manner, but it did not appear in a vertical position like the ordinary rainbow, but inclined to the plane of the earth; it did not partake in any degree of the nature of a halo. It became visible at 5 minutes past 10 a.m., the sun shining with all its brightness, and lasted till 45 minutes past 10 a.m. It was not accompanied by any parhelia, nor was there any appearance of cloud till half past 11 a.m., when a few light clouds were seen passing in the superior strata of the atmosphere; the afternoon was overcast without rain.

LXXXV. *Notices respecting New Books.*

Analytical Geometry: Part First, containing the Theory of the Conic Sections; Part Second, the Theory of Curves and Surfaces of the Second Order. By J. R. Young, Professor of Mathematics, Royal College, Belfast. 2 vols. 12mo. Souter.

THE properties of curves and surfaces of the second order, whilst they are the most interesting of all geometrical speculations, are at the same time the most important in their application to physical inquiry. They did not, however, originate in the wants of the physical inquirer; nor, indeed, have any great number of them been discovered since the time of Kepler. There are comparatively few properties of these curves now known to geometers, which were not either known to the ancients, or which are not merely easy deductions from such as were known to them. If we except the doctrine of curvature, and the properties derivable from Desargue's theorem upon the "involution of six points," it would be difficult to specify a proposition which is not included under the statement just made. At all events, if a different view should be taken of it, no one will contend that the discovery of these properties, with very rare exceptions, was the consequence of any feeling of a want of them for aiding philosophical inquiries.

In truth, almost numberless as the properties of the conic sections are, the number which the physical inquirer needs is very few; and for the most part, these are comparatively elementary and easy of demonstration, either after the manner of the ancients or by the geometry of coordinates. The greater portion of them in the *present* state of science are mere objects of enlightened curiosity and analytical or geometrical exercise.

Were we, however, to make the wants of natural philosophy the standard of utility, we should greatly mistake the objects of mental culture, and in no case more completely than in the one before us.

We might at once urge that by the same rule practical utility would become the standard by which we should measure the extent to which natural philosophy itself ought to be pursued.

Of the two methods by which the properties of the conic sections and surfaces of the second order may be demonstrated, (the method of coordinates and the methods of pure geometry,) the prevailing prejudice is in favour of the former. That the coordinate method harmonizes much better with the general methods of inquiry which are found most advantageous in physical research, no one can deny; but in any other point of view the superiority of this method is very doubtful. As an intellectual exercise there can be no question respecting the superiority of the ancient methods: as an instrument of investigation of the properties of curves and surfaces of the second order, the method of transversals (especially when we adopt Chasles's application of Prop. 129. lib. vii. of the Mathematical Collections of Pappus,) is by far the most ready and effective, as well as the most general and comprehensive. On the other hand, if the investigation of these properties be viewed in reference to exercising the student in the modes of investigation that he will be most frequently required to employ in physical research, the geometry of coordinates should be the sole method he should employ. Every judicious student will, however, acquaint himself in some degree with the other methods,—fashionable as it is in our day and in this country, (but in this country only,) to treat such modes of research as antiquated and unworthy of notice.

The systematic introduction of surfaces of the second order into an elementary course of study is comparatively recent; for, though surfaces of revolution were considered by the Greeks, only very few of their properties had been investigated prior to the time of Monge, Hachette, and their disciples of the Polytechnic School. So assiduously, however, have these surfaces been studied since, that their known properties in their most general form are as numerous, (or perhaps more numerous) as even those which relate to lines of the same order; and no treatise on analytical geometry can be considered complete without the introduction of a considerable number of these. Nor are they mere subjects of speculative curiosity even in themselves. They have a direct bearing on several physical subjects of inquiry. Their great utility, however, arises from the exercise they afford to the student in the discussion of phænomena taking place in space of three dimensions—that is, in short, of nearly all the phænomena of inorganic nature. Very few motions take place in one fixed plane, and even these can be studied only in reference to some other plane or planes. They are always, or almost always, in curves of double curvature, or upon curve surfaces; and when not so, the lines or planes in which they take place cannot be contemplated and determined but by means of some other lines and planes in which the component forces act. To speak familiarly, they are resultants to be determined—not composants actually given.

The attachment to the coordinate method of geometrical research in reference to its ulterior physical application, is not a mere prejudice in the ordinary sense of the term; though it must be admitted, on the contrary, to have engendered a prejudice against the other methods which only a very partial view of the intellectual objects of mathematical science could have rendered so generally current. It would, however, be impossible in our limited space to enter upon this discussion at any length; and we merely throw out these suggestions for the consideration of those who have adopted a prejudice against methods, solely because they are in favour of a system of investigation which more immediately and advantageously applies to that branch of study to which they have devoted their attention. These persons may be reminded that even philosophy in comparison with the arts of life is secondary, on precisely the same ground that pure mathematics, in their own creed, is secondary to physics: and then we are brought back by an application of their own argument to the superiority of the Greek over the coordinate geometry. In all the arts of life constructive processes are required,—not algebraical expressions of value: we have to actually draw the line, and cut the body into a certain form and of certain dimensions,—not to compute the coordinates of points, or to form the equations of lines and surfaces. We must supply methods of actually tracing the contours of the bodies to be formed, and that by practicable processes, rather than give the relations of the coordinates of the curve or surface. Who would think of assigning the equations of the lines which by their intersection give the vanishing points of any line in a picture? Who would ever dream of laying down every point of a building from an algebraical formula? What architect would commence his operations for a roof, an arch, a groin, an ogee, by finding their equations? What carpenter would calculate the points of a handrail of a circular staircase from its equation? What mason would adopt such a method for cutting the complicated stones that are required in an ornamental building? Plain, straightforward, and simple geometrical constructions are those required in all the arts, both of life and refinement.

But it is time that we briefly notice the two elegant volumes before us.

A considerable number of works of this class have been published on the Continent, though this country can boast but very few; and of those few, it does not appear that any one of them supersedes the necessity of that now under consideration. For the greater part they are mere compilations; in most cases professedly so. Mere compilation never yet produced a good book on science. To effect this, something more is required than a mere translation of detached passages from other writers. That author who succeeds in producing a good elementary work on any scientific subject must possess powers of a high order, and knowledge very extensive, an original cast of thought, and a clear comprehension of the philosophy of science. We think ourselves tolerably familiar with the foreign writers on these subjects, and we know of no work that, for the purposes of useful and careful study, could be so advantageously put into the hands of the novice as that of Professor Young. The continual cautions against too hasty inferences from the algebraical results, (the great scientific vice to which the method itself is calculated to give birth,) the reiterated suggestions respecting the kind of caution to be observed in interpretation, and the repeated explications of the singular and seemingly absurd expressions that result from the investigation,—these, more than in any work we are acquainted with, render it a valuable book of early study. The unity of the system of development too,—the logical concatenation of the several parts,—the generality and completeness of the methods;—these give it a character peculiar to itself amongst English works on the subject, and a peculiarity which every judicious teacher knows how to estimate properly.

We remark, too, much that may be called original in method; more, indeed, than in an elementary book we should reasonably look for. To one point only can we advert here.

The doctrine of curvature has always been the great obstacle to the production of a treatise on the conic sections, which should not either directly or implicitly involve the employment of the principles of the differential calculus. This difficulty is at length overcome by Professor Young, and the entire investigation is rendered one of purely elementary algebra. It would have given us pleasure to be able to transfer it to our pages; but as the work is accessible to our readers, we think it unnecessary to do so.

LXXXVI. *Intelligence and Miscellaneous Articles.*

TARTARIC AND PARATARTARIC ACIDS, ETC.

M. DUMAS has read before the Royal Academy of Sciences of Paris, a report in his own name and those of MM. Robiquet and Pelouze, on a memoir of M. Frémy, relating to the modifications which heat occasions in tartaric and paratartaric acids.

It was first remarked by M. Braconnot, that tartaric acid when submitted to fusion was changed in its properties. M. Frémy has investigated this subject, and stated the results in the memoir now reported. The reporter observes, that the nature of the oxygenated acids may be explained by two theories, both of which are probably true, but which are probably only applicable to a certain number of bodies. One of these theories, and that which is almost universally admitted, consists in regarding them as distinct bodies, as true oxygenated acids, which combine with water and bases to form salts. The other theory does not regard these as acids when they are anhydrous, but considers them as hydro-acids, and their salts as analogous to the chlorides.

These two theories are most opposed to each other in relation to the nature of tartaric acid; for one of them, that which considers tartaric acid as an oxacid, is incompatible with the analysis of anhydrous emetic tartar; and if, according to the composition of this substance, tartaric acid is considered as an hydracid, a difficulty immediately occurs in accounting for the results obtained by M. Frémy; these are indeed much more easy of explanation in considering tartaric acid as an oxacid, as will appear on examination.

M. Frémy has discovered the substance which, according to the views generally admitted, ought to have the name of anhydrous tartaric acid. He obtains it with great facility, for it is sufficient to expose tartaric acid to the action of heat in a capsule. The acid fuses, loses water, swells up, and leaves a spongy mass which consists mostly of anhydrous tartaric acid; it is so sparingly soluble in water, that this fluid dissolves from it the tartaric acid, which has not been completely deprived of water.

It is well known that the composition of racemic or paratartaric acid is similar to that of tartaric acid. In all destructive reactions, tartaric and racemic acids undergo similar alterations, so that up to the present time nothing is known which explains the differences that may exist in their rational formulæ. M. Frémy subjected paratartaric acid to the same treatment as that by which he obtained anhydrous tartaric acid, hoping that some difference would appear in this way between these two bodies; none however occurred. Paratartaric acid behaved like tartaric acid, and yielded an analogous substance, which must be considered as anhydrous paratartaric acid.

Connected with these facts, which are sufficiently remarkable on account of the two acids obtained, M. Frémy communicated two others, which on account of their novelty, have strongly excited the attention of those who are engaged in the development of organic chemistry.

In fact it is not anhydrous tartaric acid which is directly produced by the fusion of tartaric acid. Before it arrives at this condition, common tartaric acid gives rise to two intermediate products, of great interest as regards theory. The first is tartralic acid, the second the tartrelic acid of M. Frémy. The tartralic acid is represented by tartaric acid, which, instead of saturating two atoms of base, saturates only $\frac{3}{2}$ atoms. Tartrelic acid is represented by tartaric acid, which saturates only one atom of base. So that commencing with the most probable formula for tartaric acid[*] $C^{16} H^8 O^{10}, 2H^2 O$, or $C^{16} H^{12} O^{12}$, it will be seen that the three products in question will be represented as follows, in their respective salts of lead.

$C^{16} H^8 O^{10}, 2 PbO$ tartrate.
$C^{16} H^8 O^{10}, \frac{3}{2} PbO$ tartralate.
$C^{16} H^8 O^{10}, PbO$ tartrélate.

M. Frémy has thus ascertained, that in proportion as tartaric acid loses water, it gives rise to bodies which combine with smaller quantities of base, and which form salts with bases equivalent to the proportions of water which they retain. These modifications recall those which have been assigned by Professor Graham as the causes of the variations which phosphoric acid and the phosphates undergo by the action of heat. The author has satisfied himself that the tartralic and tartrelic acids readily return to the state of tartaric acid.

The reporter observes that it results from the experiments of

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[*] These are the formulæ in the original.

M. Frémy, that tartaric acid may lose water and go through modifications analogous to those of phosphoric acid, until it becomes anhydrous tartaric acid. Paratartaric acid undergoes the same changes. M. Frémy has therefore introduced into the study of the organic acids a new point of view, and which is exclusively his own. He seems, at first, to have decided the question concerning their nature, since by discovering anhydrous tartaric acid he appears to have removed all doubt as to the formula of this acid in the state of hydrate. But with a little attention it will be observed that these new results are easily explained when tartaric acid is considered as a hydracid.

In fact, in such proportion as tartaric acid loses water, it gives rise to products whose capacity of saturation diminishes until it entirely ceases. For anhydrous tartaric acid is no longer to be considered as an acid, and after tartrélic acid, other substances are formed which have still less saturating power.

Tartaric acid and the new acids described by M. Frémy may be regarded as distinct hydracids. As to anhydrous tartaric acid, this would be a product of decomposition, but not itself an acid. These theoretic views it was considered necessary to detail in order to prove that the researches of M. Frémy do not in any way destroy the results given by the analysis of emetic tartar.—*L'Institut*, May 3, 1838.

ON THE ACTION OF FERMENTATION ON A MIXTURE OF OXYGEN AND HYDROGEN GASES. BY M. THEOD. DE SAUSSURE.

It is well known that the quantity of hydrogen gas contained in the atmosphere does not amount to 1-1000dth of its volume. Nevertheless the decomposition of organic matters continually adds fresh quantities of this gas to atmospheric air; on the other hand there are few substances which occasion the combination of hydrogen with oxygen at common temperatures; and the circumstances which the combination requires, prove that the disappearance of the hydrogen cannot be accounted for in this way. M. de Saussure states that he has found that the combination is effected by the fermentation of organic substances universally distributed over the surface of the soil, even when on account of the smallness of their quantity and the slowness of their operation no rise of temperature takes place.

By exposing fermentable bodies in pieces of the size of a nut to the mixed gases M. de Saussure has arrived at the following conclusions:—The combination of hydrogen and oxygen gases may be effected without inflammation at the temperature of the air, by bodies submitted to slow fermentation.

They usually produce this combination when they are accumulated and impregnated with a sufficient quantity of water to prevent their complete contact with the oxygen gas. If this contact be made by increasing the surface of the fermentable body, or by diminishing the quantity of water, the hydrogen gas is not absorbed, and the oxygen gas disappears in other combinations.

The porosity of the fermenting body greatly contributes to the destruction of the detonating mixture.

Many observations prove that the hydrogen gas which disappears by fermentation combines with the oxygen gas, in the proportion of the elements of water. The demonstration requires that the oxygen shall be employed only to form this water, and all the carbonic acid produced in the operation.

The fermentable substances mentioned in the memoir do not effect the combination of the oxygen and hydrogen gases before they ferment, nor when the fermentation is stopped by an antiseptic. Soils and humus, mixed with different earths, undergo a slow fermentation as soon as they are moistened, which gives them the power of destroying the mixture of oxygen and hydrogen gases.

Gaseous oxide of carbon, carburetted hydrogen gases, hydrogen gas obtained by decomposing water with red hot iron, were not destroyed by fermentation when they were substituted for common hydrogen gas, in the explosive mixture formed of two volumes of hydrogen gas and one volume of oxygen gas. Azotic, hydrogen and oxygen gases, added to the explosive mixture, do not present any remarkable obstacle to the destruction of an explosive mixture by a fermenting body, nor to that which is effected under the same circumstances by a plate of platina recently cleaned.

Oxide of carbon and olefiant gas and others, which prevent the combination of oxygen and hydrogen by platina, are also great obstacles to the same result by fermentation.

Nitrous oxide, added to the explosive mixture, was partly decomposed by fermentation, and did not prevent the combination of the hydrogen and oxygen gases.—*Bibl. Univ.* Feb. 1838.

ACTION OF SULPHATE OF AMMONIA UPON GLASS.

A mixture of muriate and nitrate of ammonia strongly corrodes glass, particularly glass containing lead. Sulphate of ammonia has precisely a similar action. As this salt upon being heated parts with a portion of its base, it may be considered as a salt with excess of acid. When heated in a glass vessel to the temperature of 316° Fahrenheit, it begins to melt: up to 600° Fahrenheit it does not suffer any further changes; at this temperature ammonia is driven off, sulphate and sulphite of ammonia sublime and the glass vessel is much corroded. The whole inner surface of the glass becomes dim, while the sulphuric acid combines with the potash, and probably the ammonia as it is driven off combines with the silicic acid. The glass generally flies to pieces and in the centre is much acted upon; the fragments are fused with difficulty, and are recognised by the blowpipe as sulphate of potash.

I have often further remarked that the watch-glasses (containing lead) which I am in the habit of using, to dry substances in vacuo over sulphuric acid, after from two to four weeks become covered with numerous flaws, and small splinters may be easily separated from them. I have not been able to detect any loss of weight, therefore the appearance cannot be due to the abstraction of any air contained in the glass, as Bischof, who observed something similar, surmises. I have never observed the same action to take place upon the glass of the air-pump or upon other glass. R. F. Manhand.—Poggendorff's *Annalen*, 1837, No. 12.

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